The Local Reaction to Unauthorized Mexican Migration to the US

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Preliminary draft. Comments are welcome.
The latest version is available here.
The appendix alone is available here.

Abstract

We study the political impacts of unauthorized Mexican migration to the US. We exploit exogenous variation in migrant networks from data on over 7 million likely unauthorized migrants who obtained consular IDs. We find evidence of conservative electoral and policy responses. Unauthorized migration significantly increases the vote share of the Republican Party in federal elections, decreases spending on education, and increases relative spending on policing and on the administration of justice. We show that migrants have concentrated impacts on poverty and on employment in "migrant-intensive" sectors. They also drive changes in the composition of the electorate, both causing out-migration and increases in in-group values of the remaining residents. By contrast, unauthorized migrants have no discernible impact on wages or unemployment. Despite the investment in policing and courts, migrants do not impact crime, but they increase police use of deportation programs. Together, our findings suggest that narrow economic losses and underlying population change account for some of the observed political response. Commonly discussed mechanisms do not fully explain our findings.

Keywords: unauthorized migration, political economy, public expenditures, elections

JEL Codes: D72, J61, J15

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1 Introduction

Political backlash of citizens against migrants from developing countries is well documented (Alesina and Tabellini, 2021; Rodrik, 2021; Rozo and Vargas, 2021; Mayda et al., 2022).[1] While not new (Tabellini, 2020; Alsan et al., 2020), this backlash has gained attention because of media bias (Couttenier et al., 2021) and its connection to the rise of right-wing populism (Edo et al., 2019; Halla et al., 2017; Barone et al., 2016; Harmon, 2018; Otto and Steinhardt, 2014). Most contemporary evidence comes from Europe, focuses on refugees, and is limited to electoral responses and civic attitudes. This study expands on such literature by estimating the US citizen's reactions to unauthorized Mexicans, the largest and most politically salient group, and by exploring the economic, social, and ideological channels behind these responses. Namely, we document how this migration has shaped local politics in the US and how it has done so.

Mexican emigres to the United States make up the world's largest national diaspora in a single country. As of 2020, nearly 11.4 million Mexican-born persons lived in the US, and an estimated 4.5 million of them had irregular migration status (Passel and Cohn, 2018; Gonzalez-Barrera, 2021; United Nations Department of Economic and Social Affairs, 2021). The labor market impacts of Mexican unauthorized migration are generally small and limited to a few sectors (Hanson, 2009; Monras, 2020). However, there is little systematic evidence of their political and social impacts.

Selection bias is a central challenge to estimating political and social effects of unauthorized migrants. This challenge has been compounded by the lack of appropriate data. There are no direct records of newly arrived unauthorized Mexican migrants to the US. Rather, the literature has traditionally relied on proxies from survey data, like ACS (Borjas and Cassidy, 2019), and on apprehensions at the border (Hanson and Spilimbergo, 1999). These approximated samples are probably not representative, as unauthorized migrants may avoid being counted for fear of legal consequences. Moreover, Mexican migrants, like any

[1] However, see Hill et al., who document less reactionary behavior.

migrant, choose particular locations because of political and social context, making simple cross-section comparisons biased. To address these concerns, several recent articles have used an administrative data set on consular identification cards issued by the Mexican government to nationals living in the United States (Allen et al., 2018; Caballero et al., 2018; Bhandari et al., 2021; Dinarte Diaz et al., 2022). While the data set does not explicitly sample unauthorized migrants, consular IDs are useful exclusively for them—as we demonstrate in Section 3.1.3. The consular IDs provide geographic specific data. The structure helps to address selection. With this granularity, we leverage historical networks to predict current day migrant flows.

We use a confidential version of this data set with the novel feature of being able to track individuals over time. Our data contains information on Mexican municipality of origin, US county of residence, gender, age, marital status, and educational attainment for 7.4 million people (14.5 million observations) from 2002 to 2020. Such variation in time and geographic cross-section, along with the stable migration patterns documented elsewhere (Durand et al., 2001; Munshi, 2003), allows us to predict Mexican migration as fraction of county population. In particular, we instrument migration using a leave-one-out shift-share strategy. Namely, we multiply the share of migrants from Mexican municipality M living in US county C—calculated using the first five years of the data—by the number of unique new migrants from municipality M that arrived in the US in subsequent years, net of those that actually established residence in the core-based statistical area (CBSA) of said county; this is the leave-one-out component. Since we are interested in measuring the effect of relative influxes of migrants, we also divide such instrument by a measure of predicted county population, created by projecting population in C using growth rates of similar counties in other regions of the country. Namely, we leverage on geographic variation of consular data to create an instrument that works around selection bias.

Our identification assumption is that the predicted number of migrants impacts the outcomes of interest only by its effect on observed migration. We assume that the US county-level characteristics that attracted Mexicans from particular municipalities in earlier decades do not affect the evolution of economic, political, and social characteristics of the county today. Despite reflecting historic migration patterns, we claim that our initial shares are unrelated to the evolution in our outcomes of interest (Goldsmith-Pinkham et al., 2020; Borusyak et al., 2022). We interrogate this assumption by looking for pre-trends and differential trends. To account for the possibility of non-random exposure to migration, we implement the correction proposed by Borusyak and Hull (2020). The results are robust to a number of our attempts to examine the identifying assumption.

We study the effects of migration on voting and explore consequences and mechanisms. Our primary study is the effect of unauthorized migration on vote shares in House and presidential elections. We find that unauthorized migration increases the vote share for Republicans across House, Senate, and presidential elections. The effect is largest for midterm House elections. In our main specification, a one standard deviation increase in newcomers increases the Republican vote share in midterm House elections by 0.26 standard deviations, an impact large enough to alter some elections. Our results reinforce that voters have a heightened response to unauthorized migrants, similar to (Mayda et al., 2022; Alesina and Tabellini, 2021) in the US.

Once we establish the conservative electoral response to unauthorized migration, we ask whether this shift alters policy. We study the impact on local public goods expenditure. Following Alesina et al. (1999), Hanson et al. (2007), Facchini and Mayda (2009), Card et al. (2012), Hainmueller and Hopkins (2014) and Alesina et al. (2022), we expect that larger flows of migrants will decrease the provision of public services due to coordination failures and out-group bias. Indeed, we identify that migration reduces absolute expenditures in education and increases absolute and relative expenditures in policing and on the administration of justice. This finding is consistent with Derenoncourt (2022), who documents similar effects following the Great Migration. In our main specification, a one standard deviation increase in newcomers decreases the expenditures in education by 0.10 standard deviations and increases the share on policing and the administration of justice by 0.15 and 0.21 standard deviations.

Last, we explore prominent explanations for right-leaning reactions to unauthorized migrants. We document concentrated economic impacts at the bottom of the distribution and changes in the underlying county population. In our main specification, a standard deviation of newcomers decreased employment in construction by 0.08 standard deviations and employment in hospitality and leisure by 0.03 standard deviations, and increased the poverty rate by 0.16 standard deviations. We also find quantitative and qualitative changes in the electorate. As a response to migration, counties adopt less universalist (more communal) moral values—standardized coefficient of 0.17. Namely, people are less supportive of ideas and policies that benefit everyone, regardless of specific characteristics of affiliations, which maps well with right-wing preferences (Enke et al., 2020). Also, we document that counties with more migrants observe a decline in total population—standardized coefficient of 0.01—almost entirely explained by out-migration—standardized coefficient of 0.09. We find no evidence of impacts on crime, GDP per capita, median household income, unemployment or total employment. Further, we find limited evidence that participation in a deportation program becomes more targeted, suggesting that the police response may not be simply anti-migrant.

We advance the understanding of migration and political effects in several ways. We provide causal estimates of the local electoral and fiscal responses to the largest diaspora in the US and one of the most politicized group of migrants. Our data set allows us to isolate the impacts of unauthorized migrants. Previous studies have measured the impacts of "low-skill" vs. "high-skill" migration Mayda et al. (2022). However, to our knowledge, none has specifically studied the country-wide impact of unauthorized migration. We document the different channels that drive such response. The mechanisms are relevant by themselves, as they portray a more holistic picture of the newcomers in their communities of arrivals. We find limited impacts on economic and social (criminal indicators), essential to counter anti-migrant rhetoric. These results are helpful to identify potential losers from this particular migration in order to compensate them. We take on the suggestion of Alesina et al. (2022) and specifically study how migration shapes moral values. We demonstrate that value change is an important explanation for the political effects we observe. Last, the estimated impacts for the political effects are considerably larger than those of the mechanisms. Thus, this research suggests that the reaction is not fully explain by the broad set of mechanisms explored.

In the remainder of the paper, we describe the background of Mexican migration and review existing theory for how migrants influence policy (Section 2). Then we present our novel dataset and demonstrate how it is apt for our question of interest (Section 3). We explain our shift-share instrument and examine the key identifying assumption (Section 4). We demonstrate that flows of migration drive an increase in vote share for the Republican party and shifts public spending consistent with fiscal conservatism (Section 5). We explore a host of mechanisms that explain the rightward shift and highlight concentrated losses, demographic change, and a shift in values among the electorate. We investigate robustness and discuss implications.

2 Background

2.1 The Political Effects of Immigration

A growing literature in political economy documents political backlash to globalization (Rodrik, 2021), including immigration (Alesina and Tabellini, 2021). This literature establishes that economic shocks, along with cultural and social conditions, lead voters to support more conservative parties and policies. Historically, in the US, internal migration generated an out-group reaction —migration out of cities (Boustan et al., 2010; Boustan, 2010; Derenoncourt, 2022). Currently, among migrants in the US, perhaps none are more politically contentious than those who are unauthorized. However, scholars have yet to isolate and examine these dynamics regarding unauthorized migration in the US.

An extensive literature describes the demography of this population and analyzes the labor market impacts. Wassink and Massey (2022) describe the policy context and demographic consequences of the post-2000 immigration regime. While the number of new unauthorized Mexican migrants has declined in recent years, large flows still persist. Wage differentials between the US and Mexico remain a compelling explanation for the flows (Hanson and Spilimbergo, 1999). In the labor market, scholars have found null overall impacts on wages and on unemployment, but small negative impacts in certain sectors and regions (Hanson, 2009; Monras, 2020; Clemens et al., 2018). Nevertheless, the political and social effects of these flows have not been widely analyzed.

Three recent articles explore contemporary political responses to migration in the US. Two focus on the impact of groups larger and less politicized than unauthorized migrants. Mayda et al. (2022) study the impact of "high-skilled" and "low-skilled" immigration on political outcomes and find that "high-skilled" immigration shifts voters to the Democrats, while "low-skilled" immigration shifts voters to the Republicans. In contrast, Hill et al. (2019) studies the impact of Hispanic population and non-citizen immigrants on political outcomes during the 2016 presidential election. They find these groups shift the electorate to the left. The final article studies unauthorized migration in Georgia. Baerg et al. (2018) observe that counties with higher fractions of unauthorized immigrants in the state of Georgia tend to vote more Republican.

Data limitation and selection bias have hampered scholars' efforts to understand unauthorized Mexican migrants and their impacts in the United States. A few articles have addressed these challenges using consular identification cards issued by the Mexican government to nationals living in the United States to address these challenges. We build on these scholars' work. Their papers leverage the data on municipalities of origin and place of residence to estimate the impact on wages of building border walls and fencing (Allen et al., 2018), to more accurately model the size and geographic characteristics of the unauthorized population in the United States (Caballero et al., 2018; Bhandari et al., 2021), and to identify the dynamics of remittances during the COVID-19 pandemic (Dinarte Diaz et al., 2022). Using similar fine-grain data we are able to explore other impacts of the unauthorized Mexican population.

While work on contemporary migration in the US does not explore political and social impacts, historical studies provide some evidence. Work in comparative political economy shows that waves of immigration from the early 1900s had positive impacts on long-run economic prosperity (Sequeira et al., 2020), but these waves were met with more conservative politicians and policies (Tabellini, 2020). Evidence also supports that there are specific winners and losers with global flows. The positive economic effects of immigrants on US counties since 1850 depend on the cultural, political, and economic characteristics of the immigrants' places of origin (Fulford et al., 2020). Current global flows continue to generate winners and losers, and some regions bear disproportionate costs (Autor et al., 2016). Similar to much of the existing literature, we find evidence of county-level political shifts to the right.

2.2 Explaining Political Shifts

We identify three prominent sets of explanations in the literature for this shift to the right (Alesina and Tabellini, 2021; Rodrik, 2021; Hanson, 2009). The first explanation is that migration, although economically positive overall, generates some losers. Migrants will compete with natives with similar skills (Card, 2005; Monras, 2015; Cortes, 2008; Burstein et al., 2020). Competing natives experience higher unemployment or lower wages. Politicians may play to the worse-off group of voters by promoting policies against migrants.

The second explanation relates to heterogeneity. By virtue of their otherness, migrants may trigger out-group responses. The reasons behind this may be economic, since natives would prefer lower redistribution to ethnically different people (Alesina et al., 1999; Alesina and Giuliano, 2009). They might be demographic, since natives want to preserve the current composition of their communities (Card et al., 2012). Last, they might be political, since natives may want to preserve their power in a polarized environment (Bazzi et al., 2019).

Flows of unauthorized migrants would make it harder to deliver public goods, either because coordination is more difficult or because preferences for redistribution change. In the context of US democratic institutions, this may be reflected in voters' preferences for policies associated with the Republican party: lower taxes and lower government spending.

The third explanation has to do with attitudes and perceptions. Natives, driven by political entrepreneurs or the media (Couttenier et al., 2021), may assign negative characteristics to migrants (Hainmueller and Hopkins, 2014; Alesina et al., 2022; Facchini and Mayda, 2009). The ideas that migrants threaten natives, increase crime, or do not contribute economically to their place of residence lead citizens to vote for anti-migrant politicians and policies. Rozo and Vargas (2021) demonstrate how politicians can use these (mis)perceptions of migrants strategically to gain office.

