Migration and U.S. Labor Market Effects of the Mexican Drug War

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Abstract

We estimate the effect of the Mexican drug war on Mexico-to-U.S. migration and the resulting effects on population, employment and wages in U.S. labor markets. Our empirical strategy compares U.S. counties connected by the preexisting migration network to drug violence driven by close elections in Mexican municipalities in 2007 and 2008. Over the following decade, migrants fleeing the violence—the vast majority of whom are undocumented—cause native-born U.S. workers' employment rates to increase and unemployment rates to fall, while wages do not change. Employment gains are driven by less-educated natives. Business starts increase, suggesting higher profits for local firms.

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The Mexican drug war erupted in 2007 following the election of Felipe Calderon of the National Action Party (PAN) and his subsequent crackdown on several large drug cartels. Over the next four years, homicides in Mexico surged from about 10,000 to more than 25,000 each year (Figure 1). Since then, continuing drug violence has driven the migration of refugees out of Mexico and Central America (Clemens, 2021). The United States has been the largest receiver of these migrants, and as formal pathways to immigration remain constrained, the vast majority have come to the United States as undocumented immigrants.[1]

We use the outbreak of the Mexican drug war to bring new evidence on the effect of violence on migration and the effect of undocumented immigration on the modern U.S. economy. Despite intense interest in ongoing migration due to drug violence, as well as in the economic effects of these migrants on receiving communities, evidence is scant. Undocumented migration and the market for undocumented labor differ materially from documented migration and the market for documented immigrants and, by their nature, are difficult to study because of the lack of data. In particular, undocumented migrants face significant barriers to participation in the U.S. labor market and, consequently, they primarily provide labor in spot markets concentrated in a handful of sectors and occupations.[2] The transient nature of employment also reflects the lack of legal protection from wage theft and other abuses, as well as the fact that many undocumented Mexican migrants return to Mexico within a few years of arrival. Recent studies of short-term migration find that migrants do not compete with incumbent workers and that their employers possess a large degree of monopsony power (Naidu et al., 2016; Clemens and Lewis, 2024; Amuedo-Dorantes et al., 2024).

We overcome the challenges in studying undocumented migration by using newly available data linking Mexican-to-U.S. migration and a natural experiment that generates plausibly random variation for where in Mexico drug violence broke

[1]For example, the asylum data from the Transactional Records Access Clearinghouse show no meaningful increase in the asylum granted during the most relevant period between 2008 and 2012 (https://trac.syr.edu/phptools/immigration/asylum/).

[2]For example, the agriculture, house-cleaning, construction, and food-service sectors account for about 15 percent of the nation's total employment, but over 50 percent of likely-undocumented Mexican immigrants work in these sectors (see Table B.1).

out at the initiation of the drug war. Crucially for measuring migration linkages between Mexico and the U.S., our analysis leverages the universe of Matricular Consular de Alta Seguridad (MCAS) cards, an identification card issued by Mexican consulates and held by a large share of undocumented Mexican migrants. Using this data, we can form the network links between Mexican municipios and U.S. counties, allowing us to observe flows through the network, including card renewals as a proxy for length of stay in the United States.

The analysis proceeds in three steps. In the first step, we provide a new quasi-experimental estimation of the effect of violence on out-migration. Specifically, we estimate a regression discontinuity model that compares MCAS applications from Mexican municipios in which the mayoral candidate from Calderon's PAN narrowly won or lost.[3] There are no differential changes in out-migration between winning and losing municipios in the preelection period. Following the inaugurations of narrowly winning PAN mayors, migration rises to a peak in Years three and four relative to the municipios where PAN candidates narrowly lose. Long-run differences in out-migration persist more than a decade later. Our migration elasticity of 0.07 is slightly smaller than the estimates of Clemens (2021) for unaccompanied minor children. These estimates contribute to a growing literature on violence and migration, which has produced a range of estimates of the effect of drug violence on migration.[4]

The second step in the analysis links violence-driven out-migration to population movements in the United States. A well-known problem in the study of migration is the non-randomness of immigrant destinations, particularly in that immigrants disproportionately choose to move to strong labor markets. We overcome this issue by predicting Mexico-to-U.S. immigration from PAN-winning and PAN-

[3] Dell (2015) documents how homicide rates differentially rose in response to narrow PAN victories in these elections. The surge in violence following these elections suppressed local labor market activities and exports (Velásquez, 2020; Osuna Gómez, 2021; Gorrín et al., 2023).

[4] See Basu and Pearlman (2017), Orozco-Aleman and Gonzalez-Lozano (2018), Clemens (2021), and Leo et al. (2024) for estimates of drug violence and out-migration. Empirical evidence on publication bias suggests regression discontinuity evidence is of particular value compared to other quasi-experimental methodologies (Brodeur et al., 2020). Bohra-Mishra and Massey (2011), Melnikov et al. (2020), and Escamilla-Guerrero et al. (2023) also link migration and violence in Mexico; a large literature links violence and migration in other regions (Becker and Ferrara, 2019).

losing municipios using the MCAS data. We then compare population changes in U.S. counties—measured in U.S. survey data—based on the counties' network exposure to these predicted flows. The resulting increase in the Mexican-born population in networked U.S. counties is driven by likely-undocumented immigrants (with likely-undocumented status imputed in the American Community Survey (ACS) based on a combination of demographic and economic characteristics). The level and share of likely-undocumented Mexicans rises before leveling off three to four years after the elections. Using our extract of MCAS data, we can decompose flows in the U.S. into newly arriving migrants (those applying for MCAS cards for the first time) and pre-drug war migrants who lengthen their stays. We find that effects on populations in the U.S. are driven by a combination of increased immigration out of Mexico induced by the drug war, as well as longer stays by both new arrivals and pre-drug war migrants. We find no effect on the population of likely-documented Mexicans and no evidence of changes in native populations. Surprisingly, we find longer-run inflows of non-Mexican foreign-born individuals, a fact we return to in discussing labor market impacts.

The third and final step compares changes in U.S. labor markets based on their network ties to municipios with close mayoral elections.[5] We find that the arrival of Mexican drug war migrants causes local booms in native employment in receiving U.S. counties, with little-to-no evidence of wage changes. Over the decade following the 2007 and 2008 elections, the employment rate of native-born U.S. workers in the receiving counties increases, while unemployment rates fall. Our estimates suggest that 1 additional undocumented Mexican immigrants creates 0.8 native jobs over the next five years. The implied elasticity of native employment with respect to the local share of undocumented Mexicans is 0.06. Native wages show little evidence of a change, and confidence intervals exclude meaningful decreases. The modern literature on the economics of immigration emphasizes the importance of skill differences between domestic workers and new arrivals in determining the overall economic impacts (Card, 2005; Ottaviano and Peri, 2012; Llull, 2017). Skill

[5] An alternative empirical approach using all drug war homicides produces similar conclusions to the election-based identification, though there are more underlying differences between U.S. counties with strong and weak network ties to the drug war.

differences are particularly acute for undocumented workers, as most unauthorized migrants do not speak English fluently if at all, while high school graduation rates for U.S.-born individuals aged 25 to 34 exceeded 90 percent by 2010. When we disaggregate our estimates on native employment by education level, we find similar increases in employment among high school dropouts, high school gradates, and those with some college, and no effect on those with a bachelor's degree or more; there is minimal evidence of an effect on wages in all subgroups.

In the longer run, the native employment boom ends, and populations and labor markets in U.S. counties reach a new equilibrium. Over the decade following the close elections, undocumented immigrant arrivals continue at a higher level in counties with stronger network ties to the PAN-winning municipios, consistent with changes in the local economy and growing enclaves. Despite the continued presence of additional migrants, the effects on employment and unemployment rates of natives dissipate, indicating that the unexpected character of migration plays an important role in the medium-run surge in labor demand. We view these dynamics as a key feature of the analysis, since many shocks that increase undocumented immigration would be expected to generate a similar set of dynamic responses in population. We conclude that the long-run economic effects of undocumented immigration on local employment and wages are modest.

Following the main results on migration and labor market effects, we present evidence on potential mechanisms and connections to the theoretical literature on migration. Three explanations appear to account for the findings. One, the arrival of immigrants boosts labor demand. This is due to the increased demand for goods and services from the immigrants themselves, and the concentration of the immigrant employment in particular sectors. Native employment gains occur in the sectors with low employment shares of likely undocumented workers. Second, newly-arriving immigrants complement incumbent workers due to both differences in skills and legal barriers to working in jobs held by incumbents. The differences between the immigrants and natives is sufficiently large that we find no evidence of substitution (in the form of declining employment or wages) even in the sectors in which immigrants concentrate. Similarly, positive employment effects are concentrated among workers with less than a bachelor's degree, the group most likely to

compete with new arrivals. And, third, the immigrants increase local firms' profits because of their limited labor market opportunities and resulting employer monopsony power. Consistent with a model of monopsony and rising firm profits, we find an increase in new business starts in U.S. counties that receive drug war migrants.[6]

The paper makes several contributions to the literature. Most notably, it provides novel evidence on undocumented migration to the literature debating the effects of the large increase in immigration to the United States that began in the second half of the twentieth century, summarized in Dustmann et al. (2016) and Mackie and Blau (2017). Undocumented migration is difficult to measure, leaving the questions addressed in this paper among the most important open areas in this literature. Much of the evidence on the impact of immigration in the United States relies on the Decennial Census, which is conducted every 10 years and hence would not capture the dynamic effects we document here. The lack of credible natural experiments to study immigration has also constrained the literature, and our paper adds a new push factor to the small set of studies that focus on exogenous variation in arrivals (e.g. Card, 1990; Hunt, 1992; Dustmann et al., 2017; Monras, 2020; Peri et al., 2022). Two related studies use natural experiments to examine the effect of undocumented immigration in the U.S. after the year 2000. Allen et al. (2019) use the new construction of border walls on the U.S.-Mexico border. This natural experiment increases the costs of migration, and a calibrated spatial equilibrium model finds modest wage declines among low-wage natives.[7] East et al. (2023) study the expansion of the Secure Communities program, a large deportation program partnership between the Federal Bureau of Investigation (FBI) and Immigration and Customs Enforcement (ICE). The findings in East et al. (2023) indicate that the combination of deportations and deportation risks reduce the employment of nonimmigrants, echoing our findings indicating that arrivals increase the employment of nonimmigrants.[8] Alternative empirical strategies (such as Caiumi and

[6] We do not have information on who started these businesses, but Azoulay et al. (2022) and other show that immigrants are likely to be entrepreneurs.

[7] Feigenberg (2020) reports on the direct effets of border wall construction on migration and the selection of migrants.

[8] Our study reaches some distinct conclusions and makes several contributions that were not possible in this previous work. One reason for differences in our conclusions is that the nature of the change in immigrant population is conceptually distinct, in that the drug war increases *intended*

Peri (2024)) and historical evidence also find that low-skill immigration positively affects native employment (as in Long et al. (2024)). Related work examines the effects on immigrant integration and political outcomes (Zaiour, 2023; Tiburcio and Camarena, 2023).

The paper also contributes to the growing literature on migration networks, which documents the linkages in economic and social outcomes between Mexico and the United States (e.g., Caballero et al., 2023; Osuna Gomez and Medina-Cortina, 2023). Finally, the paper contributes to the long literature on the economic adjustments to population shocks. Labor market adjustment to migration and labor supply shocks have long been of interest as a determinant of regional macroeconomic dynamics (e.g., Blanchard and Katz, 1992; Kline, 2008; Amior and Manning, 2018; Yagan, 2019). Our findings both provide new evidence on the dynamics of adjustment over more than a decade following the shock, as well as emphasizing the interaction of migrants' characteristics and labor market structure in mediating the response of wages and employment to an exogenous increase in population. The rest of the paper is organized as follows. Section 1 describes our MCAS data, the network construction and U.S. labor market data. Section 2 lays out our empirical strategies for both the migration effects of drug war homicides and the effects on U.S. populations and labor markets. Section 3 provides our main results. Section 4 address the mechanisms behind the labor market effects. Section 5 concludes the paper.

1 Data

1.1 Matricula Consular de Alta Seguridad Program (MCAS)

Data constraints limit undocumented migration research in the United States. Undocumented immigrant stocks and flows are commonly approximated using method-

arrivals of mostly undocumented immigrants, while Secure Communities increases *unplanned removals* of undocumented individuals who have a law enforcement interaction. The population effects of these changes may also differ because of the responses of families and future migrants (Medina-Cortina, 2023). The longer time span of our study also helps us examine dynamics, which show that employment responses to changes in the undocumented population fade out.

ologies that combine survey and administrative data. Each has advantages and disadvantages that restrict the insights generated. Even when stocks or flows are available, data rarely cover migrants' origins beyond the country or region. The restricted-access MCAS records we use offer significant improvements over previously available data because of their refined geographic information.

Mexican consulates in the U.S. issue Matricula ID cards to Mexican citizens living in the United States.[9] To obtain the card, an applicant must schedule an appointment, pay an application fee, and be interviewed at the consulate corresponding to his or her U.S. residency. Although proof of residency in the United States is required, no proof of legal status is requested, making it a popular source of identification for undocumented immigrants who lack alternative means of identification. The card is valid for up to five years and can be renewed if it expires, is lost or damaged, or the holder moves. Recent uses of these data have provided important contributions to migration research.[10] Our extract comprises the universe of all cards ever issued from 2004 to 2019.

Two essential features of these data make our analysis possible. First, the data allow us to create static municipio-to-county links between two countries, which we use to define networks and predict inflows into different U.S. counties. We define our measurement for the connection as the share of municipio m's migrants in county c in 2005, two years before the elections:

$$S_{m,c,2005} = \frac{n_{m,c,2005}}{N_{m,2005}}, \tag{1}$$

where $n_{m,c,2005}$ corresponds to the number of newly arrived immigrants from municipio m in each U.S. county c in 2005, and $N_{m,2005}$ indicates the total number of newly arrived immigrants from municipio m.

Second, the data include personal identifiers that allow us to distinguish newly

[9] The card is a valid identification form in Tucson, Los Angeles, Dallas, Houston, Phoenix, Denver, San Antonio, and Chicago. It can be used to apply for a driver's license in 13 states: California, Colorado, Connecticut, District of Columbia, Delaware, Hawaii, Illinois, Maryland, New Mexico, Nevada, and Utah. Other uses include school registration, acquiring a U.S. birth certificate for the children of migrants, registering for business licenses, opening bank accounts, cashing checks, and sending money abroad.

[10] See, for example, Allen et al. (2019), Bhandari et al. (2021), and Caballero et al. (2023).

arrived undocumented migrants from card renewals. This distinction is important when we estimate the migration elasticity with respect to homicides in Mexico, where the goal is to capture new migrants fleeing violence. Conceptually, the violence-driven stayers who need to renew their card could also be included when estimating migration elasticity, but because Matricula cards are valid for five years once issued, people who choose not to return because of violence may not need to re-apply for the cards. The timing of the renewal may not completely line up with the surge in homicides. For completeness, in the analysis below, we estimate new applications and renewals separately. To better estimate new migrants, we also try to exclude those who have previously resided in the United States but applied for a card and experienced significant delays: we focus on applicants aged 18 to 35 years old, the prime ages for first-time travel to the United States (Camarota and Zeigler, 2019). This measure, however, could still capture some previous migrants who prolong their stay in the United States and thus apply for a card in response to the violence in Mexico.

Crucially, the MCAS data are used to generate a flow measure of undocumented migration that, while imperfect, offers significant improvements over previously-available data. The card is available to all Mexicans, but it is presumed that holders are undocumented immigrants, because the card adds no additional benefits to those that are provided by a valid passport or visa (Massey et al., 2010). Additionally, long wait times for authorized migration to the United States from Mexico, which can stretch a decade for individuals who are not married to a U.S. citizen or permanent resident, make it unlikely for changes in MCAS applications to reflect authorized immigration.