3 Data

3.1 Consular Data

Since the mid-1800s, the Mexican government has offered identification cards to its citizens living in the United States (Laglagaron, 2010; Márquez Lartigue, 2021). With the Patriot Act, requirements for identification became more stringent in the United States, and so migrants without work authorization had even more limited access to US-issued identification cards (Bruno and Storrs, 2005). Without identification, these individuals are virtually unable to access some basic services, such as banking or housing (Mathema, 2015). Mexican Consular Services responded to more stringent identification requirements by upgrading the identification available to Mexican nationals in the United States. The updated administrative database is the source of our data. Because migrants with valid visas or work authorization have access to identification from US authorities, the working assumption among scholars using this data is that it captures fundamentally unauthorized migrants (Massey et al., 2010; Bhandari et al., 2021; Caballero et al., 2018).

3.1.1 Data Context

In 2002, the Mexican government strengthened the requirements to obtain an ID. Before then, the identification was a piece of paper. The new (current) consular card, called "Matrícula Consular de Alta Seguridad," is a formal plastic card with several authentication mechanisms (Bruno and Storrs, 2005; Massey et al., 2010). Every Mexican person, regardless of age, is eligible to get an ID. To obtain one, a person must show proof of residence and nationality and pay a fee of US 35$. IDs are valid for five years, and the renewal process is identical. Importantly, there are no immigration status requirements. The 2002 upgrade to consular IDs was part of a larger effort of the Mexican Consulate to help Mexican nationals get banked and remit more efficiently. This service is central to consular activities, so much so that most of the personnel working in the consular network are employed issuing either passports or consular IDs. Further, to facilitate getting IDs, Mexican state governments have established offices near many consulates to help people retrieve birth certificates. Letters from churches can serve as proof of residence. While there is no data, it is generally agreed that virtually everybody with the necessary documentation is able to get the ID.

The Mexican government, therefore, has data on the municipality and date of birth, marital status, educational attainment, sector of employment, and US county and state of residence of cardholders. The National Institute of Mexicans Abroad (IME) intermittently publishes aggregated versions of this data.[2] However, the aggregated dataset does not show specific people, nor does it allow construction of Mexican municipality-US county pairs. The Mexican Ministry of Foreign Affairs (SRE) has shared with us a confidential, detailed version of the dataset. It contains all the demographic information, except name, of every Mexican national who got an ID between 2002 and 2020. The SRE created an identification number that allows us to track people over time. This number has no relevant meaning nor is linked in any form to other demographic information. The data consists of 16.7 million observations

[2] At the time of writing this paper, the official site had inconsistent links to download the data. Here is the link for the 2018 information https://www.gob.mx/ime/acciones-y-programas/estadisticas-de-matriculas-de-personas-mexicanas-en-estados-unidos-2018

corresponding to 8.8 million individuals.

3.1.2 Constructing a Measure of Newcomers

There are two main challenges to estimating the number of newcomers from our data. It is necessary to (1) differentiate between renewals and first-timers and (2) to make assumptions about the likelihood that an average newcomer applies for an ID and remains in the same county. We have taken the following approach. First, we count the number of new cards per person in 4-year intervals, 2007–10, 2011–14, and 2015–18. [3] This frequency is convenient because it allows us to observe the compounded number of migrants during an election year. Moreover, there is evidence that cardholders tend to be newly arrived migrants, that the majority of newly arrived Mexican migrants obtain a card over a five-year period, and that cardholders tend to remain at least in the same state over those five years (Allen et al., 2018; Caballero et al., 2018; Massey et al., 2015). Second, for each one of these periods, we classify as newcomers the people who got an ID for the first time in a new core-based statistical area (CBSA), a geography that encompasses several counties. The strategy, as opposed to counting solely the people who got an ID for the first time, considers migrants moving from one CBSA to another as newcomers. Third, we count only the observations with complete and consistent information regarding place of birth and county of residence. We estimate that there were 2.13 million newcomers in 2010, 1.3 in 2014 and 0.95 in 2018. As Appendix B shows, these figures are consistent with Passel and Cohn (2018). Unauthorized Mexican migration has decreased consistently since its peak in the early 2000s.

To calculate the fraction that the migrants represent in every county, we divide the total number by the county population in the final year of the period—e.g. 2010, 2014, and 2018—using population estimates from the US Census. Figure 1 shows the national distribution of the fraction of unauthorized Mexican migrants for each period. The average is 0.69% for the first period, 0.4% for the second, and 0.28% for the third. During the time of our study,

[3] We exclude the years 2002–2006, as we use them to create the shares for our instrument.

there were at least 10 migrants in 2,674 US counties,[4] around 88% of all US counties.

Recent unauthorized Mexican migrants as share of county population

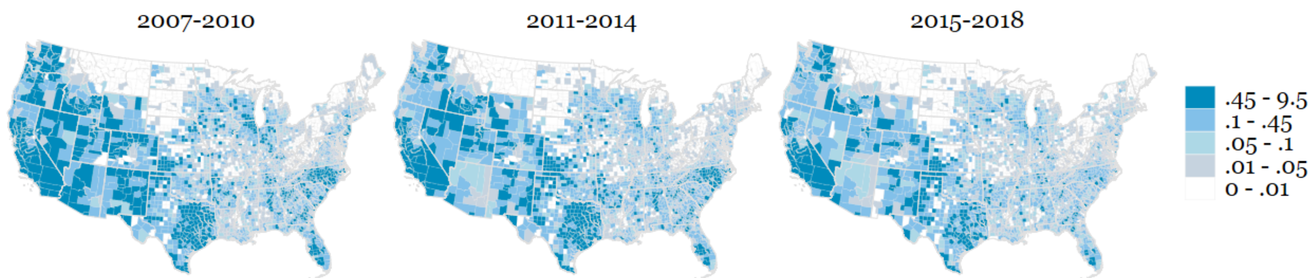

Figure 1: Map of observed number of newcomers. Sources: SRE and US Census Bureau, Population Division

3.1.3 Evaluating the Consular ID Data

There is evidence that the consular dataset captures unauthorized migrants well. Caballero et al. (2018) compare the log number of cards issued in each state between 2006 and 2010 with the log estimated number of Mexican-born residents obtained from the 2010 and 2011 American Community Surveys (ACS). Their R^2 is over 0.97. We carry out a similar analysis using ACS-5 2006–10, 2010–14 and 2014–18. Following the Allen et al. (2018), we consider a likely unauthorized newcomer Mexican migrant in the ACS-5 those people born in Mexico, with no US citizenship, with no college education, and who have been in the US for less than 4 years. Figure 2 plots the log of likely unauthorized migrants from our data and ACS-5–ACS. Our correlation coefficient is 0.82. The association is considerably weaker in areas with few migrants, probably due to low precision from ACS-5. Further, Appendix C compares key demographic variables of 441 counties covered by ACS-5 and the consular data and finds no significant differences.

Selection bias at the county level is the main threat to the validity of this data as a metric of unauthorized migration flows across counties and years. The potential problem

[4] To protect people living in areas with a very low number of migrants, we only consider counties with more than 10 migrants from 2002 to 2020. We also do not consider Alaska because the number and names of counties have changed significantly during our period of study.

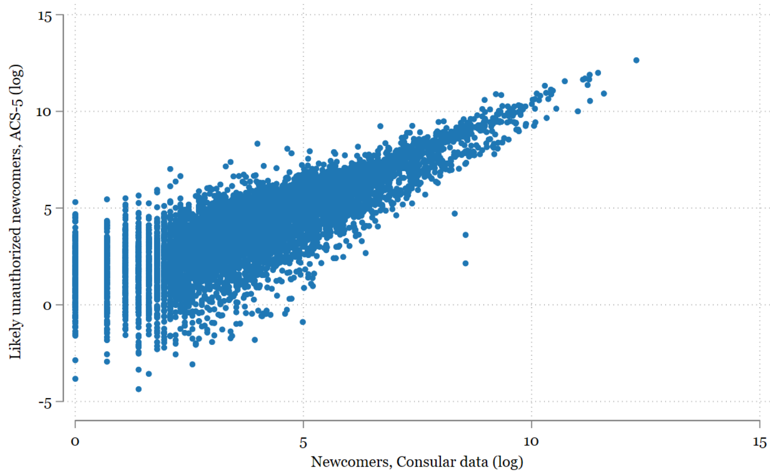

Figure 2: Correlation between ACS-5 and Consular Data

is that unauthorized migrants in counties that change their policy environments could have a stronger (or weaker) incentive to request an ID. Our assumption is that migrants get consular IDs to access basic services, like banking or housing, and to send remittances to Mexico, almost regardless of the policy environment in their county of residence. We explore this assumption by observing the evolution of IDs after some states made driver's licenses and non-driver IDs available to unauthorized migrants. The evidence suggests that within our units of analysis—4 years—policy changes do not consistently alter selecting into our dataset. Our specification controls flexibly for state by period fixed effects, so within period, shocks will not bias our results.

State governments can offer diver's licenses and IDs that are not for federal identification purposes to unauthorized migrants (Mathema, 2015). As of 2018, 12 states and DC allowed unauthorized migrants to get a diver's license (NCSL Immigrant Policy Project, 2021), as compared with only 3 before 2012. Following Callaway and Sant'Anna (2021), we use event-study to test whether states that modified their regulations observed an uptick in consular cards issued. Figure 3 shows the evolution of consular ID take-up by quarter from 2013 to 2016. A jump lasting three quarters, a time frame much shorter than what we are studying, starting before the policy went into effect, is evident. The "pre-treatment effect" is probably explained by the announcement of the program, whereas the short-lived "post-treatment effect" suggests that individuals may have waited a little longer to get their consular ID, until after the states' policies were implemented.

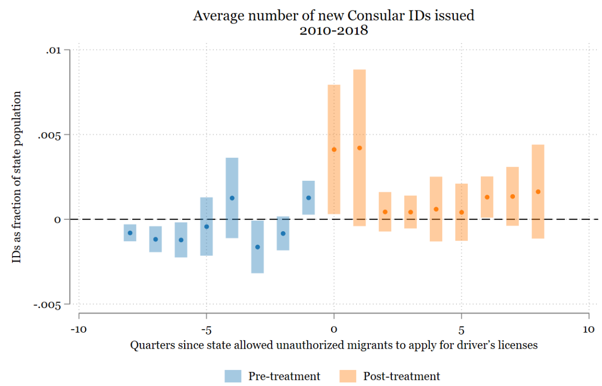

Figure 3: Driver's license

In Appendix D, we follow East et al. (2022) and Alsan and Yang (2019) and carry out an analogous exercise after the activation date of Secure Communities, a program where local police submit individuals to federal authorities for deportation review. We fail to observe significant changes. Collectively, these two results suggest that demand for consular IDs is rather inelastic to the local policy environment in the medium term. Getting a consular ID is not only important to carry out regular tasks in everyday life, but also a common, almost habitual task that migrants do.

3.2 Dependent Variables

We use two sets of dependent variables in our primary analysis. First we examine the impact of migrants on the local results of federal elections. We examine whether US voting shifts to the right using Republican vote share in Congressional and presidential elections. To examine public good provision, we use county-level revenue and expenditures and focus on spending in public education, policing, and the judiciary. We describe features of key variables here and include summary statistics in Appendix E.

3.2.1 Electoral

The electoral data comes from Dave Leip's Atlas of US Presidential Elections. It lists the number and share of votes obtained by each of the two major US parties in every county for all federal elections: House, Senate, and presidential. We are interested in studying the effect of migration during midterms and presidential years. For midterms, we analyze the elections of 2010, 2014, and 2018. For presidential year election, we analyze 2012, 2016, and 2020.

3.2.2 Public Expenditures

Public expenditure data comes from the Annual Survey of State and Local Government Finances. Conceptually, we use this data to investigate policy selection at the local level. We are interested in all local policy, not just for the county. Therefore, we aggregate all local expenditures within each county. This includes spending by the county government, cities, and townships, special districts, and independent school districts. Expenditure codes are consistent across these five types of agencies and facilitate a simple summation across government entities.

By far, the largest expenditure item at the local level is education. On average, in our sample, 40% of the total direct expenditures within counties are for education. Other "productive goods and services," like sewage and highways, represent 3% and 4% respectively. We study the effect of migration on total direct expenditures, direct expenditure in education, and direct expenditure in police protection and administration of justice. Since *a priori* it is ambiguous whether migration affects absolute or relative expenditure, we explore both. The absolute expenditures are the log of total expenditures per capita, in 2010 thousand dollars. For this and all other per capita measures, we use US Census data for county population. The relative expenditures for education, police, and justice, are the shares they represent of total direct expenditures.

One shortcoming of this dataset is that, except for school districts, it surveys all the counties only in years that end in 2 and 7. For the rest of the years, the estimates are based on a sample of around 15% of the total number of local agencies, all of which are in counties of more than designate population—county governments of more than 100,000 people in 2010, for instance (Annual Survey of State and Local Government Finances, 2010). We use data for 2012 and 2017. We estimate the effect of newcomers in period 2007–2010 on expenditures in 2012 and of newcomers in period 2011–14 on expenditures in 2017.

4 Empirical strategy

A simple comparison between counties with more and fewer unauthorized migrants would provide biased estimates. The number of migrants that counties receive is not random. For example, migrants may select into places that are more economically promising or more friendly toward migrants. To address this bias, we use a shift-share strategy. We use a leave-one-out shift-share instrument, and interact pre-period shares with observed migration flows excluding the area of interest.

Equation 1 details the second stage estimation:

$$Y_{cst} = \beta_0 + \beta_1 Recent\widehat{Mex}Migrants_{cst} + \psi_c + \eta_{st} + \epsilon_{cst} \tag{1}$$

where Y_{cst} are the outcomes of interest for county c in US state s during the 4-year period t. β_1 is the effect of the predicted unauthorized Mexican migrants as share of predicted population. ψ_c are county fixed effects and η_{st} are state-period fixed effects.

Equation 2 is the first stage of this estimating equation:

$$RecentMexMigrants_{cst} = \gamma_0 + \gamma_1 Z_{cst} + \phi_c + \pi_{st} + u_{cst} \tag{2}$$

where Z_{cst} is the shift-share leave-one-out instrument and ϕ_c are county fixed effects and π_{st} are state-period fixed effects.