The MCAS data are not perfect. Most importantly, not all undocumented Mexican immigrants apply for the card, and those who do may not do so upon arrival.[11] However, we expect these data limitations to have a small impact on our analysis for

[11] Unfortunately, no information is available on how long new immigrants take to apply for a card. The only data available comes from a survey of 4,836 people conducted in Mexican Consulates (Pew Hispanic Center's Mexican Migrant Worker Survey 2005). The survey asks how long each applicant has been in the United States before applying, but it does not indicate whether the card is being renewed or issued for the first time. This survey indicates that at least 40 percent of the surveyed applicants obtain the card within the first two years of arriving.

two reasons. First, our main analysis relies on the pre-election network constructed using the MCAS data, meaning that we do not depend on yearly flows for our main conclusion. Caballero et al. (2018) evaluate the quality of the MCAS aggregated data and the implied network, showing that the MCAS and the ACS data reveal very similar distribution of Mexicans across U.S. states and that the MCAS and the Mexican Census also demonstrate similar migrant patterns across Mexican municipios. They thus conclude MCAS can be used to measure the network of municipo-to-county connections. Similarly, Allen et al. (2019) show that the MCAS data is highly predictive of the variation of the migrants across core-based statistical areas (CBSAs). Second, as we will show later, we complement the MCAS data with estimates of likely undocumented Mexican immigrants using ACS data. Both measures show similar effects of the violence on undocumented immigrant populations. Therefore, we argue that at the aggregate level, any selection issue due to the details of the card applications has limited impact on our analysis.

1.2 The American Community Survey (ACS)

The U.S. labor market data come from the publicly available ACS household survey from 2005 to 2019 and the 2000 5-percent sample from IPUMS-USA (Ruggles et al., 2023). The ACS is representative at national and subnational levels, providing population counts and annual labor market data. We aggregate the data at the county level and restrict the sample to those between 18 and 35 years of age who are not in school or living in group quarters at the time of the survey.[12] The one-year files include only counties with at least 65,000 residents, which means that we observe 376 counties between 2005 and 2011, and 430 counties between 2012 and 2019. We then restrict the sample to counties that consistently present in the data between 2005 and 2019. We further drop the counties with less than 1 percent of Mexicans in the population in 2000 and the counties for which we do not observe the key labor market outcomes for one of the key subpopulation groups between

[12]The choice of the age range is based on the prime age range of the migrants discussed above. We find similar results in an age 18-to-64 sample.

2005 and 2019.[13] These two restrictions leave us with 120 counties that account for 73.2 percent of total Mexican population in 2000 and 69.4 percent of new card applications in 2004.

These data are used to construct estimates for county-level population, employment, and wages by demographic groups. We also estimate the likely undocumented Mexican population using the Borjas and Cassidy (2019) "likely undocumented" imputation algorithm.[14] The ACS data provide a measure for the stock of likely undocumented Mexican immigrants, which includes new migrants from Mexico, stayers, and possibly migrants from other counties. Table B.2 provides summary statistics for these counties in preelection years.

1.3 County Business Patterns (CBP) and Quarterly Workforce Indicators (QWI)

In addition to the ACS, we use data collected from the firm side: the CBP and the QWI. These data sources provide more comprehensive county-level employment and business counts than the ACS does, and are thus useful when industry-level impacts are of interest. Table B.1 shows that construction and eating and drinking places are the two industries with the most undocumented Mexican immigrant workers, accounting for 38 percent of total undocumented Mexican immigrants workers. These two industries will be our main focus. Agriculture is another important industry, but neither the CBP nor the QWI provides good coverage, so it is excluded from the analysis. Going down the list, landscaping and services to dwellings and other buildings together account for 8 percent of total employment among undocumented Mexican immigrants. These job categories belong to the same four-digit NAICS code (5617), so we include NAICS 5617 in the analysis

[13] The key variables and subpopulations we take into account are listed in Table B.2. The first restriction excludes small counties that can be outliers. The second restriction ensures that our sample remains stable when extending the analysis to other labor market outcome variables.

[14] An immigrant is classified as likely documented if the person 1) arrived before 1980, 2) is a citizen, 3) received social benefits, 4) is a veteran or is in the Arm Forces, 5) is in the government sector, 6) resides in public housing or receives rental subsidies, 7) is born in Cuba, 8) is in an occupation that requires licensing, or 9) has a spouse who is a legal immigrant or citizen. If an immigrant does not meet any of these criteria, the person is classified as likely undocumented.

too.

Both data sets provide annual data from 2000 to 2019. In the CBP, we observe county-level total number of payroll employees, number of establishments, and number of establishments by establishment size. Compared to the CBP, the QWI provides more detailed information on employment dynamics such as new hires and new separations. From the QWI, we use variables representing new hires, new separations, beginning-of-quarter employment, and earnings of workers who stayed with an employer for the full quarter. Because these are quarterly data, we average across quarters in a year to conduct the analysis at the annual level.

A crucial characteristic of these two data sets is the reliance on administrative unemployment insurance records in their construction. Thus, workers in these data participate in the formal sector. Undocumented workers can access the formal sector with fraudulent documents or, over time, by changes in their legal status. We are not aware of an estimate of the share of undocumented workers who work in the formal sector. For this reason, the ACS likely represents a more complete, albeit survey-based, picture of the employment status of the undocumented population.

2 Empirical Strategy

Since the start of the drug war in 2007, homicides in Mexico have increased from about 10,000 a year to more than 25,000 (Figure 1). At the municipio level, the increases in homicides range from zero to over 500 percent, with a median increase of 208 percent.[15] We first explore the correlation between the municipio-level increases in violence and municipio-level increases in new card applications. We construct two cumulative variables in which each year's new card applications and homicides are the sum of all the cards and homicides since 2007, the year right before the spike of the homicides. Then, for each year between 2008 and 2019, we regress the change in the log of new card applications on the change in the log of homicides from 2007 to the year of interest:

$$\Delta Log(Newcards_{mt}) = \beta_0 + \beta_1 \Delta Log(Homicides_{mt}) + \varepsilon_{mt}, \tag{2}$$

[15] Authors' calculation based on the homicides data.

where m indexes municipios, and t indexes years. Each year is a separate regression. The term β_0 represents a constant term, and ε_{mt} is the error term. Figure A.1 plots the estimates for β_1 in each of the regressions. These estimates represent elasticities of outmigration with respect to homicides. For example, in 2012, 0.067, with a standard error of 0.023, suggests that a 1 percent increase in homicides leads to a 0.067 percent increase in new card applications. This correlation provides supportive evidence that the Drug-War-related violence has led to outmigration to the United States. The concern of using just this correlation is that the labor demand in the United States could affect Mexican municipio-level economic conditions through the network, which in turn could affect both out-migration and violence. To separate out supply-side shock, we use the mayoral election results in 2007 and 2008 combined with the violence.

2.1 Regression Discontinuity: Violence and Out-Migration

Dell (2015) shows that homicides increase more in the municipios where PAN candidates narrowly won mayoral elections than in the municipios where they narrowly lost, using a regression discontinuity (RD) design. The advantage of an RD design is that among the municipios where the races were close, the likelihood of whether a PAN candidate won the election is as good as random. This randomness leads to the similarity in municipio characteristics across but close to the threshold, and the differences post inauguration can be attributed to the elections and resulting violence. We first extend the estimation in Dell (2015) to investigate how the number of homicides changes over time after inauguration.

In Figure B.1, we break down the RD analysis by calendar year relative to the year of the election (where $t = 0$).[16] The results suggest that the increase in violence happens almost immediately after the inaugurations, and that homicide rates remained to be elevated three years after the inaugurations. We then link the outflows from Mexico to the inflows into the United States, using the network shares

[16]Note that we aggregated the monthly data in Dell (2015) to be at calendar-year level to be consistent with the Matricula data that are at the annual level. We define a year to be "post inauguration" if at least six months are post inauguration in that year. In that paper, the author scales the monthly homicide variable up to a year (that is, she uses monthly homicide cases $\times$ 12 months as the outcome variable), so our estimates are directly comparable to hers.

$S_{m,c,2005}$ to calculate county c's predicted new card applications in year t:

$$CNC_{ct} \equiv CountyNewCard_{ct} \equiv \Sigma_{m \in MC}(MunicipioNewCard_{mt} \times S_{m,c,2005}).$$

In the equation, the set MC collects all the municipios that are sending municipios for county c. Similarly, we calculate network-predicted homicides, which reflect the extent to which a county is exposed to the increase in homicides.

2.2 Identifying the Inflows of Undocumented Immigrants and Impact on the U.S. Labor Market

Following the idea of the RD design, we restrict our analysis to the municipios where the PAN marginally won or marginally lost, that is, the ones where the win or loss is within the margins of 0.5 percent.[17] As discussed above, we use the network share in 2005, $S_{m,c,2005}$, to predict the number of new card applications in each county associated with these "marginal" municipios, and we interact the dummy variables indicating marginally winning and marginally losing municipios with the number of new card applications to predict two sets of the number of new cards for each county. For example, the predicted county c's new card in year t from all marginally winning sending municipios is

$$CNC_{ct}^{Win} \equiv CountyNewCard_{ct}^{Win} \equiv \Sigma_{m \in MC}(MunicipioNewCard_{mt} \times Dummy_{m}^{Win} \times S_{m,c,2005}).$$

The new card for marginally losing counties, CNC_{ct}^{Lose}, is defined in the same way.

To identify the impact of this inflow on the U.S. labor market, we utilize the variation in the predicted inflows across counties and estimate a set of two-way fixed-effects models. We first interact municipio-level post-inauguration dummies to predict county-level new cards post inaugurations:

$$CNC_{ct}^{WinXPost} = \Sigma_{m \in MC}(MunicipioNewCard_{mt} \times Dummy_{m}^{Win} \times S_{m,c,2005} \times Post_{mt}).$$

[17] Although the identification strategy only compares observations on either side of the threshold, it is comforting that the visual relationship between PAN vote share and out-migration closely resembles the relationship between vote shares and homicides away from the threshold as well.

A concern of this type of shift-share predictors is that even if the assignment of the immigrants across different counties is as good as random, the unobserved characteristics of some counties can lead these counties to systematically attract more undocumented Mexican immigrants than others. For example, we may be concerned that complex dependencies in the network relate the arrivals of immigrants in receiving counties, or that the results are sensitive to the realized election results of the largest muncipios near the threshold. To address these concerns, we use the randomization inference method of Borusyak and Hull (2020) to demean the predicted county-level cards. We first randomize the election results among the marginally winning and marginally losing municipios using the actual election-winning rate and calculate the predicted new cards for each of the counties using the randomized election results. We run the randomization for 1,000 times and calculate the average predicted new cards corresponding to marginally winning and marginally losing municipios for each relevant county. These two sets of the average predicted number of new cards capture the heterogeneity in new card applications driven by unobserved county characteristics. Finally, we subtract the two averages from the actual predicted new cards to construct the demeaned predicted new cards.

Then we construct $RelNewCard_{ct}^{post}$ to be the difference between the inverse hyperbolic sine (IHS) transformed and BH-method demeaned predicted new card applications from the PANwin ($CNC_{ct}^{WinXPost}$) and the PANlose ($CNC_{ct}^{LoseXPost}$) municipios post inauguration.[18] This variable captures the predicted relative increase in new cards in marginally winning municipios with respect to the marginally losing municipios. We then run a set of regressions in the following form:

$$Log(y_{ct}) = \alpha_0 + \alpha_1 RelNewCard_{ct}^{post} + \delta_c + \gamma_t + \mu_{ct}. \tag{3}$$

The dependent variable $Log(y_{ct})$ is the log of the county population for a selection of demographic groups of interest.[19] Each regression includes one outcome variable

[18]The IHS transformation, as opposed to log transformation, is necessary because the demeaned number of cards can be either positive or negative.

[19]Because the ACS includes only large counties, not many counties have zero values for the variables that we are interested in. We thus use log transformation, as opposed to IHS transformation,

and two years of data. We use 2006, the year before the first election, as the pre-period, and one post inauguration year, between 2008 and 2019, as the post period. We use only one post period so that the estimates for α_1 will not be confounded by heterogeneous and dynamic treatment effects (Callaway and Sant'Anna, 2021). To test for pre-trends, we also estimate whether the relative change in cards predicts the change in the outcome variables in 2000, 2005, and 2007 relative to 2006. In these regressions, we construct a hypothetical relative change in cards using 2006 and 2012 data and use that as the key independent variable to "predict" the responses in the pre-periods.[20] County fixed effects (δ_c) and year fixed effects (γ_t) are included in the regressions. We use the Borusyak and Hull (2020) method to simulate how election outcomes influence the distribution of potential flows through the network. Standard errors, μ_{ct}, are clustered by county.

3 Main Results

3.1 The Mexican Drug War and Migration to the United States

We begin by documenting the patterns of migration from Mexican municipios to the United States in which a close election occurred. Figure 2 reports the regression discontinuity results describing the effect of narrow PAN victories in Mexican municipios on applications for Matricula Consular identification cards in the United States. The running variable is percentage change in municipio-level new card applications, for which the changes are calculated relative to the year before the elections, which we label as $t = -1$. The outcome measures out-migration from Mexican municipios and does not condition on the destination county in the United States.

Figure 2 shows that Matricula Consular card applications increased markedly in the years following the a narrow PAN electoral victory. Trends in out-migration from PAN-winning municipios are similar to trends in out-migration from PAN-

for its nice properties. For some of the variables, however, number of observations drops when the outcome variable is zero.

[20]This variable equals 0 in 2006 and equals the relative change in cards from 2006 to 2012 for the pre-periods.

losing municipios in the years before the elections. Card applications remain similar between winning and losing municipios in the year of the elections and the following year, before beginning a sustained relative increase in the second year among marginally winning municipios following the elections. Out-migration measured by card applications is strongest in the third and fourth year following the elections. Point estimates, in the long-run, display a small but consistent relative increase in migration from PAN-winning municipios to the U.S., that lasts through the entire sample period.

Figure B.2 plots the RD estimates for homicides and new card applications. Summing up the estimates between Period 1 and Period 9, the estimated elasticity of migration with respect to homicides is 0.07. This elasticity is similar to the ones estimated for unaccompanied minors in Clemens (2021) of around 0.2 to 0.3.[21] It is worth noting that the very large increase in homicides may complicate the interpretation of the elasticity and comparison to other studies.

Some institutional detail may influence the interpretation of patterns in the data. For example, immigrants may not apply for a card immediately upon arrival. The lag between the election and card applications likely reflects this delay, in addition to the increase in violence over the three years following the election. Similarly, not all undocumented immigrants will apply for a card. In general, card applications reflect a stronger commitment to remain in the U.S. Thus, the new card applications primarily represent the migration of undocumented immigrants, but they also reflect an increase in expected length of stay in the United States.

3.2 U.S. Population Effects of the Mexican Drug War

We next turn to the effect of the onset of the Mexican drug war on undocumented immigration to the United States and other responses in the U.S. populations. Figure B.3 plots the trends for the sum of predicted new cards across counties, that is, $\Sigma_c CNC_{ct}^{Win}$ and $\Sigma_c CNC_{ct}^{Lose}$ for all relevant receiving counties in the United States. We normalize the numbers relative to the year 2007—the year before any inaugura-

[21] Orozco-Aleman and Gonzalez-Lozano (2018) use an instrumental-variables model based on electoral cycles in Mexico to calculate an elasticity of out-migration with respect to homicides of 1.38, noting heterogeneity by region and exposure to transit violence.

tion took place. This figure shows that since 2007, the aggregate inflow from both sets of municipios decreases over time, but the predicted inflow from the marginally winning municipios declined less than that of the marginally losing municipios. The gap gradually starts to increase post-inauguration, and the increase persists in the following years. The gap prior to the inaugurations is small, and the two lines follow parallel trends. Both observations suggest that the inflows from the marginally winning and marginally losing municipios are similar prior to the inauguration. The parallel trends provide support for the validity of the RD design. All of these observations are consistent with the out-migration results on the Mexican side.