This empirical strategy is inspired by Tabellini (2020). The first step is to construct the endogenous variable, the observed number of migrants, the way we described in Section 3.1.2. We count the unique new consular IDs in every US county during each of the three 4-year periods 2007–10, 2011–14, and 2015–18.[5]

To estimate the predicted number of migrants, we create pre-period shares using the first five years of data (2002–2006). We count all the individuals that got a consular ID in every county C in this five-year period—following the same rejection rule regarding the CBSA duplication. We decompose this total number of migrants by county according to their municipality of origin M in Mexico. Migrants from our sample come from 2,449 municipalities, over 99% of the total. Then, we add up the migrants from each municipality living in all US counties during that period. Finally, we calculate the share of those migrants from municipality M that lived in each US county C. Thus, our initial shares are the proportion of migrants from municipality M who live in county C. For example, we counted 585 people from Alvarado, Veracruz, in the US from 2002–2006. Among them, 9.2% lived in Los Angeles County, CA, 7.5% in Ventura County, CA, and 5.8% in Milwaukee County, WI.

Next, we aggregate all the migrants from municipality M that received a new ID in each of the following 4-year periods. We then multiply the original fraction of migrants from municipality M living in county C by the total number of migrants that entered the US during that period, net of those that eventually settled in that county's CBSA. There are a few counties that do not belong to any CBSA. For those, we only leave out the county itself. The product of the initial share and the new flow, leaving out the CBSA, is our leave-one-CBSA-out shift-share. For example, we count 550 people from Alvarado in the US between 2007 and 2010; 52 settled in Los Angeles' CBSA, 21 in Ventura's and 93 in Milwaukee's. Thus, the predicted migration in each county is 46 ($0.092\times(550–52)$), 39.8 ($0.075\times(550–21)$), and 26.6 ($0.058\times(550–93)$).

[5]To ensure uniqueness we drop likely change of address IDs in the same period. That is, when individuals get a new ID in the same period and a different county of the same CBSA, we cannot rule out a simple change of changed address. Therefore, we drop these records.

Last, we scale the leave-one-CBSA-out shift-share by predicted population of the county. We use predicted population since the presence of unauthorized migrants could affect the population of the county. We follow Tabellini (2020) and calculate the predicted population by multiplying the population of the county in 2006 times the population growth of similar counties in terms of the urban-rural classification in other regions of the US. Formally, the instrument is given by Equation 3.

$$Z_{cst} = \frac{1}{\widehat{P}_{cst}} \sum_{m} Sh_{mcs,2006} * O_{mt}^{-cbsa} \tag{3}$$

where $\widehat{P}_{cst}$ is predicted population, Sh fraction of migrants from Mexican municipality m in US county c in US state s during the pre-period 2002–2006. O_{mt}^{-cbsa} is the total migrants from municipality m in period t that migrated to the US, net of those that migrated to county's CBSA.

In line with recent developments in two-stage least squares (2SLS) literature (Blandhol et al., 2022), our preferred specification does not control parametrically for covariates. We only include county and state by period fixed effects.

4.1 Identifying Assumptions

To provide causal estimates, at least one of the components of shift-share designs must be exogenous (Borusyak et al., 2022). Given that we have panel data, and exploit only within county variation, the exogeneity of the shift-share in our setting relates to changes, rather than levels. We argue that, while our initial shares (fraction of people from Mexican municipality M living in US county C) reflect historic linkages between US cities/counties and Mexican municipalities/states (Durand, 2016), they are likely exogenous to our variables of interest. We also claim that the shifters are exogenous; our assumption is that by excluding the CBSA of the county of interest, the constructed shock is uncorrelated with any unobserved factors in the residuals. A key component of this assumption is that the number of migrants are not spatially correlated among CBSAs (Borusyak and Hull, 2020).[6]

The main identifying assumption of shift-share designs with panel data is analogous to the parallel trends assumption of difference-in-differences estimators (Goldsmith-Pinkham et al., 2020; Cunningham, 2021). We assume that the observed effects in the variables of interest are solely due to the instrument via the endogenous variable. The argument is that, conditional on county and state by period fixed effects, predicted migration affects the evolution of electoral and policy outcomes only through observed migration.

There are two main threats to identification. One, our results would be biased if counties that received more Mexican newcomers were already on a different political and socioeconomic trend from those that received fewer Mexican newcomers. This would occur if either the variables of interest or other key regressors would be on different trajectories or if the initial shares had persistent effects. This would be a violation of the parallel trends-like assumption.

Two, our results would be biased if counties were non-randomly exposed to migration shocks. This would be the case if simultaneously a) the Mexican municipal shares—i.e., share of people from municipality M living in US county C—between counties were markedly different, b) the composition of the Mexican shares was correlated with our variables of interest, and c) the migration patterns between municipalities changed significantly during the period of study. To illustrate, assume that the people from northern Mexico would have stronger networks in more conservative US counties and the people from southern Mexico, with comparable population, would have stronger networks in more liberal counties. Further, assume that the migration from northern Mexico increased during our period of study and migration from southern Mexico decreased. As a result, liberal counties would receive fewer Mexican migrants. This scenario represents the non-random exposure to shocks described by Borusyak and Hull (2020).

[6]The potential for spatial correlation is the reason we leave out the entire CBSA, not only the county itself. In Appendix F we show that, while there is some spatial auto-correlation in the number of newcomers among counties (Moran's I of between .44 and .3), the correlation among CBSAs is significantly lower (Moran's I of between .21 and .18).

We conduct the following checks to provide evidence against both these concerns. First, we test for pre-trends by analyzing the association between the instrument and the lagged outcomes. We find a statistically significant correlation only one lagged outcome, providing support for the parallel-trends assumption. Second, we test for differential trends by interacting key pre-period characteristics with period indicators. Our baseline results are, for the most part, robust to these controls; key regressors do not seem to have evolved along predicted unauthorized migration. Third, we implement the correction proposed by Borusyak and Hull (2020) to deal with possible non-random exposure. This is, we control for a constructed counterfactual instrument.[7]. Controlling by this simulated variable is also useful to test whether the results are solely driven by the initial shares. Our main results are largely unaffected. All these results are displayed in Table 9. Finally, we analyze concentration of migrant networks by county. Predicted Mexican migrant composition in counties is not excessively concentrated. The top 50 sending municipalities account for a little over 30% of predicted migrants. On average, each of these top 50 municipalities provide only around 2.5% of the total predicted migrants per county, but have migrants living in over 700 counties. The average county has predicted migrants from around 650 municipalities, out of which the top 20 per county provide on average 85% of the total.

4.2 First Stage

The stability of the migration patterns results in a strong first stage. As Column 2 of Table 1 suggests, conditional on county fixed effects and state by period fixed effects, a 1 percentage point increase in the instrument is associated with a 1.16 percentage point increase in the observed share of newcomers. The F-stat of our instrument is 822.

For comparison purposes, Table 1 presents four instruments, from strongest to weakest.

[7] To obtain the simulated instrument, we average 2,000 instruments created by interacting 2,000 randomly permuted shifters with the original shares. To illustrate, from the total 585 migrants from Alvarado in the 2002–06 period, 9.2% lived in Los Angeles County, 7.5% in Ventura County and 5.8% in Milwaukee. The shifter for each of these counties—created via the described LOO—in the period 2007–10 was 46, 39 and 26. In each simulation, we use instead any other of the over 300,000 shifters from that period—say 2,300 and 15.

Column 1 shows the results for the least conservative instrument. This is almost an identical instrument to the one described before; however, it leaves out only the county of interest rather than the CBSA. This results in the strongest first stage with an F-stat of 1024. The instrument in Column 3 leaves the whole state out.

Table 1: First stage

	(1) LOO county out	(2) LOO CBSA out	(3) LOO state out	(4) Push factors shift-share	(5) Push factors only	(6) Municipality county links
Newcomers, percent population	1.12*** (0.03)	1.16*** (0.04)	1.32*** (0.06)	1.26*** (0.34)	0.60*** (0.10)	0.23*** (0.05)
Observations	8019	8019	8019	8019	8010	8019
F statistic	1024	822	488	14	34	27
Mean of Dep. Var	0.463	0.463	0.463	0.463	0.463	0.463
Mean of Ind. Var	0.421	0.404	0.318	0.417	0.468	0.470

Column 1 displays the results for a leave-one-out (LOO) shift-share regressor that leaves the country itself out. Column 2 displays results for a LOO shift-share regressor that leaves the CBSA out. Column 3 displays results for a LOO shift-share regressor that leaves the state out. Column 4 displays results for a shift-share regressor that predicts yearly migration by municipality using push factors like homicide rates, economic activity, and variation in temperature. Column 5 displays results for an instrument constructed by multiplying the predicted municipal migration, as in Column 4, by the share of migrants from that municipality living in that county in that period. Column 6 uses all the Mexican municipal variables to predict county-municipality migration patterns per period. Standard errors clustered at the county level except for column 2, which are at the CBSA level. All estimations control for county and state-year fixed effects. *p<0.05,**p<0.01,***p<0.001

Column 4 displays the results of a more demanding shift-share instrument. We use time-variant characteristics of the municipality of origin as shocks to predict the annual number of migrants. For this prediction exercise we employ a Lasso Poisson regression to select among a dozen of potential regressors. The final set of variables includes a metric of social development, total value added, deviations from historic precipitation and temperature, homicide rate, total investment, production per capita, employees per capita, and hours worked per capita. This instrument has an F-stat of 14, but is still strong enough to conduct analysis. The instrument in Column 5 uses the same set of push factors, but does not rely on historic shares. Namely, it interacts predicted migration from push factors with contemporaneous (period-by-period) shares. Finally, the instrument in Column 6 uses these push factors to predict migration at the US county-Mexican municipality level. This final instrument does not rely on shares at all, but rather aims to predict a larger set of migration flows every period.

Throughout the rest of the paper, we use the leave-one-CBSA-out instrument as our preferred specifications. Results are similar with the other two LOO instruments. The fact that leaving out the county, CBSA, or state delivers similar impacts reveals that the the bulk of the variation we leverage comes from the initial shares. Furthermore, Appendix N carries out all the analysis using the instrument from Column 4. The main estimates remain statistically significant.

5 Main Results

In this section we examine the impact of unauthorized Mexican migration on voting, and then we respond to two subsequent questions: Can migrants affect the outcome of elections? Do migrants affect the policy delivered? Consistent with much of the work reviewed in Alesina and Tabellini (2021), unauthorized migrants shift a county's vote share toward the right. In House elections, these impacts are large enough that the flows of unauthorized migrants could have changed the outcomes of close elections. Unauthorized migrant flows prompt local spending consistent with the fiscal conservatism and the law-and-order policies of the Republican party.

5.1 Voting Behavior

We begin by examining voting behavior across a series of specifications. Qualitatively, many of the specifications suggest that unauthorized migrant flows drive greater vote share for the political right. Table 2 displays estimated impacts of unauthorized migrant arrivals on Republican party vote share in House and presidential elections. We estimate midterm and presidential years separately. Estimating midterm and presidential years separately reveals a pattern consistent with aggregate outcomes in the elections studied. In each of the presidential elections studied, the Democratic candidate won the majority of the vote, and in two of the three midterm elections, Republicans made gains in the House.

Throughout the article, we interpret effects in terms of mean flows of unauthorized migrants. Substantively, mean flows are quite small. In a county of a 100,000 people they translate into an average of 101 people annually (404 per 4-year period, assuming a uniform distribution over time). This quantity helps us to compare the political and policy behavior of a county in the presence of a mean flow of unauthorized migrants to the absence of that flow. To facilitate interpretation, we present coefficients for our estimated effects with corresponding clustered standard errors. Below these, we report standardized coefficients and effects at the mean ($\hat{\beta}\bar{x}$). The standardized coefficients are useful for comparing magnitude across models. However, they largely capture cross sectional variation, and counties are unlikely to move a standard deviation in flows of unauthorized migrants. The impact at the mean thus provides a more informative measure for policy.

The baseline OLS estimates, in Panel A, show that there is a statistically significant, positive relationship between unauthorized migration and Republican vote share. The coefficients present a pattern that is consistent with the causal estimates, too. The House midterm relationship is the largest (Columns 1 and 2). A mean flow of unauthorized migrants is associated with 2.67 point increase in the share of votes that go to Republicans. Presidential year relationships are smaller in magnitude, both for the House of Representatives (Columns 3 and 4) and for the President (Columns 5 and 6). Finally, our weighted and unweighted estimates seldom differ statistically. We focus on population weighted estimates throughout the remainder of the article because these estimates are often more precise and robust than the unweighted estimates.

The 2SLS estimates in Panel B are one of three LATEs in Table 2. They are from our preferred specification. We use the leave-one-CBSA-out shift-share (LOO CBSA) instrument which follows Tabellini (2020) and captures migration that is due to the unauthorized migrant network of the early 2000s. Migration flows that are due to the migrant network

Table 2: Effects of arrival of unauthorized Mexican migrants on GOP vote shares, 2010–2020

	House, Midterm		House, Pres. Year		President	
	(1) Weight	(2) Un-weight	(3) Weight	(4) Un-weight	(5) Weight	(6) Un-weight
A. OLS						
Newcomers, pct. pop.	6.51***	5.25***	2.82**	3.25***	3.19***	3.17***
	(0.87)	(0.70)	(1.06)	(0.65)	(0.65)	(0.45)
B. 2SLS, LOO-CBSA						
Newcomers, pct. pop.	8.49***	9.18***	3.49**	4.93***	4.42***	5.51***
	(1.03)	(1.05)	(1.21)	(0.94)	(0.71)	(0.70)
Std. Coefficient	0.26	0.23	0.10	0.12	0.16	0.17
$\hat{\beta} * \bar{X}$	3.93	2.52	1.61	1.35	2.12	1.57
C. 2SLS, shift-share push						
Newcomers, pct. pop.	11.42**	10.91***	0.29	5.36**	2.45	4.67***
	(3.67)	(2.20)	(4.86)	(1.81)	(1.82)	(1.24)
Std. Coefficient	0.35	0.27	0.01	0.13	0.09	0.14
$\hat{\beta} * \bar{X}$	5.29	2.99	0.14	1.47	1.17	1.33
D. 2SLS, county-municipio						
Newcomers, pct. pop.	1.81	2.61	0.95	1.11	-0.76	2.20
	(2.96)	(1.78)	(3.69)	(1.72)	(1.83)	(1.23)
Std Coefficient	0.06	0.06	0.03	0.03	-0.03	0.07
$\hat{\beta} * \bar{X}$	0.84	0.72	0.44	0.30	-0.36	0.63
Observations	7995	7995	8015	8015	7236	7236
Dep. Var., Mean	48.16	61.70	47.24	63.79	45.83	61.54
Dep. Var., Sd	19.44	18.14	19.92	19.14	16.39	15.52

Dependent variables are share of Republican vote. Source: Dave Leip's United States Election Data. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population as described in Section 3. Panel A displays the results for OLS estimator. Panel B displays results for a LOO shift-share regressor that leaves the CBSA out, our preferred specification. Panel C displays results for a shift-share regressor that predicts yearly migration by municipality using push factors like homicide rates, economic activity and variation in temperature. Panel D uses all the Mexican municipal variables to predict county-municipality migration patterns per period. Standard errors clustered at the CBSA level. All estimations control for county and state-year fixed effects. *p<0.05,**p<0.01,***p<0.001

drive responses larger in magnitude than OLS estimates suggest. In the House midterm elections, a mean flow of unauthorized migrants causes a 3.93 point increase in vote share for Republican candidates (Column 1, std coeff: 0.26). In presidential years, a mean flow of unauthorized migrants causes a 1.39 point increase in vote share for Republican House candidates (Column 3, std coeff: 0.10) and a 1.86 point increase for the Republican presidential candidate (Column 5, std coeff: 0.16). When unauthorized migrants choose their location because of past networks, US natives respond by voting more conservatively.

The 2SLS estimates in Panel C are from the shift-share instrument that leverages shocks in Mexican municipalities. This LATE captures the impacts of migrants who arrive because of shocks in Mexico and the migrant network. Generally, the estimates from this instrument are substantively similar to those from the LOO CBSA instrument. The instrument is sufficiently strong to conduct analysis. However, it is much weaker than the LOO instrument. Therefore, results are less precise. A mean flow of migrants (moved by shocks and migrant networks) causes a 5.29 point increase in Republican vote share (Column 1, std coeff: 0.35). With this instrument, the weighted impacts in presidential years are imprecise (Columns 3 and 5); however, the unweighted estimates are substantively similar and statistically indistinguishable from the estimates in Panel B (Columns 4 and 6).

The last LATE we examine is based on an instrument that leverages shocks in Mexican municipalities and the distance between Mexican municipalities and US counties. This instrument is not a shift-share instrument and does not use the existing shares of unauthorized Mexican migrants to predict flows. It captures migrants driven by shocks in Mexico alone. Like the instrument in Panel C, this instrument is strong enough to conduct analysis, but relatively weak compared to the LOO CBSA instrument. It yields estimates that are not precise and substantially smaller than those from the other instruments. Still, the estimates point in the same direction.

Collectively, these results suggest that migrant flows driven by the existing migrant network prompt more conservative voting. These effects are consistent with historical findings and findings from other countries receiving migrants. We estimate multiple LATEs to get a sense of the distribution of causal effects. Not all unauthorized migrant arrivals are met with conservative reactions. The LOO CBSA compliers, those who arrive by virtue of a migrant network from decades earlier, generate these conservative reactions. We use these LOO CBSA compliers, throughout the remainder of the paper to explore consequences of and explanations for the conservative reaction to unauthorized Mexican migration.

For each analysis in the remainder of the paper, we present three sets of estimates. In Panel A, we show OLS estimates for a baseline comparison. Panel B displays second stage estimates and is our preferred specification from Equation 1. Panel C displays reduced form results, the effect of the instrument on the outcome of interest. Since our instrument is closely correlated in our first stage, we use the reduced form estimates when we investigate robustness and heterogeneity.

5.2 Election Outcomes

Even though the impacts on Republican vote share are large, it is not clear from these estimates that average flows of migrants will alter the outcome of any House election or the composition of the House. It is possible that our mean effect is coming from already secure Republican counties, since the average vote share for Republicans across counties is already near 60%. To examine this question, we estimate the impact of unauthorized migrants on midterm House elections across two different distributions. We create five categories of counties: first, we categorize according to the Republican vote share in the 2006 House election; second, according to the closeness of the 2006 election. Figure 4 presents the coefficients from the 2SLS estimate (analogous to Table 2, Panel B, Column 1) scaled by the mean flow for each group.

The left graph displays the results by 2006 Republican margin. On the left is the effect of newcomers on Republican vote share in counties that Republicans lost by the 35

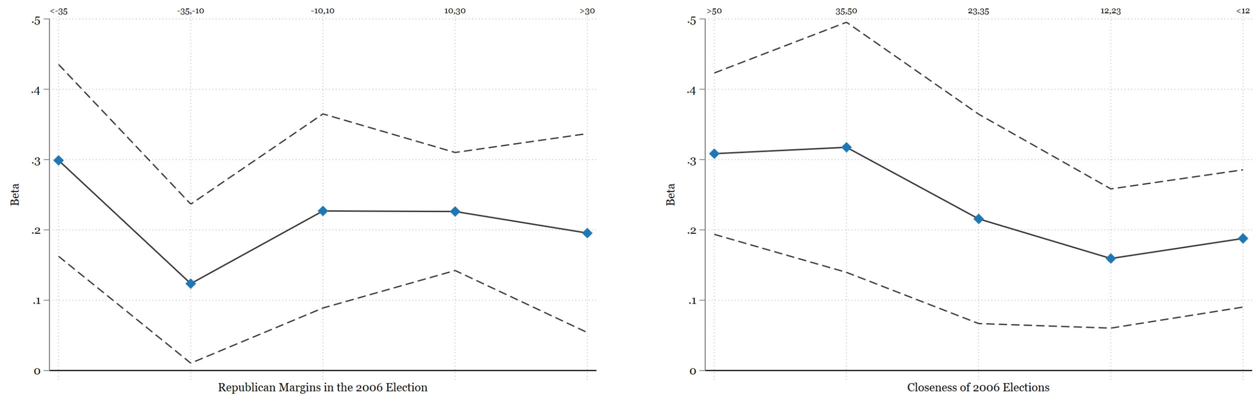

Figure 4: Regressions across bins of counties by electoral results in 2006

points or more: Democratic strongholds. Moving right, the estimates reflect the effect of new unauthorized migrants on Republican vote share where Republicans have done better. While none of the estimates are statistically distinguishable from the others, we find that, if anything, counties that are Democratic strongholds or close calls are more responsive to migrants than Republican strongholds. This makes it more likely that flows of unauthorized migrants have changed close elections.

In the right graph, the left side are the counties where either party won by a landslide in 2006. In each successive quintile, the electoral margin narrows. The results are similar. None of the estimates across the bins are statistically distinguishable from the others. At the extreme, close elections are more responsive on average to the arrival of new migrants. In House elections that are closer than a margin of 2 or 3 points, a mean flow of migrants can change the outcome in favor of the Republican candidate.

5.3 Policy Change

Even though, voters are changing their behavior in federal elections, this may be driven by national narratives. Changes in federal elections may not be changing local policy. To explore policy changes in response to unauthorized migration, we study county-level public expenditures. Examining public spending gives us leverage on two questions of interest.

First, it allows us to explore whether the inflow of new unauthorized migrants creates a reduction in the provision of local public goods. Second, we are able to explore whether the changes in public spending are consistent with a party that is more fiscally conservative, opposes redistribution, and focuses on law and order. We find evidence of reallocation in public expenditure consistent with more conservative policy. In response to new unauthorized migrants, counties spend less in total dollars per child on public education (log), and they increase the proportion of spending on police and the court system. Namely, local politicians respond by limiting spending on public education and investing in security.

When we investigate public-goods provision by studying county-level expenditure, we find evidence that unauthorized migrants decrease revenue and spending, suggesting that the right-leaning shift may operate through heterogeneity and redistribution. We find stronger evidence of reallocation, though. In response to migrants, local spending is reallocated away from public education, which may be construed as local redistribution or suggestive of compositional amenities. Reallocation goes toward police and the judiciary. These findings are consistent with the logic that responses to migrants are operating through threat, an explanation we explore further below.

Results in Table 3 present a pattern similar to that of the impact on voting. The OLS estimates provide a baseline which suggests a bias toward zero (Panel A), consistent with the expectation that migrants self-select into more economically promising counties. Only the relationship between arrivals and education spending is statistically significant. Second stage (Panel B) and reduced form (Panel C) estimates are larger in magnitude, and most are precisely estimated.

Local revenue and direct expenditure go down in response to unauthorized migration consistent with conservative policy and the preference for less redistribution. The second stage estimates (Panel B) suggest that a mean flow of unauthorized migrants reduces revenue by 2 percentage points (Column 1, std coeff: -0.05) and reduces direct expenditure by 2

Table 3: Public spending effects of arrival of unauthorized Mexican migrants 2012 and 2017

		Expend (log pc 2010 USD)				Share of Direct Expend		
	(1) Revenue	(2) Direct exp	(3) Educ	(4) Police	(5) Judicial	(6) Edu	(7) Police	(8) Judicial
A. OLS								
Newcomers, pct. pop.	-0.02	-0.02	-0.03*	0.02	0.08	0.32	0.20	0.13
	(0.01)	(0.01)	(0.01)	(0.02)	(0.06)	(0.42)	(0.12)	(0.09)
B. 2SLS								
Newcomers, pct. pop.	-0.03*	-0.04**	-0.05**	0.04*	0.15*	0.23	0.42**	0.26**
	(0.01)	(0.02)	(0.02)	(0.02)	(0.07)	(0.61)	(0.14)	(0.10)
Std. Coefficient	-0.05	-0.07	-0.10	0.06	0.12	0.01	0.15	0.21
$\hat{\beta} * \bar{X}$	-0.02	-0.02	-0.03	0.02	0.08	0.13	0.23	0.15
C. Reduced form								
Instrument	-0.03*	-0.05**	-0.06**	0.05*	0.18*	0.27	0.49**	0.31**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.08)	(0.71)	(0.17)	(0.12)
Std. Coefficient	-0.06	-0.08	-0.11	0.06	0.13	0.01	0.17	0.23
$\hat{\beta} * \bar{X}$	-0.02	-0.03	-0.03	0.03	0.10	0.15	0.27	0.17
Observations	5340	5340	5330	5338	5296	5330	5338	5296
Dep. Var., Mean	1.57	1.54	1.97	-1.43	-2.95	40.88	5.45	1.40
Dep. Var., Sd	0.39	0.39	0.34	0.50	0.88	11.59	1.85	0.85
Ind. Var., Mean	0.55	0.55	0.55	0.55	0.55	0.55	0.55	0.55
Ind. Var., Sd	0.68	0.68	0.68	0.68	0.68	0.68	0.68	0.68
Inst., Mean	0.49	0.49	0.49	0.49	0.49	0.49	0.49	0.49
Inst., Sd	0.63	0.63	0.63	0.63	0.63	0.63	0.63	0.63

Dependent variables in columns 1–5 are in log 2010 dollars per capita, except education (column 3) which is per child (population under 19). Dependent variables in columns 6–8 are shares of total direct expenditures. Revenue includes taxes, intergovernmental revenue, current charges, and miscellaneous general revenue. Direct Expenditure includes spending on public education, policing, and health, as well as other categories as described in section 3. Education expenditures include all public education expenditures of the county. Police expenditures include city police spending in a county, as well as sheriff department spending and local incarceration at county jails. Judicial expenditure includes all county expenditures on the administration of justice, including prosecutors, public defense, judges, court administration, and expenses related to the civil court system. Source: Annual Survey of State and Local Government Finances. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population as described in Section 3. Source: SRE. Instrument is as described in Section 3. All regressions have period and county fixed effects. Standard errors are clustered at the CBSA level. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

percentage points (Column 2, std coeff: -0.07) as well.[8]

The decrease in revenue and spending prompts reallocation across local public goods. A mean flow of newcomers prompts a 3% reduction in spending in public education (Column 3, std coeff: -0.10). The same flow prompts an increase in relative share of spending on police and the administration of justice. Police spending as a share of direct expenditure increases by .23 percentage points (Column 7, std coeff: 0.15). Judicial expenditure goes as a share of direct expenditure increases by .15 percentage points (Column 8, std coeff: 0.21).

Since we analyze multiple public expenditure variables, we carry out a Holm correction for multiple hypothesis testing. With a 0.05 significance level, we can reject the null hypothesis of 4 of the 8 tests, for police share, education (log per child), direct expenditures (log per capita), and judicial share— with a p-value of 0.009, less than Holm's benchmark of 0.01 for the 4th/8 test. In the remaining analysis, we consider these four effects statistically significant.

Our findings on public spending are consistent with both the ethnic heterogeneity (Alesina et al., 1999) and compositional amenities (Card et al., 2012) theories. Unauthorized migration causes divestment in the largest "productive expenditure" that local governments control, education. It also increases relative investment in policing and administration of justice. While these results do not help to disentangle the relative importance of redistribution or compositional amenities, we observe a change that reflects less redistribution and greater protection of property rights.

6 Mechanisms

We now turn to possible explanations for the conservative electoral and policy response to migrants. We interrogate existing explanations for the backlash. We consider four sets of explanations in turn. The movement to the right may be driven by economic losses,

[8]Since we study only two periods (2007–10 and 2011–14), the mean flow we interpret is larger, 0.55 new unauthorized migrants as a percentage of predicted population.

and right-leaning politicians' willingness to blame the migrants for those losses. Migration might be driving underlying population change, changes in the electorate, or the values of those voting. Finally, migrants may be generating this response because of increase in crime or the demand for more deportation. This last set of explanations may further explain the increase in spending on the police and judiciary. Altogether, we find evidence of concentrated economic losses and changes in the underlying population. Our findings are consistent with explanations for conservative reactions that rely on economic loss, out-group bias, and status threat.