Figure 3 reports the effects in percentage terms relative to 2006.[22] Panel (a) displays the effect of a network connections to PAN-winning municipios on cumulative new card applications in a U.S. county after the mayoral elections. New card applications, measured for newly arrived undocumented immigrants, show no association with the election outcomes in 2004–2007 before an increase in applications begins in 2008. The increase in new card applications sustains and grows over the years after 2008. Inflows of new undocumented immigrants continue and remain higher in these counties through the end of the sample in 2019. The estimates can be interpreted as a one-standard-deviation increase (6.7 in 2012) in the relative predicted number of new cards, which increases the county-level new cards by about 6.7 percent, or 915 new card applications. The 6.7-unit increase in the relative predicted number of new cards in 2012 ($RelNewCard_{ct}^{post}$) corresponds to approximately a 93 percent relative increase in card applications if we use the variables that are not demeaned by the BH method.[23]

Panel (b) shows the corresponding flows—as opposed to cumulative flows in Panel (a)—of new card applications, and Panel (c) shows flows of card renewals. Card renewals capture those who decide to continue to stay in the United States. The results suggest that the surge in the Drug-War-related violence not only induces more people to migrate out, but it also leads people who have already moved to the

[22] We discuss their interpretations in the text in relation to one another, and summary statistics appear in Tables B.2 and B.3.

[23] Note that because the key independent variable $RelNewCard_{ct}^{post}$ is based on demeaned value, the percentage change relative to its sample mean does not provide the usual interpretation. We use a standard-deviation change as a reference point.

United States to stay longer. The increase persists for over 15 years. In addition, as we expected, the increase in renewals is not as sharp and sudden as the new applications, because not all stayers need to renew their cards.

We next turn from the flow of the new undocumented immigrants to measurement of the stock of likely undocumented Mexican immigrants in the United States. Panel (d) reports the effect of a U.S. county's network connections to winning PAN municipios on the size of the likely undocumented Mexican population in that county. The outcomes can also be constructed in the 2000 Decennial Census, providing an additional check on the quality of the comparison generated by the empirical strategy. The point estimates on 2000 and 2005 demonstrate that counties connected to the drug violence show no evidence of a differential trend in their pre-election undocumented populations. The response of the stock of likely undocumented Mexican immigrants naturally follows the flow, increasing sharply after 2008 and reaching a steadily larger and larger undocumented Mexican population over the next decade. Generally, the estimates suggest that a one-standard-deviation increase (equals to 6.7 in 2012) in the relative predicted number of new cards increases the undocumented population by 13.4 percent, or about 1,791 people (based on the summary statistics in Table B.3). This increase in undocumented Mexican population corresponds to 1 percent of the native population, and 10 percent of a county's native high school dropouts in an average county.

The correspondence of the Matricula Consular card application and the size of the local undocumented Mexican population gives a strong indication of the immigration response to the onset of the Mexican Drug War. Also note that the increase in likely undocumented Mexican population is greater than the number of new card applications, suggesting a permanent strengthening of migration ties between these U.S. counties and Mexico due to the quasi-random outcome of the close mayoral elections. The initial influx of immigrants from the municipios with close elections is then followed by inflows from other municipios, a pattern that is consistent with a growing Mexican enclave attracting a broader set of Mexican immigrants in the later years. Finally, Panel (e) shows the results in which the dependent variable is the share of likely undocumented Mexican immigrants with respect to the 2000 county population, and the patterns are consistent with the ones

estimated using undocumented Mexican population.

Figure B.4 depicts the response of other aspects of the U.S. population to the increase in undocumented immigration and population. Across these panels, we find no meaningful pre-trends in population growth for any group. Panel (a) suggests that growth of the Mexican-born population who are either documented or U.S. citizens shows little or no response. This is unsurprising because during the period of the Mexican Drug War, there is no meaningful expansion of the immigration program. Panels (b) through (e) depict, respectively, the effect on the documented and undocumented non-Mexican foreign-born population, native-born population, and native-born high school dropouts. Native populations show a downward trend beginning five years after the onset of the drug war, but point estimates are small and not statistically significant at the 95 percent level. The non-Mexican foreign-born population increases, albeit with a lag relative to the growth of the undocumented Mexican population. Finally, the native high-school-dropout population shows minimal evidence of changing, having only small, negative point estimates. We single out native high school dropouts because they are likely the most affected group when inflows of undocumented Mexican immigrants increase.

Table 1 shows selective summary estimates for which we pool together multiple years. We report the results for Mexican-born and non-Mexican foreign-born by pooling together likely undocumented and documented. We separately estimate the effects in the medium (about 5 years) and long runs (about 10 years). The results are consistent with what we observe in the figures, in that the impact is persistent and the long-run effect tends to be larger in magnitude.

Overall, the evidence on the effect of the arrival of undocumented Mexican immigrants on local U.S. populations indicates that the primary response is to permanently increase the size of the local immigrant populations and the future arrival rate of immigrants. Low-skilled native populations show small-to-no response. In addition to these results identified through municipios with close election results, we also document the results for which we exploit the full variation in municipio homicides, discussed above. Figure A.2 shows the estimated correlation between the changes in the network-predicted homicides and the changes in network-predicted new card applications at the county level. Over time, the counties with the largest

increases in exposure to homicides in Mexico through the network experienced a larger increase in new card applications than other, less exposed counties. The correlation between the two variables, however, is not linear, making it hard to utilize the full variation. As an alternative, we compared the counties that experienced the largest increases in exposure to homicides (the top 10 percentiles) with the rest of the counties, using the method in Callaway and Sant'Anna (2021). We report the relevant results in the Appendix A.[24]

3.3 U.S. Labor Market Effects of the Mexican Drug War

The evidence on migration and population flows indicates that the onset of the Mexican Drug War induced a response in migration that increased the size of local immigrant populations and future immigrant arrivals. We now turn to the response of the U.S. labor market to this shock. If the inflow of undocumented Mexican immigrants enters the labor force, they are most likely going to go into the industries that intensively hire these workers. Panels (a) through (c) of Figure 4 provide supportive evidence for this hypothesis: payroll employees in the three key undocumented-Mexican-worker-intensive industries increase in response to the Mexican Drug War.[25] The results are estimated using the data from the CBP. In Panels (d) through (g), we pool together the employment in these three industries, but we break the employment down by demographic groups using the data from the ACS. The results indicate that the increase in employment in these industries isdriven by the increase in the employment of the undocumented Mexican immigrants. Interestingly, both data sets from different sources show a positive employment effect, indicating that the firm-side employment data also pick up the employment of the undocumented Mexican immigrants. Panels (h)–(k) show that while employment increases, the wage effects are limited. If anything, the only

[24] Figure A.3 shows key population results. While we observe an increase in new card applications and elevated undocumented Mexican populations, the pre-treatment trends suggest that the most affected counties could be different from the less affected ones.

[25] Figure B.5 shows more results on earnings per hour and number of establishments by detailed industries. We find some evidence of small business entry in the food service and drinking places. Business creations could be one factor that explains null-to-positive employment effect. Figure B.6 shows that hires and separations increased in all these industries.

group that is negatively affected is the non-Mexican foreign-born, though the estimates are not statistically significant at the 95 percent level.

Figure 5 shows the results on county-level employment rates, wages, and unemployment rates for three key demographic groups. As with the population outcomes taken from the ACS, we have a set of estimates based on the 2000 Decennial Census to check for pre-election differences in labor market trends in these variables. Across the nine subfigures, the pre-election trends show an expected level of noise in their relationship to the quasi-experiment. It appears, if anything, that U.S. counties exposed to the quasi-random increase in undocumented immigration had mildly deteriorating labor market conditions in the five years before the elections.

Panels (a) through (c) report, respectively, the effect of the immigration shock on the employment rate of the Mexican-born, the non-Mexican foreign-born, and the native-born. The foreign-born populations, both Mexican-born and non-Mexican-born, show no effect on their employment rates. Across the panels, only the native-born population in Panel (c) shows an increase in the employment rate. The effect on native-born employment peaks in 2012 and falls thereafter, despite the evidence on population indicating that these counties maintain larger immigrant communities and an increase in immigration arrivals through the end of the sample.

In Figure 6 we report the employment results on natives by education groups. Reading across the panels, it is important to note that the scale differs. The point estimates for native-born high school dropouts, high school graduates, and people with some college are all positive, and resemble the overall effect on natives but the estimates are noisy. It also appears that native high school dropouts experience the largest increase in employment rate. The employment rate of those who have a bachelor's degree is not affected.

Moving to Panels (d) through (f) in Figure 5, the results for wage rates show that the inflows of immigrants, despite their impact on employment rate, have minimal impact on wages. Wages of Mexican-born workers remain unchanged, as seen in Panel (d). Native wage rates in Panel (f) show minimal evidence of an increase to correspond to the rising employment rate in Panel (c). The presence of a significant negative coefficient in the 2005 native wage followed by a reversal in 2006 lacks a clear interpretation, making it difficult to judge whether the pre-election movements

in the native wage influence the results. Results in Figure 6 suggest that the effects on native-born non-college graduates shows a similar pattern to all native-born, though they are more noisily estimated. We can rule out large negative effects on the wages of high school dropouts. Finally, we do not detect a statistically significant negative impact on non-Mexican foreign born, as shown in panel (f).

Panels (g) through (i) of Figure 5 report effects on the unemployment rates of each of the three groups. Consistent with the employment effects, unemployment rates of the native born have declined since the onset of the Drug War. All three groups show either no effect or negative effects on unemployment rates. This evidence is consistent with the arrival of immigrants serving as a shock to labor demand.

Finally, Tables 2 summarize the medium- and long-run labor market effects depicted in the figures.[26] The results show that the Drug-War-related inflows of undocumented Mexican immigrants increases the overall employment rate among native workers in the medium run without diminishing employment in undocumented-Mexican-worker-intensive industries or wages within those industries or across the economy as a whole. In the first four columns of Panel A, we report the effects on employed persons in the most affected industries. As expected, employment of likely undocumented Mexicans rises, as does the employment of Mexican-born individuals overall. Interestingly, we also find evidence of increasing employment of non-Mexican foreign-born workers. This is consistent with the Drug War shock spurring in-migration of other immigrants, due either to increased job creation or changes in migration networks and Spanish-speaking enclaves. In the first four columns of Panel B, the effects on wages in these industries are all indistinguishable from zero. Thus, the local economies appear to have absorbed the influx of migrants without changes to employment of native workers in the most affected sectors, or a change in wages for any group.

The first four columns of Panels C and D contain estimates for the broader economy. In Panel C, we report the employment rate. Except for the estimates for natives, the results are statistically indistinguishable from zero. The native employment coefficient of 0.0027 and the undocumented Mexican population estimate of

[26]The corresponding results on natives by education groups are reported in Figure B.4.

0.0162 imply that the elasticity the native employment with respect to likely undocumented Mexican population is 0.17. In other words, a 1 percent increase in the immigrant population increases medium-run native employment by 0.17 percent. When we evaluate this response at the mean, it implies that 0.86 native jobs are created for the arrival of 1 likely undocumented Mexican.[27] The elasticity of the native employment rate with respect to undocumented-Mexican-to-2000-population ratio is about 0.1.[28] The employment effect returned to zero in the long run. We find no effect on county-wide wages for any group.[29]

Columns 5 through 8 in Table 2 show the estimates for the longer run, from 2016 to 2019, approximately a decade after the start of the Mexican Drug War and after the U.S.-side population effects on the U.S. side have stabilized. In Panel A, we see that employment of likely undocumented Mexicans and all Mexican-born individuals in the most affected industries has risen from the medium run. There are, however, no effects on the employment or wages of any other group in these industries. Looking at the economy as a whole, we see no effects on employment rates or wages, save for a negative effect on the wages of non-Mexican foreign–born workers. Since this group also sees population growth, it is difficult to say whether the negative effect arises from competition with undocumented Mexicans or from composition effects.[30]

3.4 The Great Recession and Results Using All Homicides

A concern with estimating the impact of the Mexican Drug War is that its timing overlaps with the 2007 Great Recession. To understand the role of the Great Reces-

[27] Based on the summary statistics numbers in Table B.3, the change in employment of natives equals 0.686×0.17 percent$\times 115098.6 = 115.7$, and the change in undocumented Mexican immigrants equals $13369.19 \times 13369.19 = 133.69$.

[28] This number is calculated by dividing 0.0027 by the medium-run estimate in Figure 3 penel (e), 0.002, scaled by mean of 0.07.

[29] In the Appendix, we report heterogeneity in the effect by gender, finding no difference in the employment effects for men or women. The lack of important heterogeneity by gender differs from the effects of undocumented deportations analyzed in East et al. (2023). We suspect the arrival of Drug War refugees generates different population responses, for example, because deportations discourage future migrants Medina-Cortina (2023).

[30] The results using all homicides also show a positive impact on the employment rate of natives and native high-school dropouts, and no negative impact on their wages, as shown in Figure A.4.

sion in this estimation, we construct a Bartik instrument (BI) to predict county-level employment during the years of the Great Recession (Bartik, 1991). Specifically,

$$BI_{ct} = \Sigma_j IS_{jc,2000}\, \Delta PEmp_{jn(-c),t-2000}, \tag{4}$$

where $IS_{jc,2000}$ represents industry j's share of county c's total employment in 2000, and the term $\Delta PEmp_{jn(-c),t-2000}$ represents the growth rate of own-county-left-out national employment of industry j from 2000 to year t. The Bartik instrument thus represents the predicted effect of the Great Recession based on county-level beginning-of-period industry mix and national industry employment changes.

Figure B.8 plots the correlation between the Drug War and predicted county-level employment growth rate using the two estimation strategies we discussed above. Interestingly, these two methods show opposing correlations with the BI-predicted Great Recession shock. Under the RD method, we find that the counties more connected to the marginal-winning municipios are more negatively affected by the Great Recession, whereas if we use all homicides, the counties associated with a higher level of homicides perform better during the Great Recession. The correlation between the all-homicides shock and the Great Recession largely reflects the concentration of network-connected homicides in Texas counties near the border, where the Great Recession was mild. These correlations between the Drug War and the predicted employment changes are not ideal, but the opposing biases provide an opportunity to understand how the Great Recession affects our main results.

In columns 1 to 3 of Table 3, we show the estimates for undocumented Mexican immigrants, native employment rate, and the Bartik instrument using the two empirical methods, pooling together recession years (2008 through 2012). In columns 4 and 5, we report the estimates when controlling for the Great Recession industry shock—i.e., the Bartik instrument. Because the estimates using the two methods are on different scales, we rescale the native employment effect by the estimates on undocumented Mexican immigrants. The results show that after controlling for the Bartik instrument, the difference between the two rescaled estimates virtually disappears. The implied elasticities of native employment rates with respect to the

undocumented Mexican immigrants are 0.17 and 0.19 using the RD and all homicides. The similarity between the two estimates suggests that the impact of the Great Recession has a relatively small role in driving the results. It should be emphasized that these patterns show that any remaining bias from the Great Recession likely attenuates our main results, based on close elections.

4 Labor Market Mechanisms

In the primary analysis, we find that these migrants have caused local booms in native employment with minimal or no effect on the wages of native workers. In this section, we address questions raised by the findings: What explains the responses of the labor market? What do we learn about the structure of the labor market from the findings—especially the patterns of substitution between undocumented workers and other parts of the labor force? And, what explains the dynamics and (the lack of) long-run effects?

Any explanation for the empirical findings must account for the undocumented status of most Drug War migrants. Labor market segmentation between undocumented and domestic workers arises because of differences in human capital and the short-term nature of most undocumented migration, as well as legal barriers to formal work and the related inability to enforce contracts with employers for benefits, leave, and salaried positions (Hall and Greenman, 2015; Kristen et al., 2015; Lee, 2018).[31] These barriers have only grown in the modern U.S. economy due to stricter immigration policy and the increasing the increasing use of computers to check immigration documents. The attacks of 9/11, in particular, led to a major shift in immigration enforcement and federal focus on detaining and deporting undocumented immigrants.

Over the same time period, the U.S. labor market and economy have been transformed by the rise of electronic transactions, online gig work, administrative record databases, and just-in-time input supplies. The computerized economy both in-

[31] Consistent with this view, Figure B.10 shows that about 90 percent of likely undocumented immigrants do not have employer-provided health insurance, a figure much higher than for documented immigrant or native workers.

creases the chances that unauthorized workers are detected and introduces barriers to their participation in the broader economy.[32] Capital inputs can be adjusted relatively quickly in the modern economy, suggesting that the primary channel by which wages would fall in the classic model—a shortfall in capital (or other inputs to production)—may be less quantitatively important in the modern economy. Taken together, these underlying barriers and ongoing changes in the labor market imply that undocumented workers face are increasingly unlikely to substitute for formal workers.