6.1 Employment and Wages

We consider whether economic losses in employment and wages can explain the conservative shift. Labor market theories of the conservative electoral reaction suggest that migrants decrease employment and wages among similarly skilled US Natives, and politicians promise anti-migrant policy to attract those who lost in the labor market. Like much of the existing literature on labor and immigration, we document small changes in the labor market outcomes as a results of unauthorized migrants. Unauthorized migration reduces employment in construction and hospitality. Further, there is some evidence that natives may move from these migrant-intensive industries to manufacturing, which is less accessible to unauthorized migrants. We find no effect on wages for all sectors, except agriculture, another migrant-intensive industry. The small magnitude of the labor market effects likely does not explain most of the political response.

To measure the economic effects of migrants, we focus on several indicators. Similarly, the Quarterly Census of Employment and Wages (QCEW) reports the annual average employment and weekly wages for multiple sectors and super-sectors. We examine total average annual employment and wages and break out the super-sectors of construction, manufacturing, leisure and hospitality, and agriculture (which also includes forestry, fishing, and hunting). The employment variables are constructed as the log per working-age population (ages 15–64, US Census). Wages correspond to 2010 dollars.[9]

Table 4 presents the results on employment. Panel A displays the same pattern for the baseline OLS estimates, generally biased toward zero and in this case, less precise. Substantively, the impact on employment and wages is modest. A mean flow in unauthorized migrants decreases employment per working age person by 2% in the construction industry (Panel B, column 2, std coeff: -0.07)) and by 1% in hospitality and leisure (Panel B, column 4, std coeff: -0.03). At the same time, mean migrant flows increase employment in manufacturing, suggesting some industry change among native workers, who are likely advantaged in manufacturing. A mean flow of newcomers increases manufacturing employment by 3% (Panel B, Column 3), leaving the main effect on total employment a precise zero (Panel B, column 1). Thus, there is substitution; unauthorized migration decreases formal employment in construction and hospitality and increases formal employment in manufacturing.

The results for construction are consistent with the literature and the dynamic of this particular sector. Day labor in construction is often readily available to Mexican newcomers. Contingent work has low barriers to entry, and Mexican communities often use informal organization to facilitate day labor, which is disproportionately in construction (Valenzuela, 2003). Our interpretation of the data recorded by QCEW is that they capture informal employment less well, and formal employment in construction is a fraction of the actual construction jobs. Hospitality may be similar. Thus, since wages do not change in these sectors, it may be simply that the decline in employment reflects loss of formal employment by US natives and gain of informal employment by arriving unauthorized migrants.

Table 5 displays the results of the analysis on weekly wages. The baseline OLS estimate in Panel A suggests little relationship between migrant arrivals and weekly wages. The 2SLS estimates and reduced form estimates (Panels B and C) reveal negative impacts on agricultural wages. A mean flow of migrants drives agricultural wages down by $22.07 weekly (Panel B, Column 5, std coeff: -0.15). This result however, is among the less robust results

[9] Since they are normally distributed, we study them in levels.

Table 4: Effect of arrival of unauthorized Mexican migrants on employment among working age population 2010–2018

	Employment, (log per working age pop)				
	(1) Total	(2) Constr	(3) Manufact	(4) Hosp and leis	(5) Agric
A. OLS					
Newcomers, pct pop.	0.01	-0.03	0.07***	-0.01*	0.03
	(0.01)	(0.02)	(0.02)	(0.01)	(0.06)
B. 2SLS					
Newcomers, pct pop.	-0.00	-0.05**	0.08***	-0.02*	-0.09
	(0.01)	(0.02)	(0.02)	(0.01)	(0.08)
Std. Coefficient	-0.00	-0.07	0.06	-0.03	-0.04
$\hat{\beta} * \bar{X}$	-0.00	-0.02	0.03	-0.01	-0.05
C. Reduced form					
Instrument	-0.00	-0.06**	0.09***	-0.02*	-0.11
	(0.01)	(0.02)	(0.02)	(0.01)	(0.09)
Std. Coefficient	-0.00	-0.08	0.06	-0.03	-0.04
$\hat{\beta} * \bar{X}$	-0.00	-0.03	0.04	-0.01	-0.05
Observations	8009	7464	7433	7919	4516
Dep. Var., Mean	-0.67	-3.62	-3.07	-2.75	-6.57
Dep. Var., Sd	0.34	0.45	0.74	0.43	1.53
Ind. Var., Mean	0.46	0.46	0.46	0.46	0.52
Ind. Var., Sd	0.59	0.59	0.59	0.59	0.63
Inst., Mean	0.40	0.41	0.41	0.40	0.47
Inst., Sd	0.55	0.55	0.55	0.55	0.59

Dependent variables are the log of average annual employment divided by working age population. Sources: Quarterly Census of Employment and Wages. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population as described in Section 3. Source: SRE. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

in our analysis. In part, this may be due to the large number of temporary (authorized) workers in the agriculture sector.

Table 5: Effect of arrival of unauthorized Mexican migrants on weekly wages 2010–2018

	Weekly Wages (2010 USD)				
	(1) Total	(2) Constr	(3) Manufact	(4) Hosp and leis	(5) Agric
A. OLS					
Newcomers, pct pop.	3.98	-14.35	12.37	-4.83	-18.30
	(14.27)	(9.58)	(24.00)	(4.45)	(12.19)
B. 2SLS					
Newcomers, pct pop.	-1.78	-22.58	16.02	-8.06	-42.74*
	(18.66)	(12.82)	(33.34)	(6.44)	(19.38)
Std. Coefficient	-0.00	-0.06	0.03	-0.04	-0.14
$\hat{\beta} * \bar{X}$	-0.82	-10.47	7.43	-3.73	-22.07
C. Reduced form					
Instrument	-2.06	-26.18	18.57	-9.35	-49.93*
	(21.62)	(14.80)	(38.74)	(7.42)	(22.42)
Std. Coefficient	-0.00	-0.06	0.03	-0.04	-0.15
$\hat{\beta} * \bar{X}$	-0.95	-12.14	8.61	-4.32	-25.79
Observations	8009	7464	7433	7919	4516
Dep. Var., Mean	873.54	992.99	1121.38	366.09	610.58
Dep. Var., Sd	261.64	223.18	355.71	120.17	198.48
Ind. Var., Mean	0.46	0.46	0.46	0.46	0.52
Ind. Var., Sd	0.59	0.59	0.59	0.59	0.63
Inst., Mean	0.40	0.41	0.41	0.40	0.47
Inst., Sd	0.55	0.55	0.55	0.55	0.59

Dependent variables are the annual average weekly wages in 2010 USD. Sources: Quarterly Census of Employment and Wages. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population as described in Section 3. Source: SRE. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

Together, these findings are consistent with literature on the impact of unauthorized migrants on labor market outcomes. Scholars generally find small reductions in wages and employment, often limited to a few immigrant-intensive sectors (Hanson, 2009; Monras, 2020). These wage and employment decreases are hardly the magnitude of decreased wages associated with other global flows (Autor et al., 2016). However, they do demonstrate that there are some economic losers in counties that receive unauthorized migrant flows, and these individuals are not compensated accordingly. Those with economic losses may well account for some voters punishing pro-immigrant politicians.

6.2 County Welfare

Broader economic and demographic impacts can explain more of the conservative response to unauthorized migration. We document evidence suggesting greater inequality due to unauthorized migration. Migrants increase poverty, while GDP per capita and median household income are unaffected. These findings suggest migrants may drive inequality and are compatible with threat explanations of conservative reactions to migrants.

Yearly unemployment rates are calculated for every county by the Local Area Unemployment Statistics (LAUS) program. County GDP figures are published by the Regional Economic Accounts of the Department of Commerce's Bureau of Economic Analysis. Our outcome is the log per capita of the figures in 2010 dollars. Finally, the poverty rate and median household income data, which we log as well, comes from US Census Bureau, Small Area Income and Poverty Estimates (SAIPE) Program. For all of these variables, we use data from 2011, 2015, and 2019, the year after the end of our periods.

Table 6 presents the results of our analysis on a set of welfare indicators—GDP per capita, median household income, unemployment, and poverty. While we have no county-level measure of inequality, the economic indicators together inform us about inequality and help to establish the possibility of status threat.

The baseline OLS estimates in Panel A follow a similar pattern as in most of our analysis. OLS is generally biased toward zero, and the 2SLS and reduced form estimates (Panels B and C) are larger in magnitude. There is no effect on GDP per capita (Column 1), median household income (Column 2), or unemployment (Column 3). The poverty rate increases. We explore poverty before and after the midterm elections, and impacts go up with more time. A mean flow of unauthorized migrants increases the poverty rate in the county by 3%

Table 6: Socioeconomic effects of arrival of unauthorized Mexican migrants 2010–2018

	County Economy (log)				
	(1) GDP pc	(2) Median household income	(3) Unemployment rate	(4) Poverty rate before midterm	(5) Poverty rate after midterm
A. OLS					
Newcomers, pct. pop.	-0.01	-0.02	0.01	0.04**	0.06***
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
B. 2SLS					
Newcomers, pct. pop.	-0.02	-0.02	0.04	0.06***	0.09***
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Std. Coefficient	-0.03	-0.05	0.05	0.09	0.14
$\hat{\beta} * \bar{X}$	-0.01	-0.01	0.02	0.03	0.04
C. Reduced form					
Instrument	-0.03	-0.03	0.04	0.07***	0.11***
	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)
Std. Coefficient	-0.03	-0.06	0.05	0.10	0.16
$\hat{\beta} * \bar{X}$	-0.01	-0.01	0.02	0.03	0.05
Observations	7887	8022	8022	8022	8022
Dep. Var., Mean	3.89	10.88	1.69	2.61	2.59
Dep. Var., Sd	0.44	0.26	0.45	0.39	0.39
Ind. Var., Mean	0.47	0.46	0.46	0.46	0.46
Ind. Var., Sd	0.59	0.59	0.59	0.59	0.59
Inst., Mean	0.41	0.40	0.40	0.40	0.40
Inst., Sd	0.55	0.55	0.55	0.55	0.55

Dependent variables are the log of GDP per capita (in 2012 USD), median household income (in 2010 USD), unemployment rate, and poverty rate one year before the end of the period (one year before midterm election) and one year after the end of the period (the year of the midterm). Sources: Small Area Income and Poverty Estimates (SAIPE) Program; US Department of Commerce: Bureau of Economic Analysis. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population as described in Section 3. Source: SRE. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

(Panel B, Column 4, std coeff: 0.09) before the midterm and increases the poverty rate by 4% (Panel B, Column 5, std coeff: 0.14) following the midterm. This raises the possibility that unauthorized migration may be impacting poverty through less redistributive policy. It may also simply be cumulative.

Together, these estimates mean that the county as a whole is as productive as it would have been without the unauthorized migrants, but the bottom part of the distribution is worse off, suggesting greater inequality. These findings are consistent with employment analysis, but they are substantively larger. The standardized coefficients reinforce this. Increasing poverty and inequality due to unauthorized migration is important for understanding the conservative electoral response. The finding is consistent with economic and status threat explanations for the conservative reaction.

6.3 Composition of the Electorate and Values

We also find evidence that unauthorized migration flows changes the electorate, through population decline and out-migration, and among the remaining, a decrease in universalist values. These findings suggest that those US natives who remain in the county may be more prone to out-group bias and have a preference for less universal redistribution. However, the remaining population does not identify as more Republican or as more conservative as a result of migration.

Preferences might change in response to unauthorized migrants, explaining our demonstrated shift. In response to unauthorized migrants, individuals in the county may become more conservative, identify more with the Republican party, or seek policies that are more restrictive or less charitable. To explore these possibilities we examine the impacts of unauthorized migrants on self-reported measures of party identification, ideology, and preference for universalist values. We find no evidence of changes in party identification or ideology. We do find evidence that people are less inclined toward universalist values. The movement away from universalist values may explain some of the shift in favor of Republicans.

Even if individual preferences for policies do not change in response to flows of unauthorized migrants, shifts in favor of the Republican party can occur because people turn out to vote (or stay home) in response to unauthorized migration. In other words, unauthorized migration may spur more Republican-leaning or conservative voters to show up at the polls. Equally possible is that unauthorized migration may dampen Democrat-leaning or liberal turnout. Finally, voters may be voting with their feet, and moving in or out of the county. On this logic, individuals who are more conservative stay in a county, while individual voters who are more liberal move out. To explore these possibilities, we look at the impacts of unauthorized migration on voter turnout, population, and out-migration.

To study population changes we rely on US Census data. The US Census systematizes data from the American Community Survey on county-to-county demographic flows. We construct out-migration rates by dividing the out-migration by county population and then taking the natural logarithm. We use the data from 2007–2011, 2011–2015, and 2015–2019.

There are few sources of values, ideas, and opinions for all of US counties for our decades of study. For party identification and ideology, we use the Cooperative Congressional Election Study (CCES), currently known as the Cooperative Election Study (CES). This is a survey of political ideas and behaviors. The cumulative data-set contains 557,455 observations across 3,079 counties. We use an ideology measure which quantifies political leanings in five categories from very liberal to very conservative. For partisan identity, we use a seven-category measure that ranges from strong Democrat to strong Republican. We adjust using the weights provided by CES, and exclude counties with less than five observations per year, dropping all the "Not Sure" and "Don't Know" responses. For values, we use the county-level index of the relative importance of universalist values created by Enke (2020) from YourMorals.org. The index is available for 2,263 counties for the years 2008, 2012, and 2016. To create it, Enke standardized and scaled the counties' average index by their signal-to-noise ratio. For consistency, we standardized the CCE variables using Enke's procedure as well.

We find changes of individual values, aggregate declines in population, and small declines in turnout in response to new inflows of unauthorized immigrants. Table 7 displays the results. As with the main estimates, OLS (Panel A) is generally biased toward zero. The 2SLS and reduced form estimates in Panels B and C are larger.

Columns 1 to 3 display results capturing preference change. The first two columns capture the impact on self-reported party identification on a 5-point scale, where larger numbers are associated with more Republican identification (Column 1), and self-reported ideology on a 7-point scale, where larger numbers are associated with more conservative ideology. People neither identify with the Republican party more nor become more ideologically conservative in response to new arrivals of unauthorized migrants. The electoral and public spending impacts, thus, occur without partisan or ideological change. Column 3 uses the Enke (2020) index of universal (versus communal) values. A mean flow of new unauthorized migrants, as a percentage of predicted population, decreases universalist values by 0.04 standardized units (Panel B, Column 3, std coeff: -0.17).