The arrival of undocumented immigrants creates three channels of response in the local labor market. One, undocumented immigrants will seek work and, hence, represent an increase to the local supply of labor. In the classic model of labor demand, an increase in labor supply will cause a temporary reduction in the wages of similar workers, at least until firm investment returns the capital-to-labor ratio to its initial level. We find no evidence of this type of effect. The classic model of labor demand may, however, be a poor characterization of the economic effects of unauthorized immigration and the Drug War migrants if there is little overlap between the labor market for newly arrived unauthorized workers and the U.S. labor market. Instead, recent work on monopsony in the labor market appears to be a more apt characterization of the undocumented labor market (Amior and Manning, 2024; Borjas and Edo, 2023). In these models, an increase in the supply of undocumented labor increases profits for local firms. Put simply, firms exploit undocumented workers' inability to participate in the broader labor market in order to underpay them relative to their productive contribution, thereby increasing profits. Figure 7 shows that the counties receiving larger inflows of undocumented Mexican immigrants have higher rates of business creation, as is consistent with increased profits. These shifts in firm profit margins have ambiguous effects on firms' demand for incumbent workers. Our finding that there is a lack of a change in native employment in the most affected sectors suggests that any increased competition is offset by, one, complementarities between native and undocumented workers due to

[32]For example, earlier research has shown that the E-verify system reduces employment for workers who are likely ineligible for the system (Amuedo-Dorantes and Bansak, 2012; Ayromloo et al., 2020).

differences in skill sets and, two, profit sharing with incumbent workers (see Garin and Silvério (2024) for evidence on profit sharing).

A second key channel by which undocumented arrivals affect the labor market occurs because undocumented immigrants will, themselves, demand goods and services, increasing the demand for the labor input into those purchases. Due to their legal status, undocumented immigrants face enormous barriers to employment in hospitals, schools, local government, and other sectors dominated by long-term, formal workers, at the same time that their presence could increase the demand for health care, education, and government services. Native employment must fill these gaps. The evidence we show indicates that this channel plays a role in the response.[33]

It is worth noting that we would usually expect an increase in native wages to follow the increase in labor demand in these sectors. We do not detect such an effect. These increases may be offset by substitution between undocumented workers and domestic workers (likely small, based on the discussion above), though it is also possible the coincidence of our natural experiment with the Great Recession reduced wage gains that may have otherwise occurred. Recessions are thought to reduce upward pressure on wages. The all-homicides results support this interpretation. In that analysis (discussed in Section 3), we find a wage increase that follows the same time path as the change in employment. This can be rationalized by the fact that the all-homicides results implicitly reweight the sample to areas of the country that experienced mild increases in unemployment, especially Texas.[34] Further evidence of surplus labor supply appears in that the surge in native employment is associated with significant reductions in the unemployment rate—firms did not need to raise wages during this period to find workers.

The third channel is that hiring costs will fall due to the need to create new jobs for newly arriving immigrants (Albert, 2021). This channel predicts a large response to the initial arrivals—a response that will fade as the rate of new arrivals slows over time. The evidence on declining unemployment rates and increases in

[33] East et al. (2023) concludes that changes in demands for goods and services explain their similar results.

[34] We also tried splitting the sample into areas of high and low exposure to the Great Recession, finding suggestive evidence of wage increases in the low-exposure areas (Figure B.9).

both separations and new hires supports the operation of this channel. Put another way, the labor market became more dynamic following the arrival of these migrants. One key piece of evidence is consistent with a decline in hiring costs in the most affected sectors—we find a long-run increase in the number of non-Mexican foreign born immigrants in the affected counties. The long-run increase in employment in the affected sectors is further evidence that the affected counties see an expansion of immigrant-friendly employment opportunities. It also possible that changes in migration routes across the border or the growth of existing Spanish-speaking enclaves can explain some portion of the finding.

To sum up, the overall response of the labor market combines the effects of changes in labor supply, labor demand, and a reduction in hiring costs. The effects of the increase in labor supply appear to be mediated by the segmented nature of the U.S. labor market, in which incumbent workers experience minimal competition with newly arrived undocumented workers because of skill differences and legal barriers to work. The lack of negative effects on native employment in the most affected sectors, in particular, indicates that substitution between new arrivals and incumbents is small. Labor demand effects in sectors that employ relatively fewer undocumented workers can explain increasing native employment in the short run. Regarding the structure of the labor market, a model of monopsony is indicated by the increase in business starts. Evidence on reduced unemployment rates, increases in both hires and separations, and an increased inflow of non-Mexican foreign-born workers indicates that hiring costs fell.

Finally, the lack of long-run effects is consistent with the possible mechanisms discussed above. For example, it is possible that there is either a short-run boom due to lower hiring costs or that labor market search erodes the monopsony power of the employers of immigrant. A specific possibility is that a growing share of migrants attain permanent resident status in the long run, thereby integrating them into the U.S. labor market. The regularization of workers can reduce the monopsony power of firms (Borjas and Edo, 2023). Finally, most undocumented migrants return to Mexico within a few years of arrival (Angelucci, 2012; Carrión-Flores, 2018). We find a long-run increase in the local population of likely undocumented people, but it is possible the persistent effect on the stock masks changes in composition due

to the combination of new arrivals and return migration. If this selection resulted in more educated, English-speaking migrants replacing the initial arrivals over time (as suggested by Reinhold and Thom (2013) and Carrión-Flores (2018)), labor market frictions and complementarities due to skill differences may fall. More detailed data linking firms and workers over time would be helpful in further disentangling the mechanisms that explain changes in the labor market.

5 Conclusion

This paper reports the first large-scale, quasi-experimental evidence on the effect of the migration of Drug War refugees and contributes a new understanding of the effects of undocumented migrant arrivals on the U.S. labor market. In the first part of the paper, we provide a new estimate of the effect of violence on migration. The results show clear effects of Drug War violence on out-migration to the United States. Downstream effects on U.S. populations reveal a surge in likely-undocumented Mexicans with no change in native or likely-documented Mexican populations. Following the arrival of the Drug War migrants, employment rates of natives rise. The evidence is consistent with an increase in labor demand, complementarity between the new arrivals and incumbent workers, and monopsony power on the part of employers of the immigrants. In the longer run, drug violence leads to an expansion of immigrant populations in the United States with little effect on native populations, employment, or wages.

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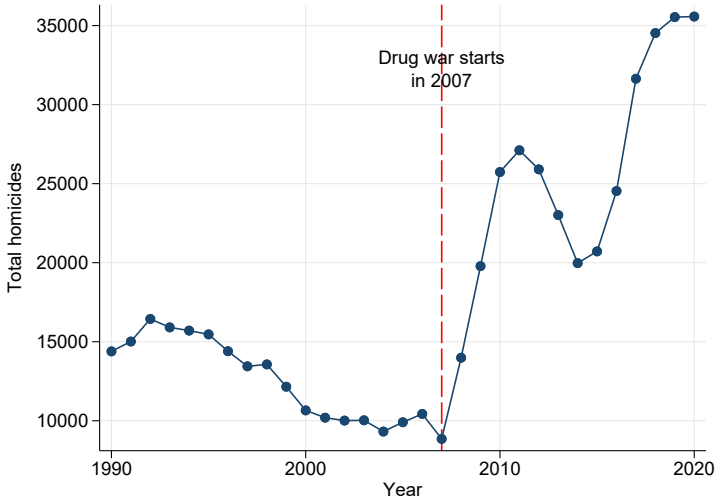
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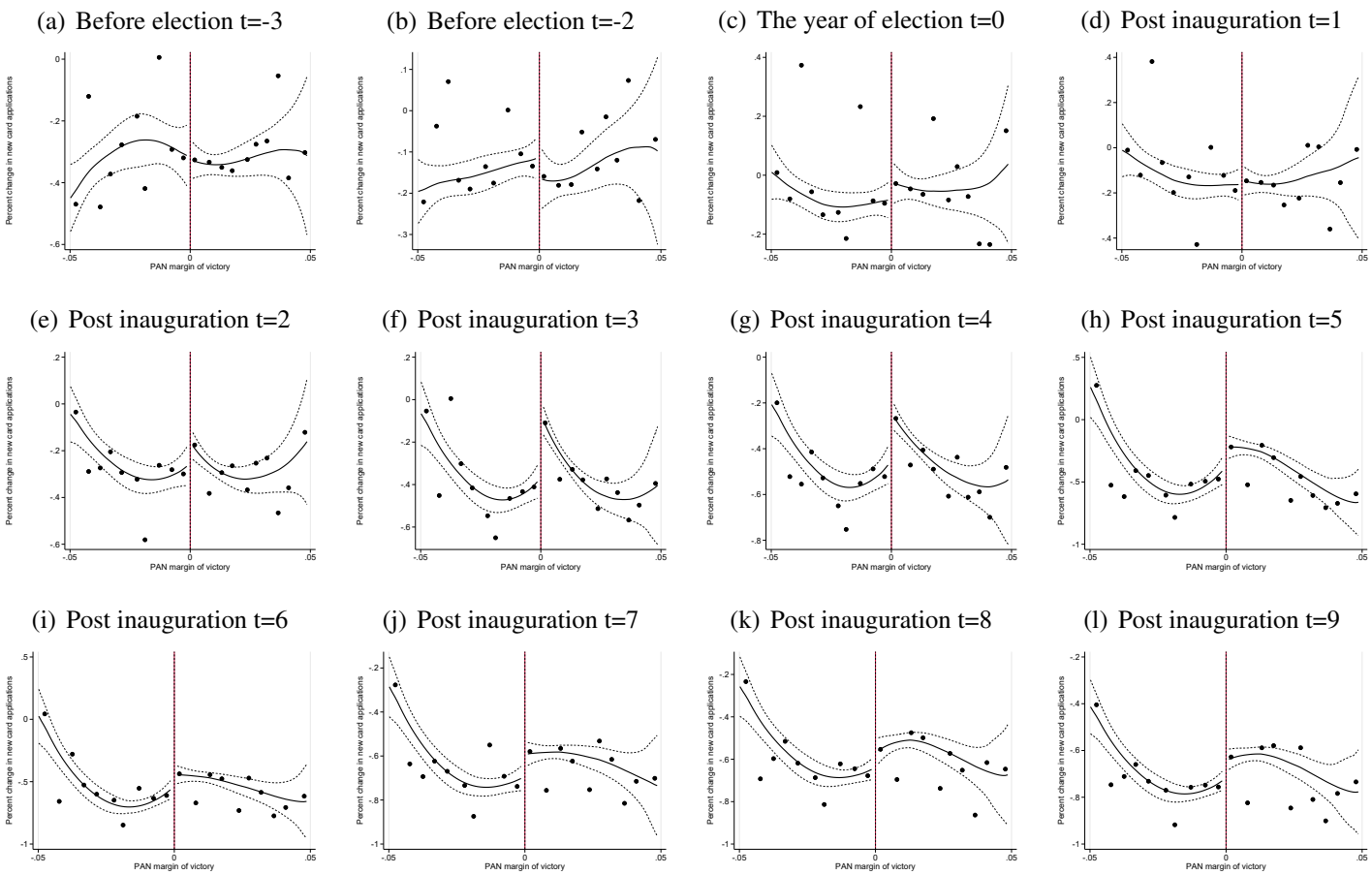
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Figure 1: Total homicides in Mexico



NOTE: This figure plots total national homicides for Mexico from 1990 to 2000. The homicides data come from the National Institute of Statistics and Geography (INEGI, from its former name in Spanish: Instituto Nacional de Estadística, Geografía e Informática).

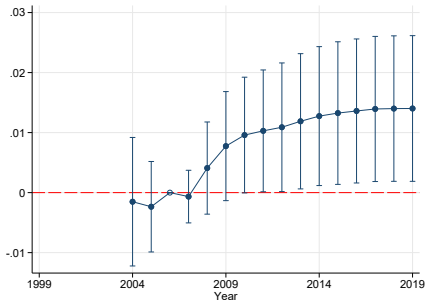
Figure 2: Percentage change in new MCAS Card applications relative to the year before the elections by margins of victory



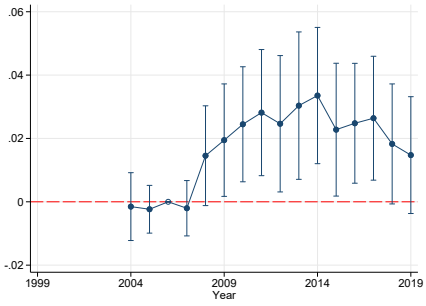
NOTE: This figure shows the percentage change in new Matricula Card applications relative to the year before the election ($t = -1$) by margins of victory in the mayoral elections of 2007 and 2008. We restrict the sample to individuals between 18 and 35 years old. Each dot represents the weighted average of percentage change in each of the bins, where the weights are 2005 municipio-level populations. The solid lines are fitted polynomial lines, separately for winning and losing municipios, with triangle kernels. The dashed lines are the associated 95 percent confidence intervals.

Figure 3: MCAS card application and likely-undocumented Mexican population responses

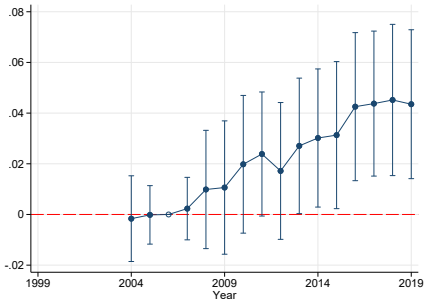
(a) New card applications (cumulative)



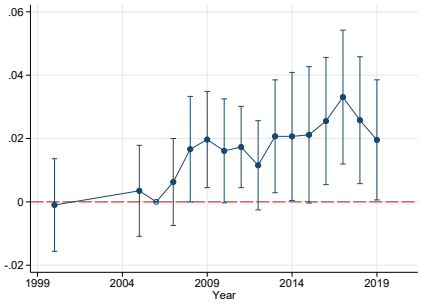
(b) New card applications (flow)



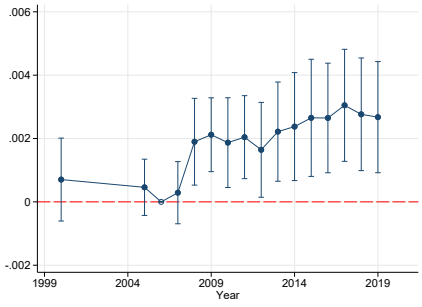
(c) Card renewals (flow)



(d) Likely undocumented Mex immigrants (ACS)