In essence, universalist values (and policy preference) are those concerned with all individuals, whereas communal values are concerned only with other individuals known to the respondent or the respondent's community (in-group). Universalist values are more abstract and closer to notions of justice. Communal values are more concrete and closer to notions of tradition and order. Counties become less universalist in response to the arrival of new unauthorized migrants. This result is the most direct indication that some of the shift to the political right occurs because migrants trigger out-group bias and preferences for less redistribution. Although this evidence is based on a smaller subset of counties, the impact is large. The change toward more communal values is consistent with theories that hinge on out-group bias. Ethnic heterogeneity breaks down trust, makes coordination more difficult, and reduces people's interest in universal redistribution (Alesina et al., 1999).

Columns 4 to 6 display results that interrogate whether the underlying voter population in the county is changing. There are some indications of this. Population declines and

Table 7: Values, ideology, and demographic effects of arrival of unauthorized Mexican migrants 2010–2018

	Preferences and moral values (scaled)			Pop (log)	Pc log	
	(1) Republican Identity	(2) Ideology Conservative	(3) Universalist Values	(4) Count	(5) Out migration	(6) Turnout midterms
A. OLS						
Newcomers, pct. pop.	0.03	0.03	-0.09*	-0.02**	0.03**	-0.35
	(0.02)	(0.02)	(0.04)	(0.01)	(0.01)	(0.48)
B. 2SLS						
Newcomers, pct. pop.	0.03	0.02	-0.13**	-0.02**	0.04***	-0.70
	(0.03)	(0.03)	(0.04)	(0.01)	(0.01)	(0.82)
Std. Coefficient	0.10	0.09	-0.16	-0.01	0.08	-0.04
$\hat{\beta} * \bar{X}$	0.01	0.01	-0.06	-0.01	0.02	-0.34
C. Reduced form						
Instrument	0.03	0.03	-0.16**	-0.03**	0.04***	-0.81
	(0.04)	(0.03)	(0.05)	(0.01)	(0.01)	(0.96)
Std. Coefficient	0.11	0.10	-0.17	-0.01	0.09	-0.04
$\hat{\beta} * \bar{X}$	0.02	0.01	-0.07	-0.01	0.02	-0.39
Observations	2209	2209	5802	8022	8020	7667
Dep. Var., Mean	-0.04	-0.03	0.15	12.93	-2.94	48.23
Dep. Var., Sd	0.17	0.16	0.50	1.59	0.28	11.60
Ind. Var., Mean	0.50	0.50	0.47	0.46	0.46	0.48
Ind. Var., Sd	0.61	0.61	0.60	0.59	0.59	0.60
Inst., Mean	0.46	0.46	0.41	0.40	0.40	0.42
Inst., Sd	0.58	0.58	0.56	0.55	0.55	0.57

Dependent variables in columns 1–3 are average county preferences (values, as reported by Enke (2020) in column 3). The values for columns 1–2 are normalized and shrunk following Enke (2020). The dependent variable in column 4 is the log of county population. The dependent variable in column 5 is out-migration, calculated as the log of out-migration divided by county population. The dependent variable in column 6 is turnout in House midterm elections, as share of registered voters. Sources: Enke (2020); Cooperative Election Study; US Census Bureau: Population Division and Small Area; Dave Leip's United States Election Data; US Census Bureau: 2007–2011, 2011–2015, and 2015-2019 American Community Surveys. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population as described in Section 3. Source: SRE. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

impacts on out-migration are significant. Column 4 examines the total population in the county. A mean flow of unauthorized migrants reduces the population of the county by 1% (Panel B, Column 4, std coeff: -0.01). Column 5 examines per capita out-migration. A mean flow of unauthorized migrants increases per capita out-migration by 2% (Panel B, Column 5, std coeff: 0.08). Despite these population changes, there is no change in voter turnout.

The finding on out-migration may complement the shift toward communal values. Work on emigration from developing countries shows that out-migration prompts distrust and makes overcoming collective action problems more difficult (Sellars, 2019). The shift towards communal values in the midst of increased poverty is consistent with accounts in Political Science of how economic insecurity in the face of ethnic heterogeneity and distrust prompts less support for government and government programming (Gest, 2016).

6.4 Crime and Deportation

Increases in crime do not appear to drive the conservative reaction of voters. We find no evidence of change in crime due to the arrival of migrants. We do find suggestive evidence that the local police submit more individuals for federal deportation review in response to unauthorized migration flows. While we cannot evaluate the police and prosecutors' response to migrants more specifically, nor politicians willingness to use misperceptions to gain office, there is little evidence that migrants cause more crime, that more people are being arrested or charged because of the presence of migrants. This collection of findings is evidence against some threat explanations for electoral reactions and evidence in favor of explanations that rely on demand for anti-immigrant policy or deportation.

The perception of immigrants as a criminal threat is widely theorized. Studying crime interrogates whether crime increases in response to unauthorized migrants and whether county officials are reasonable to invest in policing and the judiciary. Our crime information comes from the Jacob Kaplan's Concatenated Files, retrieved from the National Archive of Criminal Justice Data. This unofficial data-set condenses the information of yearly "Offenses

Known and Clearances by Arrest (Return A)" by crime reported by the Uniform Crime Reporting Program Data. We use total crime, all crime included in the violent crime index, and all crime included in the property crime index. Since these crime data are noisy, we aggregate counts for 2010–11, 2014–15, and 2018–19. We construct our measures by dividing the counts by county population and then take the natural logarithm.

Another last explanation for the shifting of votes in favor of the law-and-order party or police and judiciary spending is the demand for deportation of the unauthorized migrants. To examine this account we use the intensive margin of local participation in a federal deportation program called Secure Communities. We describe this program and some of its features in Appendix A. Secure Communities was subject to manipulation at the local level. Therefore, analyzing it allows us to distinguish among explanations of the shift to the political right. While investment in policing and the judiciary in response to the arrival of unauthorized migrants may be about fear (out-group bias), it could also be driven by populist backlash (Barone et al., 2016). If the shift is driven by a populist backlash, we would expect larger efforts to deport the unauthorized migrant population and more extensive use of the Secure Communities program.

We compile aggregated statistics from Secure Communities from October 2008 to September 30, 2013 (ICE 2013). We focus on four outcomes from the statistics. We use fingerprint submissions to capture local inquiries to ICE. Fingerprint matches are the subset of inquiries by local authorities for which ICE determines the individual is deportable. Removals are the subset of matches for which deportation actually occurs. Finally, we calculate the match success rate, which is the ratio of matches to submissions. We find suggestive evidence that deportation becomes more targeted with the arrival of new unauthorized migrants.

While detailed, the data source has a few shortcomings. Because of the timing of available data we can only estimate a cross section. Furthermore, while there is evidence that Secure Communities disproportionately targeted Hispanics, these data do not reflect Mexicans, but migrants of all nationalities who may be deportable. Additional limits and features of this

Table 8: Effects of arrival of unauthorized Mexican migrants on crime (2010–2018) and on immigration enforcement (2008–2013)

	Count by foreign population (log)			Rate	Crime (log pc)		
	(1) Submissions	(2) Matches	(3) Removals	(4) Success	(5) All	(6) Violent	(7) Property
	A. OLS						
Newcomers, pct. pop.	0.48***	0.99***	1.00***	0.52***	-0.01	-0.02	0.01
	(0.10)	(0.11)	(0.13)	(0.05)	(0.02)	(0.03)	(0.03)
	B. 2SLS						
Newcomers, pct. pop.	0.72***	1.32***	1.27***	0.62***	-0.02	-0.01	-0.01
	(0.11)	(0.13)	(0.16)	(0.05)	(0.02)	(0.03)	(0.03)
Std. Coefficient	0.27	0.38	0.33	0.37	-0.01	-0.01	-0.01
$\hat{\beta} * \bar{X}$	0.33	0.62	0.62	0.29	-0.01	-0.00	-0.00
	C. Reduced form						
Instrument	0.77***	1.42***	1.38***	0.67***	-0.03	-0.01	-0.01
	(0.13)	(0.15)	(0.18)	(0.05)	(0.03)	(0.03)	(0.03)
Std. Coefficient	0.27	0.38	0.34	0.37	-0.02	-0.01	-0.01
$\hat{\beta} * \bar{X}$	0.36	0.66	0.67	0.31	-0.01	-0.01	-0.01
Observations	7964	7584	6069	7587	7872	7820	7858
Dep. Var., Mean	7.61	4.16	2.22	1.13	-3.48	-5.87	-3.88
Dep. Var., Sd	1.62	2.08	2.31	1.00	0.94	1.03	0.92
Ind. Var., Mean	0.46	0.47	0.48	0.47	0.46	0.46	0.46
Ind. Var., Sd	0.60	0.60	0.61	0.60	0.60	0.60	0.60
Inst., Mean	0.41	0.41	0.43	0.41	0.41	0.41	0.41
Inst., Sd	0.56	0.56	0.56	0.56	0.55	0.56	0.55

Dependent variables in columns 1–3 are submissions, matches, and removals from the Secure Communities Program. These variables are calculated proportional to the time Secure Communities was in place in the county between 2008 and 2013 and proportional to the foreign population in the county in 2010. Dependent variable in column 4 is success rate–matched/submissions. Dependent variables in columns 5–7 are 2-year averages of the log of per capita total crime, violent crime index, and property crime index. Sources: Jacob Kaplan's Concatenated Files: Uniform Crime Reporting Program Data: Offenses Known and Clearances by Arrest (Return A), 1960–2020; ICE: Secure Communities Monthly Statistics through September 30, 2013. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population as described in Section 3. Source: SRE. Instrument is as described in Section 3. Standard errors are robust for the first four columns and clustered at the CBSA level for the last three. The first four estimations control for state fixed effects and the last three control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

data are discussed in the Appendix D.

Columns 1 through 4 in Table 8 display the results of the analysis on Secure Communities. The baseline OLS estimates in Panel A indicate that the arrival of more unauthorized migrants in a county is associated with a decrease of fingerprints submissions, an increase in matches (with persons in ICE's database) and with subsequent removals (deportations). The second stage and reduced form estimates are generally larger in magnitude (Panel B and C). The second stage estimates suggest that in response to a mean inflow of migrants, police departments increase the number of fingerprint submissions per foreign born population by 33% (Panel B, Column 1, std coeff: 0.27). Furthermore, counties increased the number of matches from ICE by 62% (Panel B, Column 2, std coeff: 0.38), and subsequent removals (deportations) increased by 62%, as well (Panel B, Column 3, std coeff: 0.33). These findings suggest that as more unauthorized migrants arrive in a county, police and sheriff's departments use Secure Communities more often with greater accuracy. Indeed the success rate improves dramatically.

Since these estimates are based on a cross-section, we are hesitant to draw firm conclusions from the analysis. Nevertheless, the evidence does suggest a local approach to using Secure Communities that is actively anti-immigrant. We are exploring additional sources of data to investigate the relationships further.

7 Robustness Checks

Our empirical strategy relies on the assumption that the observed effects in the variables of interest are solely due to the instrument via the endogenous variable. Namely, we assume that counties with more predicted migrants were not already in a different trend due to persistent impacts of shares of the evolution of other observed or unobserved key variables. (Borusyak et al., 2022; Cunningham, 2021; Goldsmith-Pinkham et al., 2020).

We test these hypothesis in Table 9. First, in Panel B, we test for pre-trends by regressing the instrument on pre-period outcomes—lagged 12 years, 3 periods. The values for the midterm elections are results in 1998, 2002, and 2006; for the presidential year elections in 2000, 2004 and 2008; and for the fiscal outcomes, values for 2002 and 2007. Unlike most migration studies that leverage shocks (Rozo and Vargas, 2021; Tabellini, 2020; Sequeira et al., 2020), our setting is characterized by continuity. Migration from Mexico to the US is a century-old phenomenon that saw a consistent increase from the early 1990s until the mid 2000s. Moreover, our variables of interest tend to be persistent and only move marginally in the short run. Reassuringly, however, our instrument is statistically unrelated with all but one variable: vote shares for GOP candidates. The point estimates of the association with the midterm vote share for GOP and all the fiscal variables are close to zero.