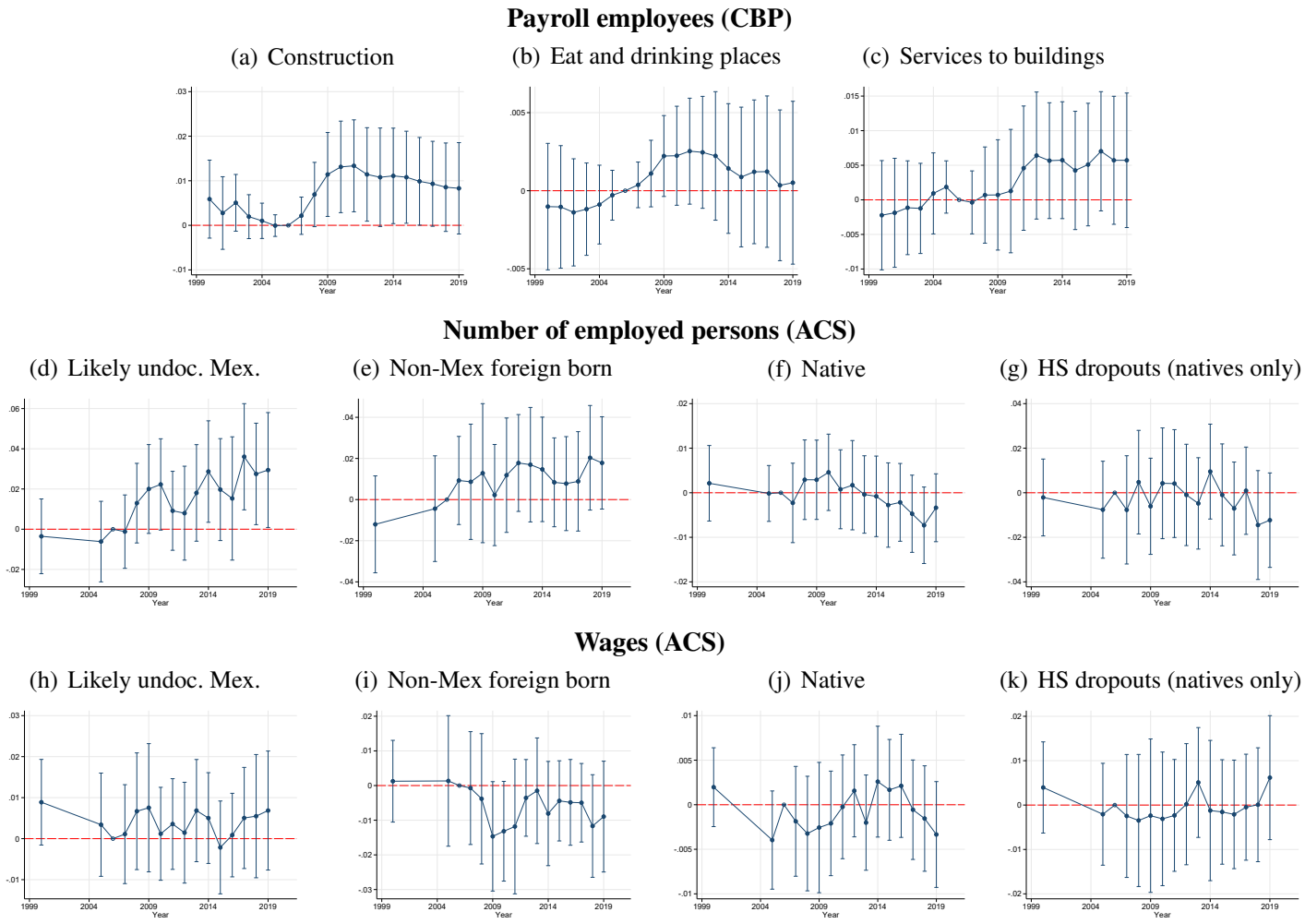


(e) Share of likely undocumented Mex immigrants (ACS)



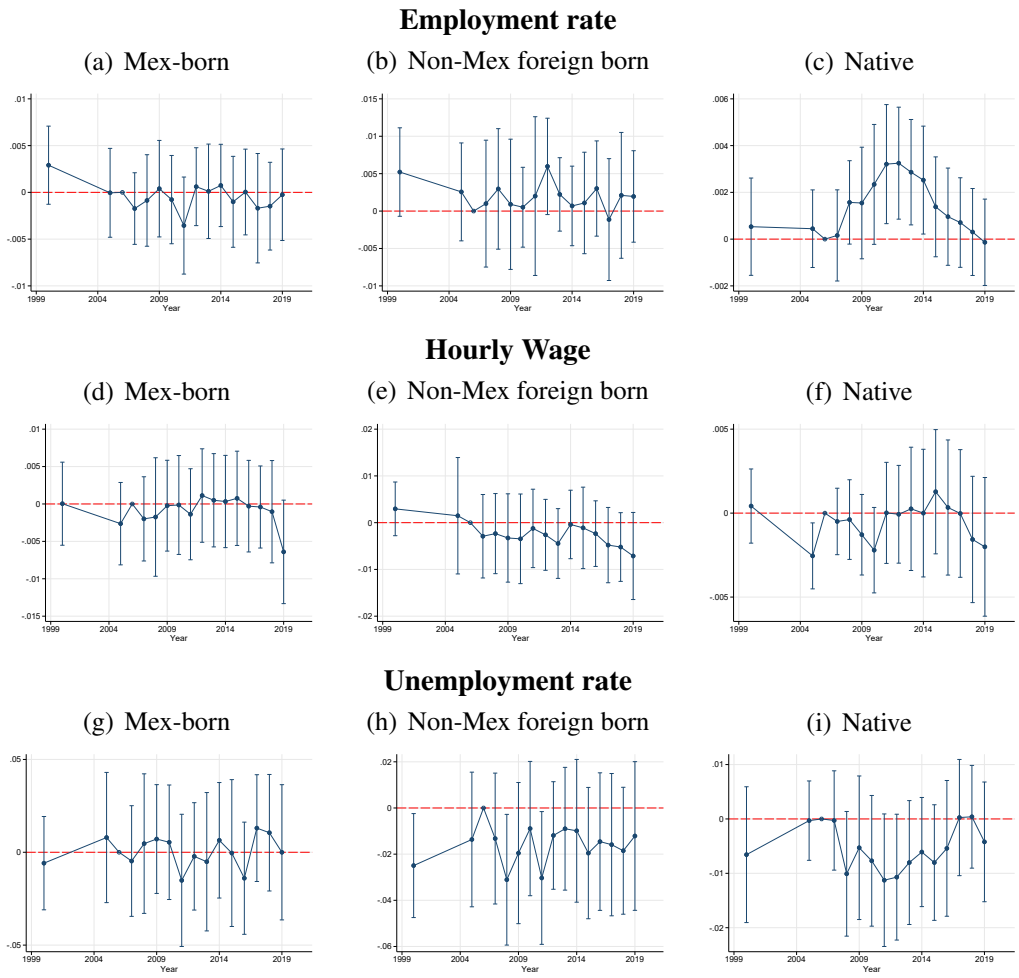
NOTE: This figure presents the estimates for α_1 in Equation 3. Subfigure (a) shows the estimates for cumulative county-level new card applications to capture total inflows since 2006. Note that the county-level new card applications here include not only migrants from the marginally winning and marginally losing municipios, but also those from other municipios. Subfigures (b) and (c) show the estimates for flows of new card applications and renewals. Subfigures (a)–(c) use Matricula data. Subfigures (d) and (e) plot the coefficients for a series of regressions using Decennial Census data in 2000 and ACS data from 2005. We follow Borjas and Cassidy (2019) to define likely undocumented immigrants. All regressions use 2006, represented by hollow circles, as the pre-period. Standard errors are clustered by county.

Figure 4: Employment and wages in undocumented-Mexican-worker-intensive industries



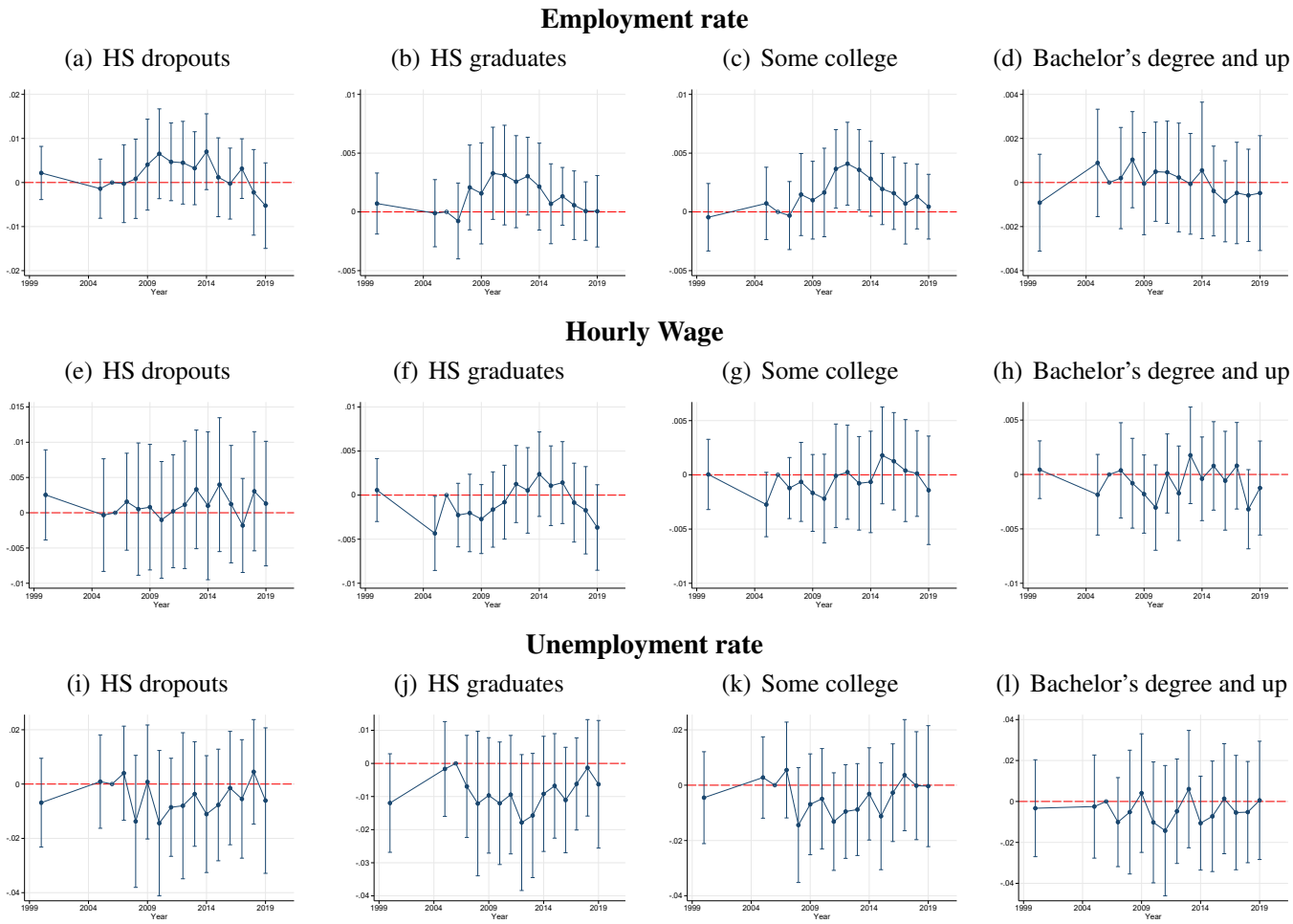
NOTE: See text and footnote for Figure 3 for details regarding the regressions. Three industries in the CBP data are based on NAICS code system: construction (NAICS 23), food service and drinking places (NAICS 722), and services to buildings and dwellings (NAICS 5617). We pool together the undocumented-Mexican-worker-intensive industries shown in the first three panels but using the corresponding Census industry code: all construction (60), eating and drinking places (641), landscape and horticultural services (20), services to dwellings and other buildings (722). Then we break down the data by the key demographic groups of interest.

Figure 5: The impact of inflows on county-level labor market outcomes



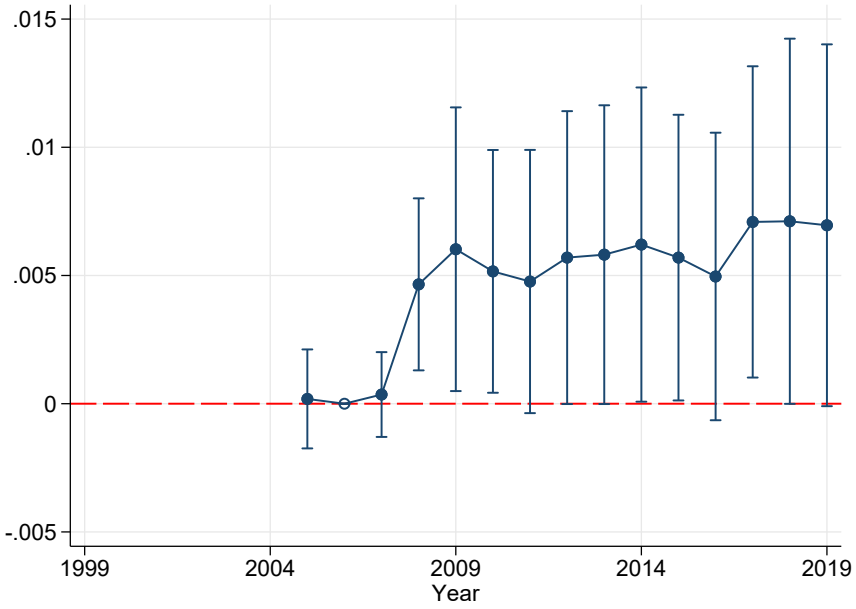
NOTE: This figure presents the estimates for α_1 in Equation 3. The 9 subfigures plot the coefficients for a series of regressions using ACS data. The outcome variables in this figure is at county level. All regressions use 2006, represented by hollow circles, as the control period. Standard errors are clustered by county.

Figure 6: The impact of inflows on county-level labor market outcomes by education levels (natives only)



NOTE: This figure presents the estimates for α_1 in Equation 3. The 12 subfigures plot the coefficients for a series of regressions using ACS data. The outcome variables in this figure is at county level. All regressions use 2006, represented by hollow circles, as the control period. Standard errors are clustered by county.

Figure 7: The impact of inflows on county-level business formation



NOTE: This figure presents the estimates for α_1 in Equation 3 where the outcome variable is county-level new business applications. We use data from Business Formation Statistics which include data between 2005 and 2019 (data source: https://www.census.gov/econ/bfs/data/county.html). All regressions use 2006, represented by hollow circles, as the control period. Standard errors are clustered by county.

Table 1: U.S. population responses

	(1) New cards	(2) Likely undoc. Mex	(3) Mex-born	(4) Non-Mex fborn	(5) Natives	(6) HSD (natives)
	Panel A: Medium run (2011–2014)					
$RelNewCard_{ct}^{post}$	0.0123**	0.0162***	0.0068	0.0128**	-0.0026	-0.0043
	(0.0059)	(0.0060)	(0.0042)	(0.0055)	(0.0018)	(0.0034)
N	240	240	240	240	240	240
	Panel B: Longer run (2016–2019)					
$RelNewCard_{ct}^{post}$	0.0146**	0.0251***	0.0090*	0.0194***	-0.0036*	-0.0027
	(0.0065)	(0.0081)	(0.0050)	(0.0064)	(0.0021)	(0.0042)
N	240	240	240	240	240	240

NOTE: This table presents the estimates for α_1 in Equation 3, pooling multiple years together. We first calculate county-level average populations by demographic groups for three time periods: 2005–2006, 2011–2014, 2016–2019. Then we estimate Equation 3 using 2005–2006 as pre-period, and the other two periods as post periods. $RelNewCard_{ct}^{post}$ represents the difference between the inverse hyperbolic sine (IHS) transformed and BH-method demeaned predicted new card applications from the PANwin ($CNC_{ct}^{WinXPost}$) and the PANlose ($CNC_{ct}^{LoseXPost}$) municipios post inauguration. Standard errors are clustered by county.

Table 2: The impact of inflows on labor market by demographic groups (ACS data)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Medium run				**Longer run**			
	Likely undoc. Mex.	Mex. born	Non-Mex fborn	Natives	Likely undoc. Mex.	Mex. born	Non-Mex fborn	Natives
	Panel A: Employed persons (pooling together undocumented-Mexican-worker-intensive industries)							
$RelNewCard_{ct}^{post}$	0.0200**	0.0118*	0.0199**	0.0004	0.0311***	0.0134**	0.0130	-0.0042
	(0.0082)	(0.0062)	(0.0095)	(0.0036)	(0.0105)	(0.0066)	(0.0094)	(0.0036)
	Panel B: Wages (pooling together undocumented-Mexican-worker-intensive industries)							
$RelNewCard_{ct}^{post}$	0.0034	0.0033	-0.0035	0.0025	0.0036	0.0017	-0.0076	0.0009
	(0.0032)	(0.0034)	(0.0082)	(0.0021)	(0.0042)	(0.0030)	(0.0061)	(0.0021)
	Panel C: Employment rate (county wide)							
$RelNewCard_{ct}^{post}$	-0.0030	-0.0004	0.0016	0.0027***	-0.0023	-0.0007	0.0004	0.0003
	(0.0022)	(0.0018)	(0.0024)	(0.0010)	(0.0023)	(0.0017)	(0.0024)	(0.0008)
	Panel D: Wages (county wide)							
$RelNewCard_{ct}^{post}$	0.0031	0.0016	-0.0030	0.0013	0.0010	-0.0007	-0.0058**	0.0003
	(0.0025)	(0.0022)	(0.0029)	(0.0016)	(0.0026)	(0.0021)	(0.0026)	(0.0019)

NOTE: This table presents the estimates for α_1 in Equation 3 but pooling multiple years together. We first calculate industry- or county-level averages for each of the variables for three time periods: 2005–2006, 2011–2014, 2016–2019. Then we estimate Equation 3 using 2005–2006 as pre-period, and the other two periods as post periods.The sample includes 120 counties, though for Panel A and Panel B, we have fewer observations when we estimate the impact on Non-Mex foreign borns. $RelNewCard_{ct}^{post}$ represents the difference between the inverse hyperbolic sine (IHS) transformed and BH-method demeaned predicted new card applications from the PANwin ($CNC_{ct}^{WinXPost}$) and the PANlose ($CNC_{ct}^{LoseXPost}$) municipios post inauguration. Standard errors are clustered by county.

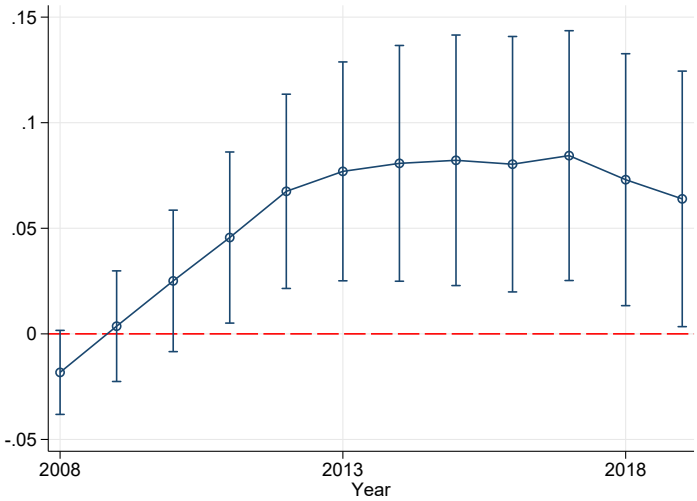
Table 3: Accounting for the Great Recession in undocumented populations and native employment responses

	(1) Likely undoc. Mex	(2) Emp. rate (natives)	(3) BI-pred. Emp.	(4) Likely undoc. Mex	(5) Emp. rate (natives)
			Panel A: Estimates from mayoral elections RD		
$RelNewCard_{ct}^{post}$	0.0143***	0.0022**	-0.0008*	0.0156***	0.0026***
	(0.0054)	(0.0009)	(0.0004)	(0.0056)	(0.0010)
Changes relative to undoc. Mex		**0.15**	**−0.056**		**0.17**
N	240	240	240	240	240
			Panel B: Estimates from all homicides		
ATT	0.3340**	0.0631***	0.0242***	0.3835**	0.0745***
	(0.1674)	(0.0191)	(0.0051)	(0.1636)	(0.0218)
Changes relative to undoc. Mex		**0.19**	**+0.072**		**0.19**
N	658	658	658	658	658
Control for BI				Yes	Yes

NOTE: This table presents the estimates for α_1 in Equation 3 and the estimates based on all homicides, but pooling multiple years together (corresponding to Appendix A). We first calculate county-level average populations of undocumented Mexican, employment rate for natives, and Bartik-instrument (BI) predicted percent change in employment for two time periods: 2005–2006, 2008–2012. The two sets of estimation are then using 2005–2006 as pre-period, and 2008–2012 as post-period. $RelNewCard_{ct}^{post}$ represents the difference between the inverse hyperbolic sine (IHS) transformed and BH-method demeaned predicted new card applications from the PANwin ($CNC_{ct}^{WinXPost}$) and the PANlose ($CNC_{ct}^{LoseXPost}$) municipios post inauguration. Standard errors are clustered by county.

A Online Appendix: Regressions using All Homicides

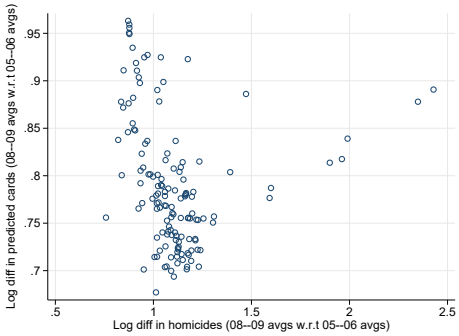
Figure A.1: Correlation between municipio homicides and municipio new card applications



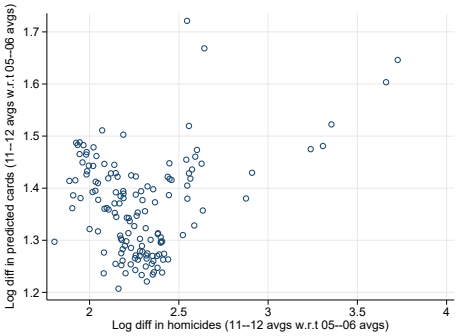
Notes: This figure shows the estimates for β_1 in Equation 2, which represents the correlation between homicides and new card applications at municipio level. Both dependent and independent variables are **cumulative** from 2007. The changes are constructed as log difference with respect to 2007. The samples for these regressions are restricted to municipios with at least 50,000 people. Robust standard errors are used in the regressions.

Figure A.2: Correlation between network-predicted cards and network-predicted homicides

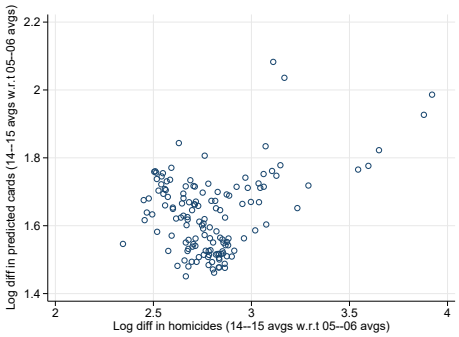
(a) 08–09 avgs w.r.t 05–06 avgs



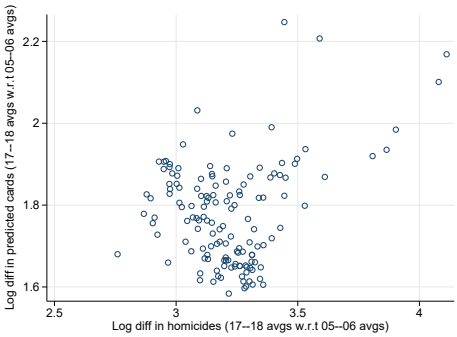
(b) 11–12 avgs w.r.t 05–06 avgs



(c) 14–15 avgs w.r.t 05–06 avgs

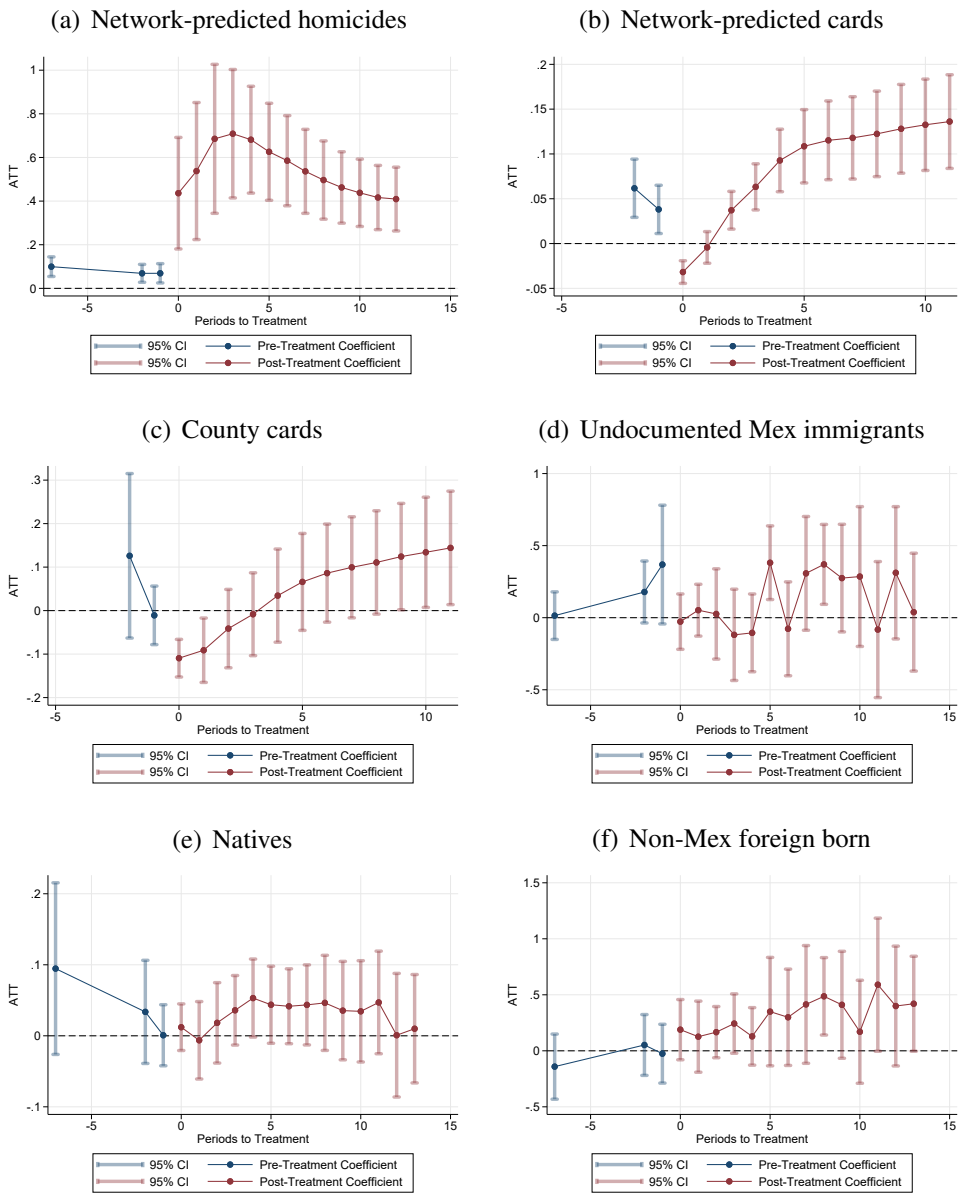


(d) 17–18 avgs w.r.t 05–06 avgs



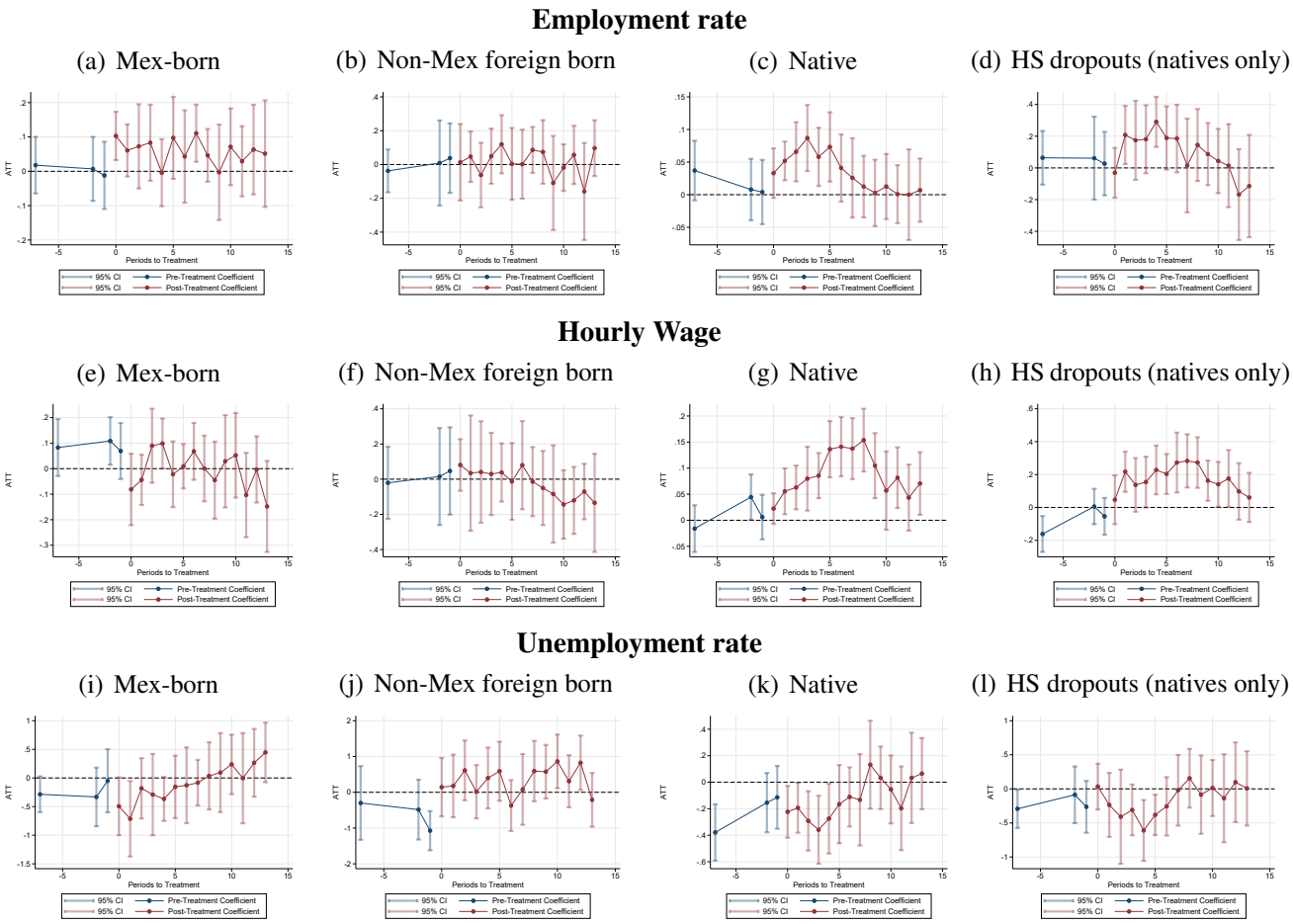
Notes: This figure shows the correlation between the change in network-predicted new cards and the change in network-predicted homicides. A dot represents a U.S. county. We restrict the sample to the municipios with at least 50,000 people and use these municipios to predict the inflows and homicides.

Figure A.3: The impact of Drug-war-related violence on U.S. counties (OLS)



Notes: This figure shows the impact of Drug-War-related violence on U.S. counties. We compare counties that are the most exposed to violence (top 10 percentiles in terms of log change in violence between 2005–2006 avg and 2017–2018 avg) and other counties. The subfigures plot the difference-in-difference estimates using the methods in Callaway and Sant'Anna (2021). In these regressions, the year before the start of the Drug War, 2007, is the base year. All estimates are relative to 2007.

Figure A.4: The impact of inflows on county-level labor market outcomes

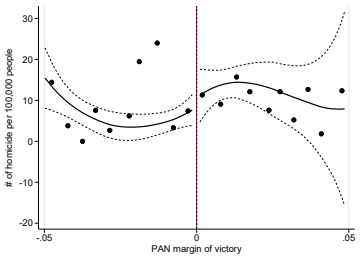


Notes: This figure shows the impact of Drug-War-related inflows of on U.S. labor market. We compare counties that are the most exposed to violence (top 10 percentiles in terms of log change in violence between 2005–2006 avg and 2017–2018 avg) and other counties. The subfigures plot the difference-in-difference estimates using the methods in Callaway and Sant'Anna (2021). In these regressions, the year before the start of the Drug War, 2007, is the base year. All estimates are relative to 2007.