Second, we test for differential trends by interacting several pre-period characteristics with period indicators in Panels C–E. The intention is to explore whether the observed effect is being driven by the evolution of key pre-period characteristics rather than by our instrument. Panel E controls by the share of Mexican population without US citizenship in 2000, obtained from the US Census, interacted with period dummies. This control aims to model the evolution of unauthorized Mexicans given the stock Mexican-born residents in 2000. Conditioning on such projection does not statistically alter our results. Panels D and E condition on the rate of high school completion among adults in 2000 and exposure to the the China shock in 2006, constructed with Peter K. Schott's Data, County Business Patterns and Acemoglu et al. (2016) replication files. Neither of two variables alters the magnitude

Table 9: Robustness checks

	Midterms	Pres year		Log pc		Share of Expend	
	(1) House	(2) House	(3) Pres	(4) D. Exp	(5) Educ	(6) Police	(7) Judicial
A. Reduced form, baseline							
Instrument	9.85***	4.04**	5.10***	-0.05**	-0.06**	0.49**	0.31**
	(1.22)	(1.37)	(0.78)	(0.02)	(0.02)	(0.17)	(0.12)
B. Lagged outcome (LO)							
Instrument	0.99	2.91	3.69***	-0.00	-0.02	-0.03	-0.02
	(2.01)	(2.04)	(0.55)	(0.02)	(0.01)	(0.20)	(0.11)
C. Mex non-citizen, sh							
Instrument	9.85***	8.09***	8.26***	-0.04	-0.06*	0.42*	0.33*
	(1.36)	(2.31)	(1.10)	(0.03)	(0.03)	(0.19)	(0.15)
D. Adult HS completion							
Instrument	11.19***	5.64***	7.26***	-0.05**	-0.06**	0.51**	0.32**
	(1.25)	(1.31)	(0.59)	(0.02)	(0.02)	(0.17)	(0.12)
E. China shock							
Instrument	8.41***	2.71	3.54***	-0.05**	-0.07**	0.53**	0.33**
	(1.35)	(1.47)	(0.76)	(0.02)	(0.02)	(0.16)	(0.12)
F. Simulated instrument							
Instrument	9.84***	7.94	7.52***	-0.05	-0.13***	0.43	0.49***
	(2.51)	(4.51)	(1.48)	(0.05)	(0.04)	(0.26)	(0.15)
G. Stock Mex foreign							
Instrument	10.12***	4.91***	5.26***	-0.05*	-0.06**	0.48**	0.31**
	(1.29)	(1.43)	(0.78)	(0.02)	(0.02)	(0.16)	(0.11)
H. No-outliers							
Instrument	11.17***	4.24*	6.08***	-0.06*	-0.06*	0.64**	0.32
	(1.37)	(1.77)	(0.87)	(0.03)	(0.03)	(0.21)	(0.17)
I. LOO-State							
Instrument	11.35***	5.59***	7.09***	-0.06**	-0.06**	0.52*	0.31*
	(1.47)	(1.37)	(0.86)	(0.02)	(0.02)	(0.21)	(0.15)
J. No pop weights							
Instrument	10.69***	5.74***	6.31***	-0.04	-0.06**	0.56**	0.24**
	(1.27)	(1.08)	(0.77)	(0.02)	(0.02)	(0.19)	(0.09)

Dependent variables in columns 1–3 are the vote share for Republicans in different federal elections. Dependent variables in column 4–5 are the log of per capita (per child population in column 4) expenditure. Columns 6–7 are the share of direct expenditure. All estimations are reduced form. Sources: Dave Leip's US Election Data; Annual Survey of State and Local Government Finances; US Census; ACS-5; USDA's Economic Research Service; Peter K. Schott's Data; County Business Patterns; Acemoglu et al. (2016), and QCEW. Panel 1 is reduced form estimation. Panel 2 estimates effect on lagged dependent variables. Panel 3 controls for pre-2007 features interacted with period dummies. Panel 4 includes simulated instrument following Borusyak and Hull (2020). Panel 5 controls for the stock of Mexican non-citizens at the beginning of period. Panel 6 excludes outliers, uses the LOO state instrument, and does not use predicted population weights. Standard errors clustered at CBSA level. Estimations control for county and state-period FE. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

or significance of our results, except for Column 2.

Third, we explore whether migration exposure was non-random. Non-random exposure would occur if migrants from certain Mexican municipalities had simultaneously sorted in politically biased counties and had a different growth rate over the period of study. In Panel F, we implement the correction proposed by Borusyak and Hull (2020). We constructed a counterfactual instrument by taking the average of 2,000 simulated instrument shifters, created by multiplying the original shares by each of 2,000 permutations of the shifters from other county-municipality dyads in the same period. Conditioning on this simulated instrument only changes the results for Column 2.

We also test for a different link between migration and political outcomes. To disentangle the role of stocks versus flows, Panel G explicitly conditions on the estimated share of residents who were born in Mexico the year before our periods started, obtained from ACS-5 2005–09, 2009–13, and 2013–17. While this variable theoretically captures both authorized and unauthorized migrants, it correlates strongly with the instrument (ρ or around 0.86). Conditioning on the share of Mexican-born residents does not change the estimates.

Finally, Panels H–J present estimates excluding outliers (percentiles 1 and 99 of the predicted migration distribution), using a LOO instrument where we leave the whole state out, and without using predicted population weights. The estimates remain consistent. Importantly, the significance of the results with the LOO-state instrument reveals that our results are being driven by the shares, rather than by the shifts. With the inclusion of county and state by period fixed effects, the initial shares are the only difference between counties in the same state. While this could be worrisome, as Panel F indicates, the initial shares by themselves (namely, interacted with simulated shifters) do not alter the significance of the results. Our interpretation is that during the period of study, migration from certain Mexican municipalities increased disproportionately, causing counties with higher initial shares to receive more migrants.

Using both the instrument and the lagged instrument, as recommended by Jaeger et al.

(2018), is intended to explore whether there is dynamic adjustment in the outcomes of interest in the presence of highly serially correlated instruments, like our setting. Their canonical example pertains to the impact of migration across decades (not years) in labor markets (not elections or budgets). The stability of our results across specifications, and their consistency with the literature, hints at a lack of dynamic adjustment. However, we implement the Jaeger et al. (2018) technique in Appendix J. Since we need a measure of lagged instrument, this correction requires us to drop the first period and lose significant power. For the electoral outcomes, that implies analyzing just 2010–14 and 2015–18 and for the public spending results of just the 2017 fiscal year, making the estimate merely a cross-section. Unsurprisingly, the results are inconsistent and do not reflect a clear dynamic pattern.

In Appendix M, we estimate a similar correction to the one proposed by Adão et al. (2019) to account for a potential correlation of the residuals between counties with comparable initial shares. Our confidence intervals are largely unchanged by these methods.

Lastly, Appendix N carries out all the main results using an alternative shift-share regressor. We use the same shares created with the first five years of the data, but instead of calculating the shift through leave-one-out, we predict migrants from every municipality using push factors. In particular, we carry out a Lasso dimensionality reduction technique with more than 20 social, economic, and weather annual variables and then implement a Poisson regression. The variables selected to predict migration are a measure of social development, deviations from the historic rainfall and temperature, homicide rate, total investment, total production per capita, value added per capita, total employees per capita, total economic units per capit,a and total hours worked per capita. The instrument is considerably weaker, F-stat of 34, but it is relevant. In general, as compared with our main instrument, we lose precision—except for GOP vote shares in the midterms and expenditures in police and justice—but the point estimates and their sign are consistent with our main results. This indicates we are estimating similar LATEs.

Importantly, in Appendix K we conduct the same robustness checks on the mechanisms. The results hold even better. They are robust to controlling for differential trends, simulated instrument, the stock of Mexican-born people at the beginning of the period, removing outliers, and not using population weights. All but one (out-migration)[10] complies with the parallel trends assumption. Two of them have an opposite effect: contemporaneous predicted migration is negatively associated with wages in agriculture and poverty rate, suggesting a reversal in the trend.

8 Conclusion

We estimate the impact of recent unauthorized Mexican migration on the political, economic, and social conditions of US counties using a leave-one-out shift-share identification strategy. In response to newcomer migrants, county vote share for the Republican party increased in House, Senate, and presidential elections. Government agencies divested in education and increased relative spending in policing and the administration of justice. We contend that there are two main explanations for this political and policy response. Migration created concentrated economic losses. While GDP per capita and median household income did not change, the poverty rate increased. There were also losers in the labor market. While employment in manufacturing increased in response to unauthorized migration, employment in construction and hospitality decreased. The second reason is that the composition and preferences of counties changed in response to migration. People moved out, population declined, and there was a decline in universalist values. These changes indicate that the remaining population responded to out-group bias and a preference to limit universal redistribution.

Our main results (vote shares for Republicans in the House, divestment in education, and investment in the policing and administration of justice) are robust to conditioning on differential pre-trends, a counterfactual instrument, a proxy of the stock of migrants, not weighting by predicted population, and removing outliers. We do not find a statistically

[10] We do not have lagged values for the variable indicating the relative importance of universalist values.

significant association between migration and lagged outcomes, making the parallel trends assumption more likely. Estimates for our main mechanisms—poverty, employment loss, population, out-migration and universalist values—are similarly robust.

These results contribute to an extensive literature on backlash against migrants from developing countries. While responses to immigrants have been studied in the US, scholars have yet to quantitatively estimate the impacts of unauthorized migrants, the most politicized of migrants. We document responses that are closer to reactions of Europeans to refugees, a group of highly politicized migrants (Barone et al., 2016). Unlike the findings in Rozo and Vargas (2021), we cannot conclude that the response is explained by a radicalization of citizens at the extreme of the political distribution. Rather, we find little evidence of heterogeneity across the political spectrum. Our estimates document consistent shifts to the right regardless of a county's past elections outcomes.

As with any instrumental variable estimator, we identify impacts among compliers. In our setting, these are migrants who arrived in a particular county by virtue of the networks from their municipality of birth. The newcomers we identify moved to places where there *were* job opportunities in the past (perhaps decades ago), rather than places with promising contemporaneous or future economic opportunity. This process inefficiently distributes unauthorized migrants across the United States. Our study for mechanisms suggests that the inefficient distribution of migrants increases poverty and unemployment in migrants' counties of residence, causes declines in population, and shifts the community away from universalist values. The conservative shift in voting and spending is mediated by these underlying economic and demographic changes.

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Appendix A Secure Communities

We use Secure Communities, a locally implemented federal deportation program, to interrogate selection in our main explanatory variable. Later we look at the the impact of unauthorized migration on outcomes from the program. Secure Communities was a federal program that facilitated information sharing between local police and sheriff's departments and Immigration and Customs Enforcement (ICE). Local departments could submit fingerprints to ICE, which could use them to identify some individuals eligible for deportation. In turn, ICE would request that an individual be held on a detainer so that the deportation process could begin.

The program is useful to interrogate our data because it is the largest immigration program during our period of study and because it was implemented at the local level. The rollout was progressive, but not entirely random. The share of Hispanics, distance to the border, and crime rates are predictors of early adoption (Cox and Miles, 2013). We follow a collection of papers that uses the remaining exogenous variation. In our case we interrogate whether applying for a Consular ID is elastic to the policy environment, using the Secure Communities implementation. We find evidence of an inelastic decision.

Once in place, we study the program as a dependent variable and examine the intensive margin. We ask whether new flows of unauthorized migrants change how local authorities use the program. As the program became established, it was subject to political manipulation. As of 2013, local authorities were required to participate in Secure Communities. However, before it was mandatory, the program became politicized. States tried to opt out. Some counties sought to circumvent the program by refusing to submit fingerprints for individuals with no or little criminal background (Mitchell, 2011). Other counties argued that detainers from ICE were requests that could be denied and announced they would decline. County officials argued that the program was facilitating deportation of non-criminals and undermining police relations in immigrant communities (Lind, 2014; Mitchell, 2011). In 2014 the program was scaled back after federal courts held that ICE detainers were optional,[11] and counties could be held liable for due process violations of individuals detained solely at the request of ICE.[12]

[11] Galarza v. Szalczyk, 745 F.3d 634 (3d Cir. 2014)

[12] Miranda-Olivares v. Clackamas Cnty., No. 3:12-cv-02317-ST (D. Or. Apr. 11, 2014)

Appendix B Evolution of estimated number of unauthorized Mexican migrants

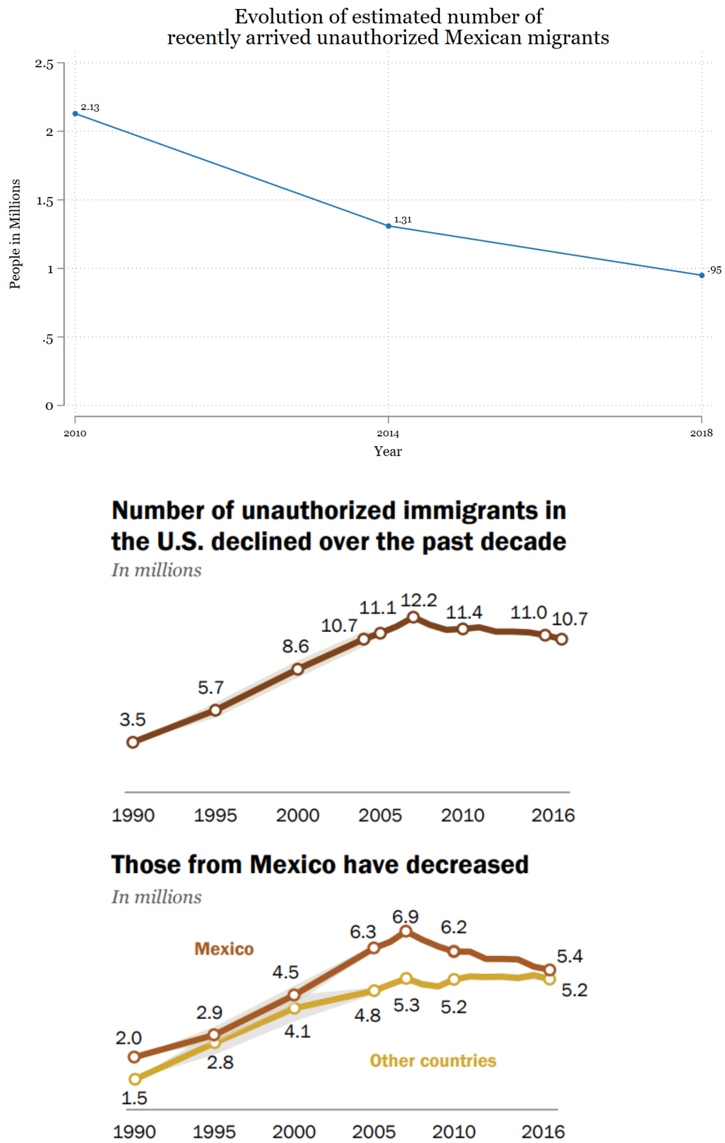

Figure 5: Evolution of estimated number of unauthorized Mexican migrants using Consular data. Source: Passel and Cohn (2018)

Appendix C Summary statistics of selected variables for newcomer Mexican unauthorized migrants using ACS 5 and Consular data

There are 441 counties in ACS-5 with detailed demographic characteristics for likely unauthorized Mexican migrants. We compare the distribution of those characteristics for those counties with that of "recently arrived migrants" in the consular data. The only substantive difference relates to age. This is not a surprising difference because children apply for consular IDs at much lower rates than adults. In our final sample, less than 2% of cardholders are under 18.

Table 10: Summary statistics of selected variables for newcomer Mexican unauthorized migrants using ACS 5 and Consular data

	(1) ACS 5	(2) Consular data same counties	(3) Consular data full sample
Female	0.41	0.41	0.41
	(0.49)	(0.49)	(0.49)
Never married/single	0.49	0.46	0.46
	(0.50)	(0.50)	(0.50)
Age	30.04	32.48	32.38
	(10.64)	(11.88)	(11.76)
Observations	45818	3677220	4380979
Number of Counties	441	441	2684

Sources: SRE, 2022 and IPUMS, 2022. The ACS 5 sample is comprised of people born in Mexico without US citizenship who arrived in the US less than five years before and with no college degree and between 16 and 64 years old. The Consular sample is comprised of unique new observations per period per CBSA.