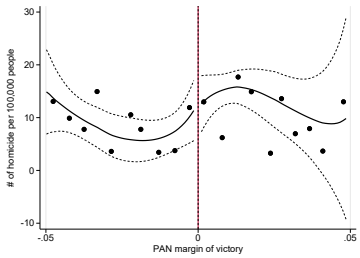
B Online Appendix Figures and Tables

Figure B.1: Homicide per 100,000 people (event, t=0 in the year of election)

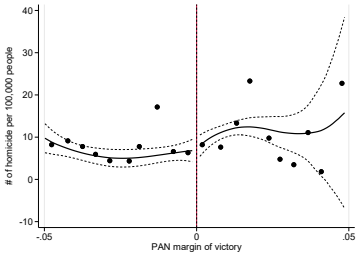
(a) Before election t=-2



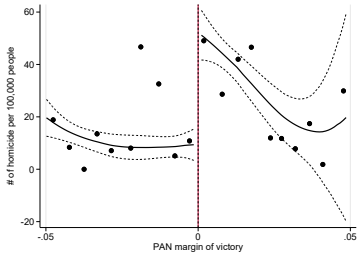
(b) Before election t=-1



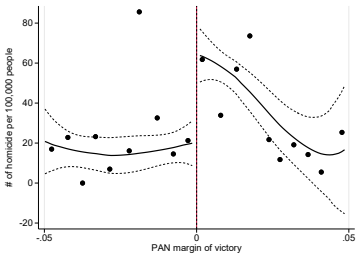
(c) The year of election t=0



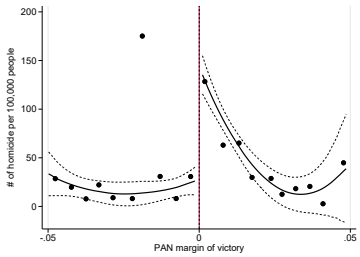
(d) Post inauguration t=1



(e) Post inauguration t=2



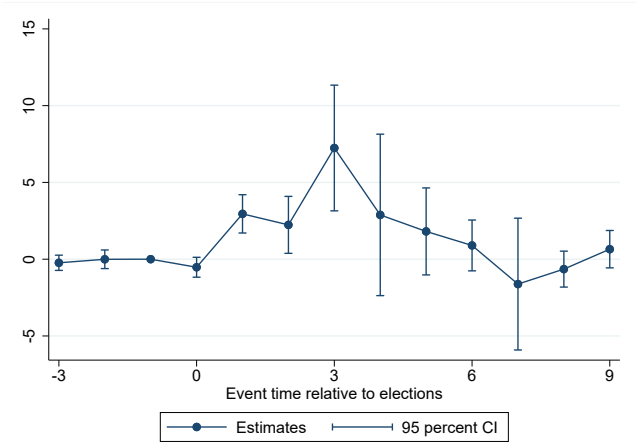
(f) Post inauguration t=3



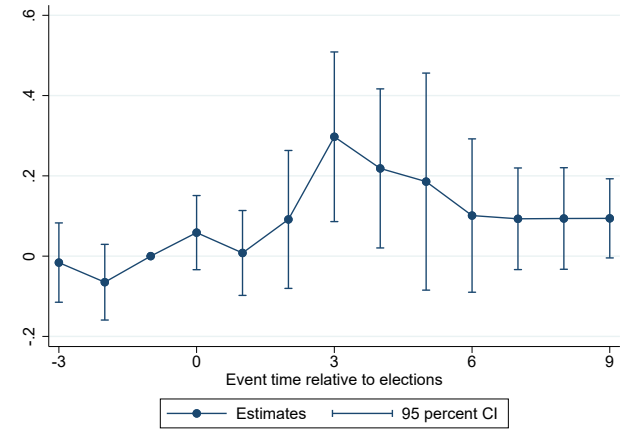
Notes: This figure shows Mexican municipio-level homicide rate by margins of victory in the mayoral elections in 2007 and 2008. Each panel represents one period relative to the year of election. See more details in the footnote of Figure 2.

Figure B.2: RD estimates

(a) Homicides



(b) New card applications

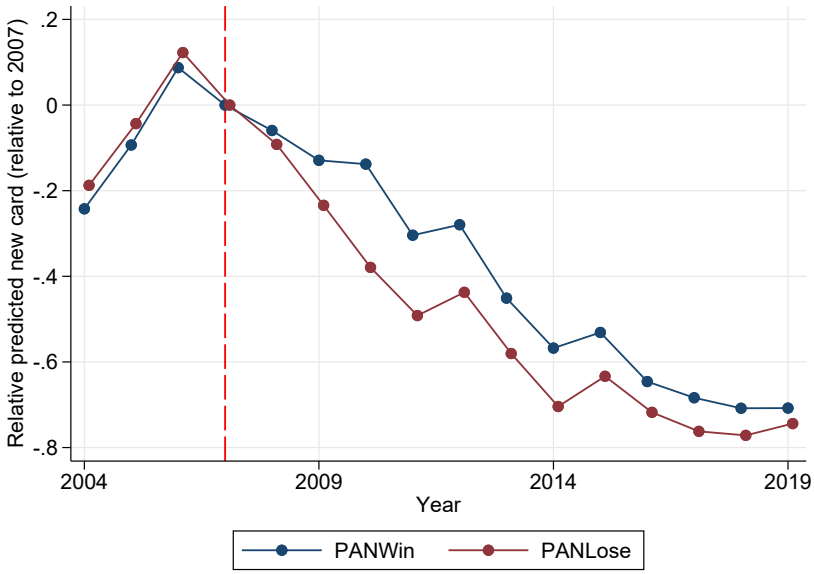


Notes: This figure plots the RD regressions as in Equation (1) of Dell (2015). Specifically, the estimating equation is

$$y_m = \alpha_0 + \alpha_1 * PANwin_m + \alpha_2 spread_m + \alpha_3 spread_m^2 + \alpha_4 PANwin_m * spread_m + \alpha_5 PANwin_m * spread_m^2 + \varepsilon_m,$$

where y_m represent percent change in homicides and new card application for each period with respect to $t-1$. Regressions are weighted by 2005 municipio population. The figure plots the estimates for α_1. The homicides data come from the National Institute of Statistics and Geography (INEGI from its former name in Spanish: Instituto Nacional de Estadística, Geografía e Informática).

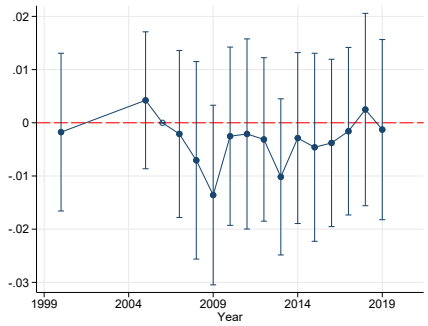
Figure B.3: Predicted inflows into the United States from marginally won and lost municipios



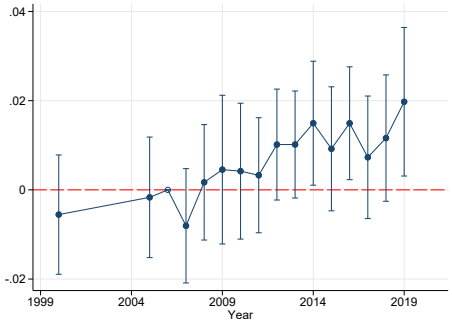
Notes: This figure plots the trends for the sum of predicted new cards across all relevant receiving counties, that is, $\Sigma_c CNC_{ct}^{Win}$ and $\Sigma_c CNC_{ct}^{Lose}$ in the United States. We normalize the numbers relative to year 2007—the year before any inauguration took place.

Figure B.4: The impact of inflows on county-level population

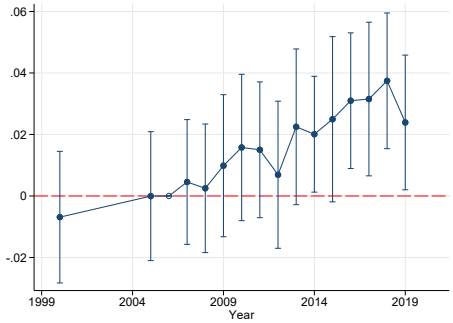
(a) Documented Mex immigrants (ACS)



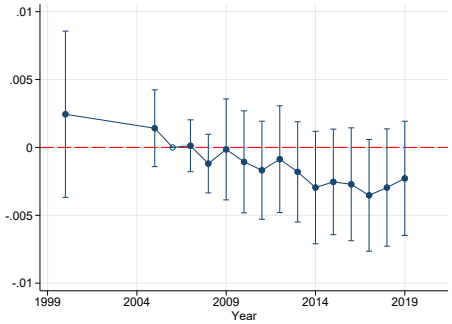
(b) Non-Mex foreign born (documented; ACS)



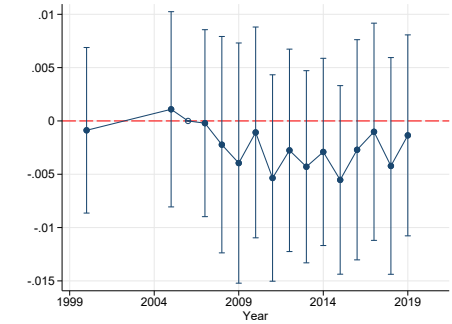
(c) Non-Mex foreign born (undocumented; ACS)



(d) Native (ACS)



(e) HS dropouts (Natives; ACS)

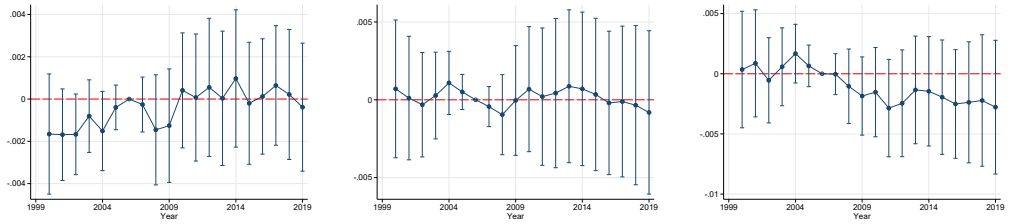


Notes: This figure presents the estimates for α_1 in Equation 3. For each figure, we plot the coefficients for a series of regressions using data in 2000 and after 2005. In these regressions, we use 2006, represented by hollow circles, as the control period. Each solid circle represents a coefficient in one regression using one post-inauguration year as the post-period. We also estimate the hypothetical treatment effect using 2000 and 2005 data to test for pre-treatment balance. In these regressions, standard errors are clustered by county.

Figure B.5: The impact of inflows on undocumented-Mexican-worker-intensive industries

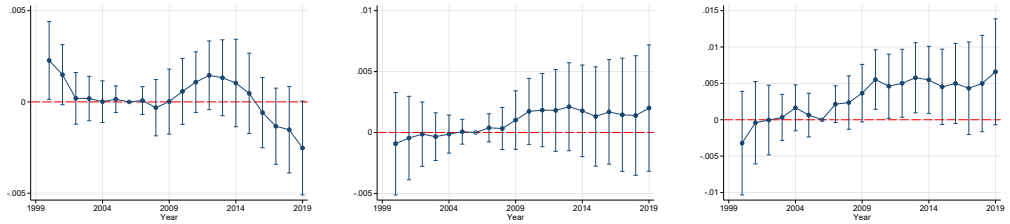
Construction (NAICS 23)

(a) Earnings per worker (QWI) (b) No. of est. (CBP) (c) No. of est. (< 5 employees)



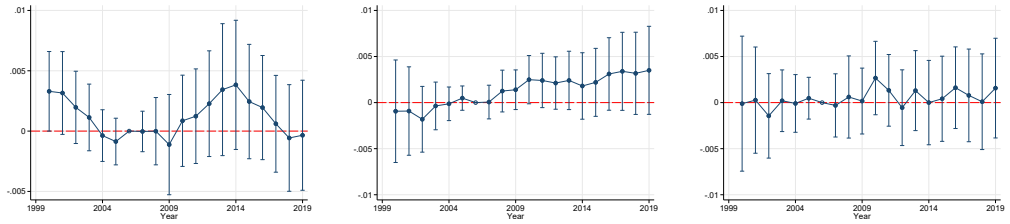
Food service and drinking places (NAICS 722)

(d) Earnings per worker (QWI) (e) No. of est. (CBP) (f) No. of est. (< 5 employees)



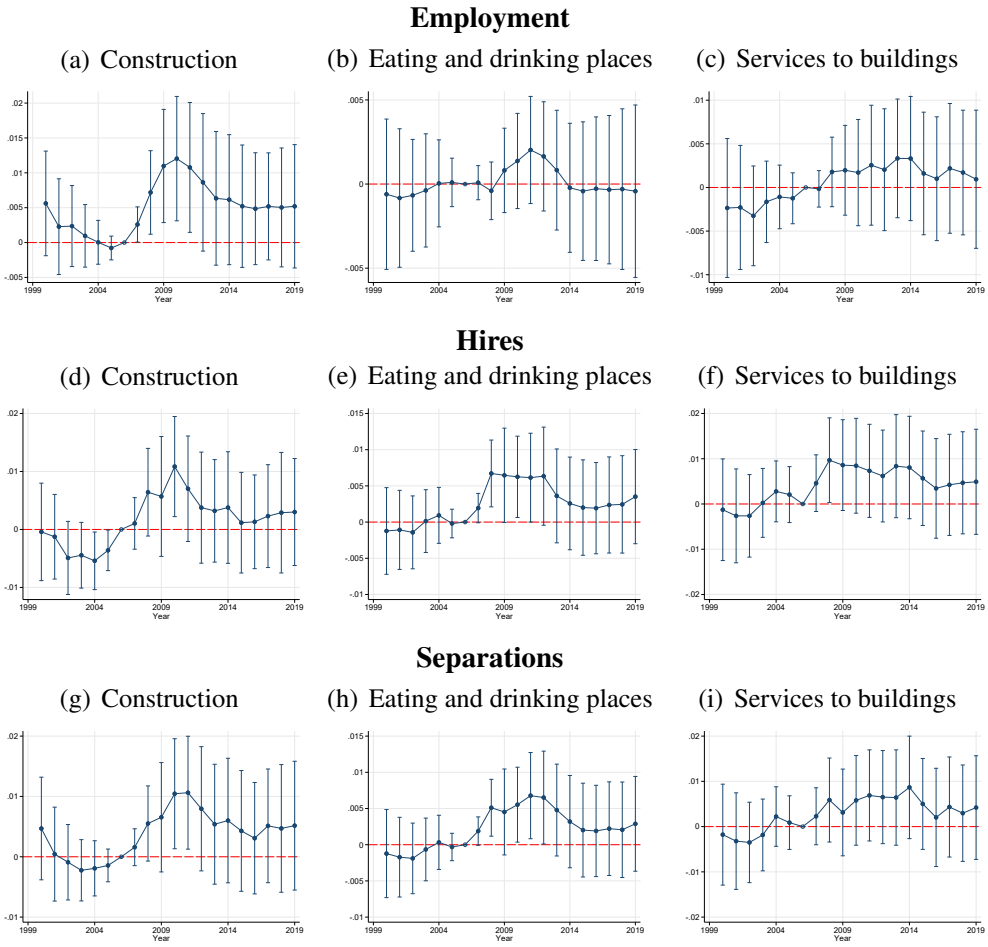
Services to buildings and dwellings (NAICS 5617)

(g) Earnings per worker (QWI) (h) No. of est. (CBP) (i) No. of est. (< 5 employees)



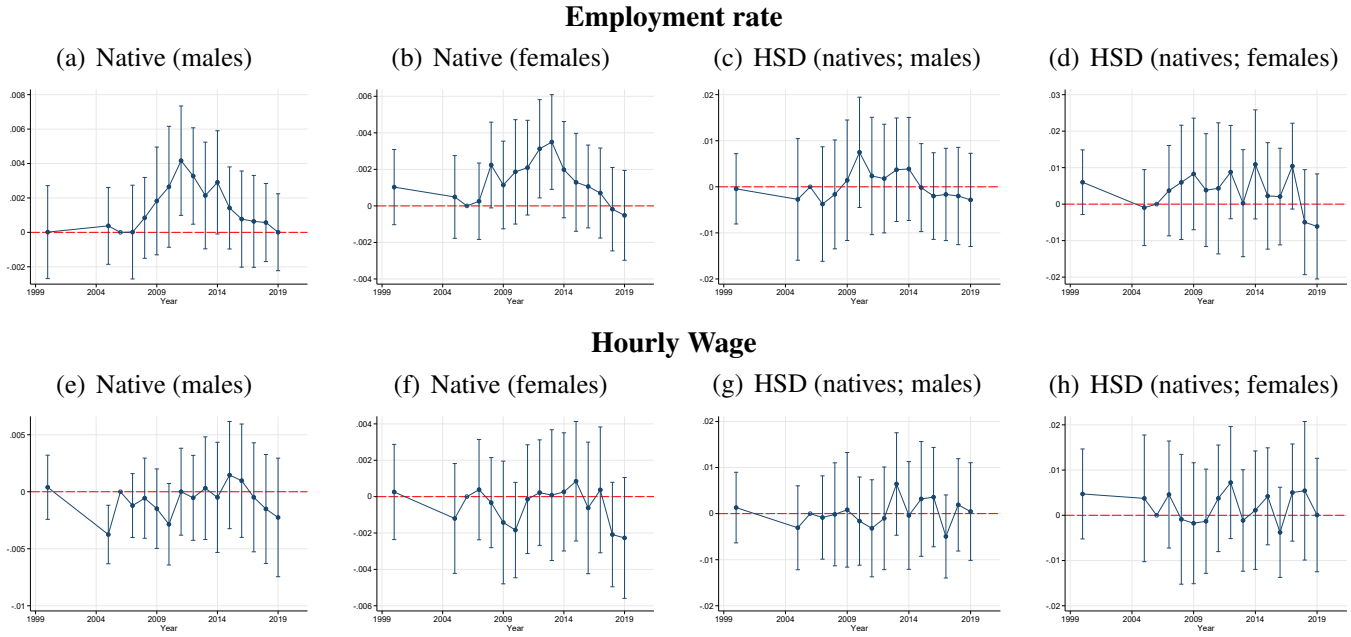
Notes: This figure presents the estimates for α_1 in Equation 3. The 9 subfigures plot the coefficients for a series of regressions using CBP and QWI data. All regressions use 2006, represented by hollow circles, as the control period. Standard errors are clustered by county.

Figure B.6: QWI—employment and turnover



Notes: This figure presents the estimates for α_1 in Equation 3. For each figure, we plot the coefficients for a series of regressions using QWI data between 2000 and 2019. In these regressions, we use 2006, represented by hollow circles, as the control period. Each solid circle represents a coefficient in one regression using one post-inauguration year as the post-period. We also estimate the hypothetical treatment effect using data between 2000 and 2005 to test for pre-treatment balance. In these regressions, standard errors are clustered by county.

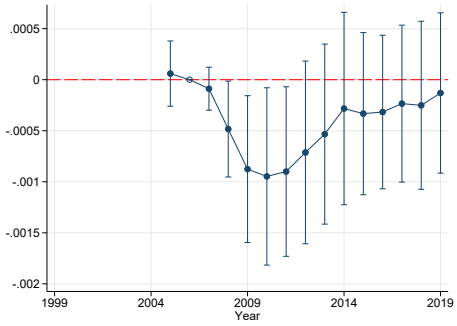
Figure B.7: The impact of inflows on county-level labor market outcomes by gender



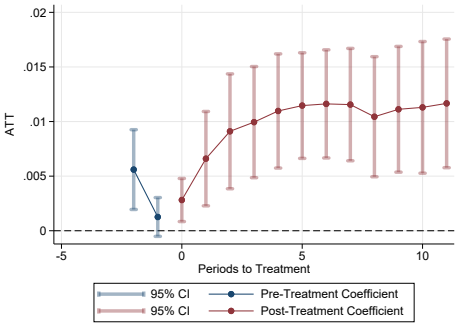
Notes: This figure presents the estimates for α_1 in Equation 3. The subfigures plot the coefficients for a series of regressions using ACS data. The outcome variables in this figure are at county level. All regressions use 2006, represented by hollow circles, as the control period. Standard errors are clustered by county.

Figure B.8: Shift-share predicted percent changes in employment

(a) RD

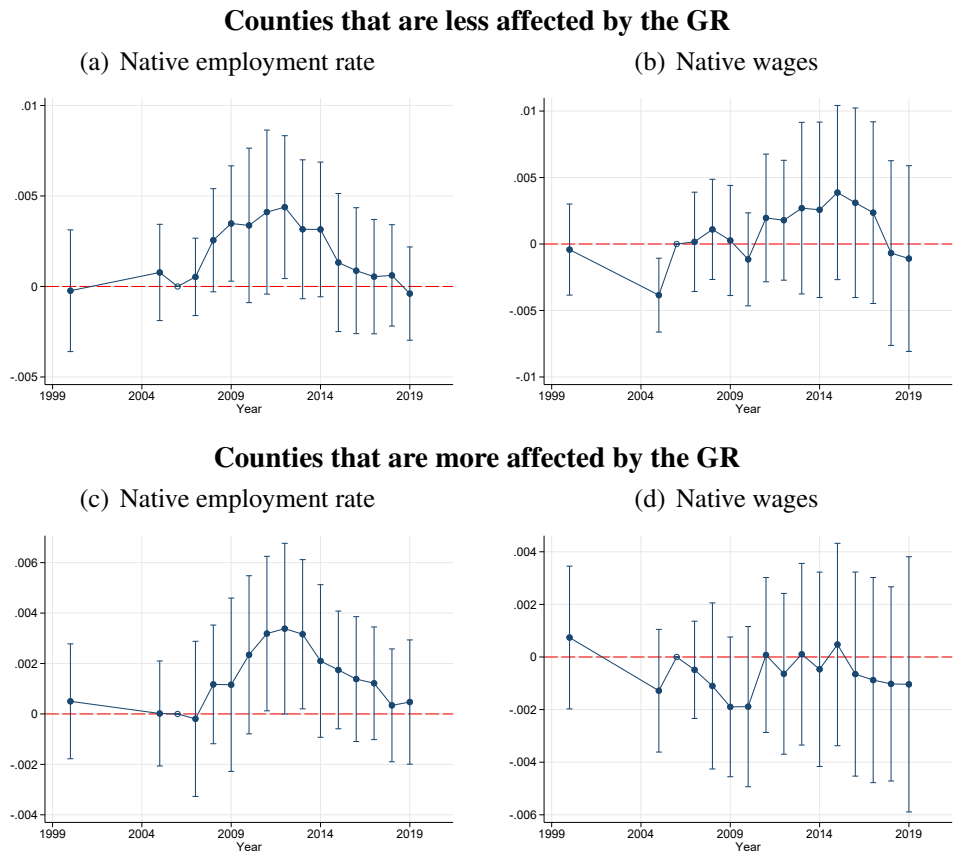


(b) All homocides



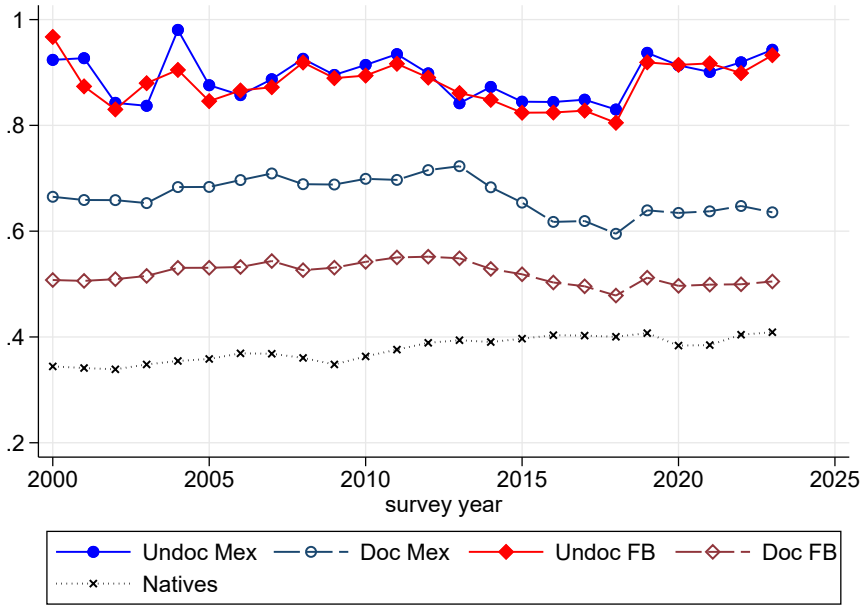
Notes: This figure shows the correlation between the inflows of undocumented Mexican immigrants and the Bartik-instrument predicted percent change in employment from 2000 to the year of interest. The Bartik instrument is constructed by interact 2000 county industry mix with own-county-left-out national changes in industry employment from 2000 to each year between 2005 and 2019.

Figure B.9: The impact of inflows on county-level labor market outcomes by severity of the Great Recession



Notes: This figure presents the estimates for α_1 in Equation 3. The subfigures plot the coefficients for a series of regressions using ACS data. The outcome variables in this figure are at county level. We measure exposure to the Great Recession using the Bartik instrument. We calculate the change in Bartik instrument from 2006 to 2010 and then divide the 120 counties using the median value. The upper 50 percentiles are the counties that are less exposed and the bottom 50 percentiles are more exposed. All regressions use 2006, represented by hollow circles, as the control period. Standard errors are clustered by county.

Figure B.10: Share of workers with no employer-provided health insurance by demographic groups



Notes: This figure shows the share of workers, between 18 and 64, without employer-provided health insurances. The calculation excludes self-employed workers. Definition for undocumented immigrants follows Borjas and Cassidy (2019). Data source: Current Population Survey.

Table B.1: Share of likely undocumented Mexican immigrants by industry in 2005 and 2019 (ACS)

Industry (Census 1990)	Share of undoc. Mex.		
	2005	2005 (Cum.)	2019
All construction	23.7	23.7	22.9
Eating and drinking places	14.5	38.2	14.6
Agricultural production, crops	5.8	44.0	6.0
Landscape and horticultural services	5.2	49.2	5.2
Services to dwellings and other buildings	2.9	52.1	5.0
Private households	2.5	54.6	2.3
Hotels and motels	2.1	56.7	2.2
Groceries and related products	1.8	58.5	1.0
Meat products	1.6	60.1	1.2
Grocery stores	1.5	61.6	1.7
Automotive repair and related services	1.4	62.9	1.6
Personnel supply services	1.4	64.3	0.7
Agricultural production, livestock	1.3	65.6	1.4
Furniture and fixtures	1.2	66.8	0.5
Laundry, cleaning, and garment services	1.0	67.8	0.7
Apparel and accessories, except knit	1.0	68.7	0.4
Sawmills, planing mills, and millwork	0.9	69.6	0.6
Miscellaneous entertainment and recreation services	0.8	70.4	0.8
Trucking service	0.8	71.1	1.3
Child day care services	0.7	71.8	0.9
Real estate, including real estate-insurance offices	0.7	72.5	0.6
Motor vehicles and motor vehicle equipment	0.7	73.2	0.5
Department stores	0.7	73.9	0.9
Miscellaneous manufacturing industries	0.6	74.5	0.3
Manufacturing industries, N.S	0.6	75.1	0.8
Automobile parking and carwashes	0.6	75.7	0.4
Printing, publishing, and allied industries, except newspapers	0.6	76.3	0.4
Machinery, except electrical, N.E.C	0.6	76.8	0.4
Miscellaneous wholesale, nondurable goods	0.6	77.4	0.2
All other indsutries	22.6	100	24.2
Total	100		100

Notes: The table shows the share of likely undocumented Mexican immigrants work in each of the industries. We follow Borjas and Cassidy (2019) to categorize whether a migrant is documented vs undocumented. Data source: ACS.

Table B.2: Summary statistics for U.S. counties (pre-period avg.)

	Count	Mean	Sd	Min	Max
Matricula cards (flows)					
Number of new applications	120	2,799	5,957	46	50,485
Number of renewals	120	437	1,081	2	7,614
Population					
Likely undoc. Mexican immigrants	120	19,720	44,768	409	397,209
Mex borns	120	28,484	65,534	977	591,153
Non-Mex foreign-born	120	34,057	69,152	362	578,355
Natives	120	144,642	182,474	17,950	1,395,687
High school dropouts (natives)	120	19,087	26,026	2,120	199,922
Emp. rate					
Mexican-born	120	0.67	0.068	0.46	0.82
Non-Mex foreign-born	120	0.68	0.064	0.48	0.89
Natives	120	0.73	0.052	0.53	0.83
High school dropouts (natives)	120	0.52	0.083	0.29	0.72
Unemp. rate					
Mexican-born	120	0.08	0.046	0.00	0.25
Non-Mex foreign-born	120	0.07	0.032	0.00	0.22
Natives	120	0.09	0.027	0.04	0.19
High school dropouts (natives)	120	0.20	0.065	0.04	0.38
Employed persons					
Mexican-born	120	18,564	42,118	691	377,353
Non-Mex foreign-born	120	23,001	45,347	272	378,449
Natives	120	104,900	128,658	13,366	964,286
High school dropouts (natives)	120	9,355	11,860	1,180	93,639
Hourly wage					
Mexican-born	120	11.0	1.5	8.7	16.8
Non-Mex foreign-born	120	16.5	3.3	10.8	27.9
Natives	120	15.3	2.6	10.4	26.1
High school dropouts (natives)	120	10.7	1.4	7.5	14.6
CBP employed persons					
Construction	120	21,449	27,652	1,126	150,241
Eating and drinking places	120	25,388	35,632	2,187	272,654
Services to buildings	120	5,309	7,621	98	43,383

Notes: The table presents the summary statistics for the county-level data used in the analysis. Number of Matricula cards are calculated using 2005 and 2006 averages. and the ACS and CBP statistics are 2000, 2005, and 2006 averages. In the underlying ACS individual-level sample, we include individuals between 18 and 35 years old. Hourly wage is calculated using the reported income wage and hours of work among those who are employed, and is then winsorized at 1 and 99 percent level. Three industries in the CBP data are based on NAICS code system: construction (NAICS 23), food service and drinking places (NAICS 722), and services to buildings and dwellings (NAICS 5617).

Table B.3: Summary statistics in 2012

	Obs	Mean	Std. dev.	Min	Max
Cumulative cards from 2006 to 2012	120	13658.59	26721.67	668	228314
Population (likely undoc. Mex.)	120	13369.19	24216.29	290	188659
Undoc. Mex population/2000 population	120	0.07	0.04	0.01	0.23
Population (natives)	120	169054.3	223981.1	17950	1781501
Population (high school dropouts)	120	17147.02	24044.63	1272	187678
Employment rate (natives)	120	0.686	0.064	0.514	0.808
Employed persons (natives)	120	115098.6	148973	12175	1139814
Employed persons (high school dropouts)	120	6662.1	9148.586	602	69692
CBP payroll employees (construction)	120	15527.74	20426.79	650	132840
CBP payroll employees (eating and drinking places)	120	29606.97	41479.32	2445	312237
CBP payroll employees (services to buildings)	120	5500.133	7634.906	115	40398
Business applications	120	8318.183	12118.71	413	91387

Notes: This table presents the county-level summary statistics in 2012.

Table B.4: The impact of inflows on labor market by education group (natives only)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Medium run (2011–2014)				**Longer run (2016–2019)**			
	High school dropouts	High school grads	Some College	Bachelor's and up	High school dropouts	High school grads	Some College	Bachelor's and up
	Panel A: Employment rate (county wide)							
$RelNewCard_{ct}^{post}$	0.0058*	0.0028**	0.0031***	-0.0001	-0.0003	0.0005	0.0006	-0.0010
	(0.0032)	(0.0013)	(0.0011)	(0.0007)	(0.0029)	(0.0010)	(0.0009)	(0.0007)
	Panel B: Wages (county wide)							
$RelNewCard_{ct}^{post}$	0.0020	0.0030	0.0011	0.0009	0.0015	0.0007	0.0013	-0.0003
	(0.0033)	(0.0019)	(0.0019)	(0.0013)	(0.0027)	(0.0021)	(0.0020)	(0.0017)

Notes: This table presents the estimates for α_1 in Equation 3 but pooling multiple years together. We first calculate county-level averages for each of the variables for three time periods: 2005–2006, 2011–2014, 2016–2019. Then we estimate Equation 3 using 2005–2006 as pre-period, and the other two periods as post periods. The independent $RelNewCard_{ct}^{post}$ represents the difference between the inverse hyperbolic sine (IHS) transformed and BH-method demeaned predicted new card applications from the PANwin ($CNC_{ct}^{WinXPost}$) and the PANlose ($CNC_{ct}^{LoseXPost}$) municipios post inauguration. Standard errors are clustered by county.