Appendix D Description of Specifications for Secure Communities

Other studies have identified the impacts of Secure Communities. East et al. (2022) identify lower employment shares among unauthorized migrants and Alsan and Yang (2019) a decline in enrollment in federal entitlement benefits like SNAP and SSI among Hispanic citizens due to fear of deportation. In theory, the activation of Secure Communities could discourage applying for a consular ID, as it would be obvious to local authorities that the cardholder is a foreign national, perhaps prompting those authorities to submit fingerprints. To identify whether applications to Consular IDs are elastic to the policy environment, we study the correlation between the activation of Secure Communities and the number of new IDs issued. Given that Secure Communities was rolled out gradually (although not randomly) we carry out six different event-study designs using Callaway and Sant'Anna (2021) generalized difference-in-differences estimator. The main differences between them are exact period of analysis and the use of controls identified in previous studies (Cox and Miles, 2013; Alsan and Yang, 2019) that correlate with the time adoption. In general, estimations progressively build to each other.

The first estimation is the simplest. Secure Communities was implemented from October 2008 to September 2013, so our period of analysis goes from the first quarter in 2006 to the fourth quarter in 2016. Always-control counties (98 out of 2678) are those that adopted the program lastly, in the first quarter of 2013. The second estimation is the same, except that it weights the regression by population. The third estimation follows Cox and Miles (2013) and controls for distance to the Mexican border and share of Hispanic population—strong correlates of time of adoption. The fourth estimation follows Alsan and Yang (2019) and, on top of controlling for distance to the Mexican border and share of Hispanic population, excludes border counties and the states of Massachusetts, New York, and Illinois. The authors argue that border counties were early adopters, possibly due to experience with immigration enforcement, and that the three mentioned states fought against the implementation of the program. The fifth estimation uses population weights on the fourth estimation. Finally, the sixth estimation uses weights and controls, like the fifth, but restricts the periods of analysis to 2008–2013. The intention is to have a larger (880) and more diverse group of always-control counties. All estimations restrict the results to eight quarters (2 years) after the activation of the program.

Figure 6 displays the evolution of take-up rates across a number of specifications reflecting different comparison options. The results, while sensitive to specifications, are consistently statistically not significant.

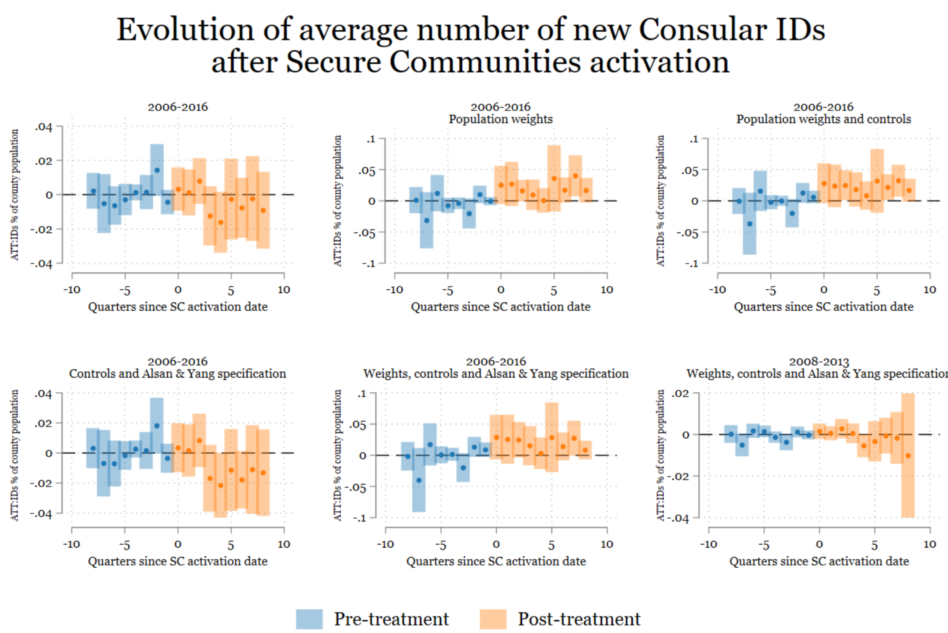

Figure 6: Secure Communities

Appendix E Summary statistics

Table 11: Summary statistics

	Mean	Std	Min	Max	Obs	Counties	Data relative to periods
Newcomers, population fraction	.462	.591	0	9.404	8022	2674	0
Instrument, leave county out	.421	.572	0	3.822	8022	2674	0
Instrument, leave CBSA out	.404	.551	0	3.822	8022	2674	0
Instrument, push factors	.417	.555	0	3.706	8022	2674	0
Vote share Republican House, mid	48.2	19.4	0	100	7995	2673	0
Vote share Repub House, Pres	47.2	19.9	0	100	8015	2673	2
Vote share Repub Senate, Pres	43.1	19.38	0	94.89	5361	2673	0
Vote share Republican President	45.77	16.45	4.09	95.43	7238	2673	2
Turnout in midterm elections	48.23	11.6	0	136.2	7667	2674	0
Total revenue, pc log	1.57	.39	-.02	4.18	5340	2634	2.5
Total (dir exp), pc log	1.54	.39	-.07	4.17	5340	2670	2.5
Edu (dir exp), pc 0-19 log	1.97	.34	.47	4.69	5330	2665	2.5
Edu (dir exp), share	40.88	11.59	3.33	89.47	5330	2665	2.5
Police (dir exp), pc log	-1.43	.5	-6.87	1.54	5338	2670	2.5
Police (dir exp), share	5.45	1.855	.023	71.746	5338	2670	2.5
Judicial (dir exp), pc log	-2.95	.88	-10.12	-.31	5296	2662	2.5
Judicial (dir exp), share	1.403	.85	.001	12.486	5296	2662	2.5
Real GDP, pc log	3.89	.44	2.12	8.34	7887	2629	1
Real Median HH income, log	10.88	.26	9.97	11.79	8022	2674	1
Unemployment rate, log	1.69	.45	.34	3.38	8022	2674	1
Poverty rate, log	2.59	.39	.99	3.87	8022	2674	1
Out-migration rate, log	.06	.02	.01	.3	8020	2674	1
All crime, pc log	-3.48	.94	-11.11	-.92	7872	2657	1.5
Violent crime, pc log	-5.87	1.03	-15.45	-3.42	7820	2652	1.5
Property crime, pc log	-3.88	.92	-11.15	-.95	7858	2656	1.5
Total emp, pc 15-64 log	-.67	.34	-3.97	1.71	8009	2672	1
Construction emp, pc log	-3.62	.45	-6.65	.42	7464	2580	1
Manufacturing emp, pc log	-3.07	.74	-8.03	-.33	7433	2540	1
Leisure emp, pc log	-2.75	.43	-7.26	.23	7919	2659	1
Agric emp, pc log	-6.57	1.53	-11.18	-1.14	4516	1892	1
Weekly average wages, 2010 USD	874	262	313	2401	8009	2672	1
Weekly wages, construction	993	223	227	2363	7464	2580	1
Weekly wages, manufacturing	1121	356	135	3760	7433	2540	1
Weekly wages, leisure	366	120	81	1048	7919	2659	1
Weekly wages, agric	611	198	136	1894	4516	1892	1
Relative importance univ values	.152	.497	-3.803	3.482	5802	2096	-2
Ideology (V. Liberal - V. Conserv)	-.036	.166	-.682	.781	2209	975	1
Partisan Id (V. Dem - V. Rep)	-.026	.158	-.733	.77	2209	975	1
Population, '000	1173.7	1973.9	.4	10061.5	8022	2674	0

Column 1 is the mean of the variable. Column 2 is the standard deviation. Column 3 is the minimum. Column 4 is the maximum. Column 5 is the total number of country-period observations. Column 6 is the number of unique counties. Column 7 reflects the year for which we have data relative to our periods: 0 indicates that the data is for the years 2010, 2014, and 2018; 1 indicates that it is for the years 2011, 2015 and 2019; 1.5 (2.5) indicates it is a 1.5 (2.5) year average after the end of our periods; -2 indicates that it is for the years 2008, 2012, and 2016. All the estimates are weighted by county population

Appendix F Spatial auto-correlation in number of observed migrants

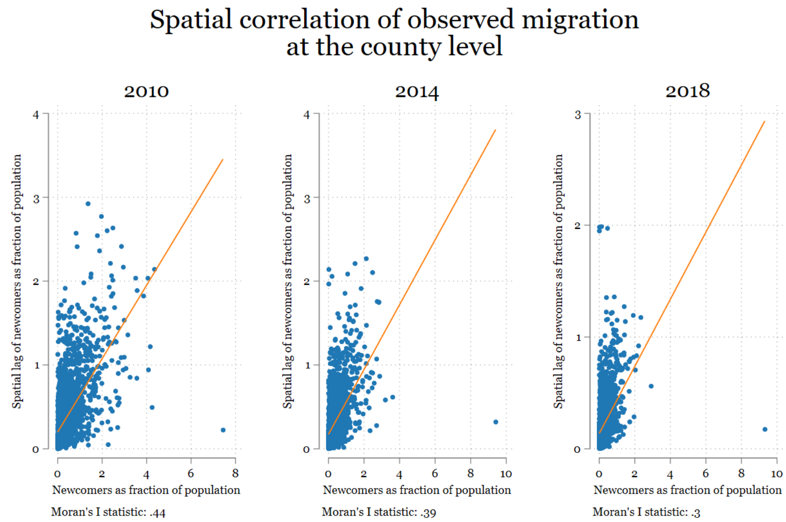

Figure 7: Spatial auto-correlation

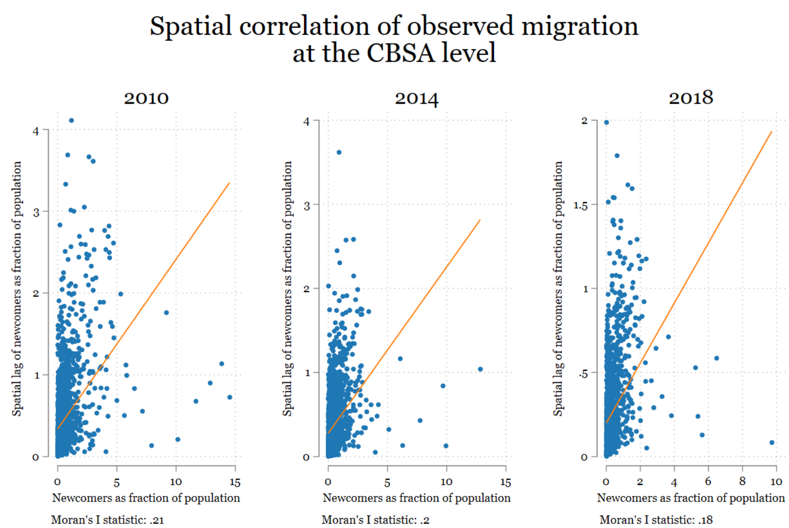

Figure 8: Spatial auto-correlation

Appendix G Correlation between instrument and stock of Mexican-born population

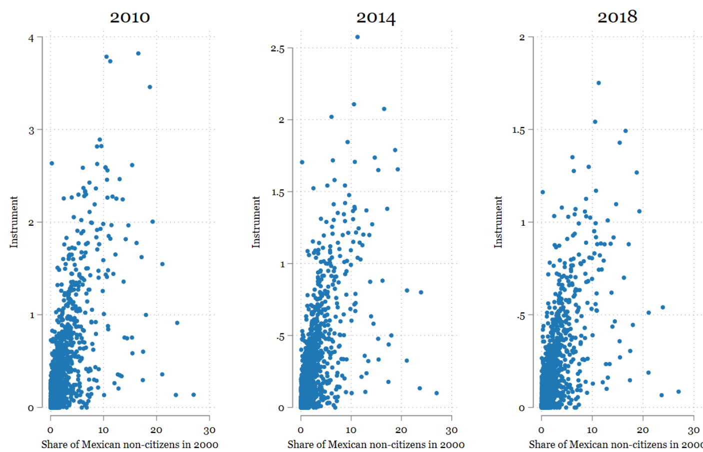

Figure 9: Correlation between instrument and estimated share of Mexican non-citizens in 2000

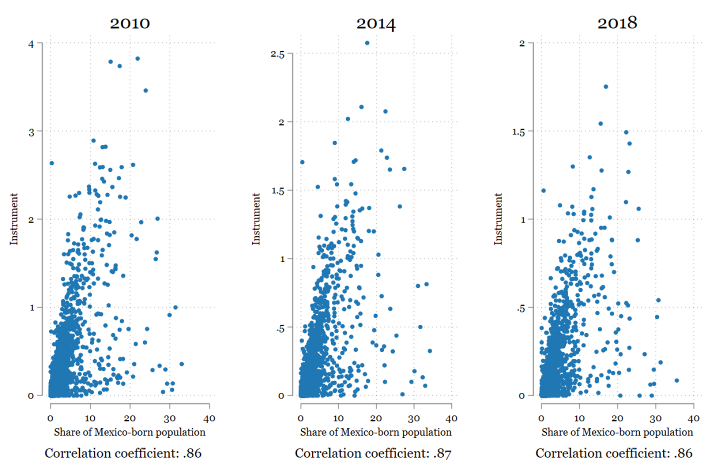

Figure 10: Correlation between instrument and estimated share of Mexico-born people at the beginning of period

Appendix H Suggestive evidence of migrants' selection into economically promising areas

Table 12: Association between economic prosperity and observed migration

	Newcomers, percent population	Newcomers, percent population
Unemployment rate	-0.188	
	(0.111)	
Real GDP per capita		0.259***
		(0.071)

Dependent variable is observed migration as share of county population. Independent variables are logged and measured the year before the beginning of the periods: 2006, 2010 and 2014. Sources: US Census; ACS-5; USDA's Economic Research Service, and LAUS. Standard errors clustered at CBSA level. Estimations control for county and state-period FE. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

Appendix I Heterogeneity by Mexican population

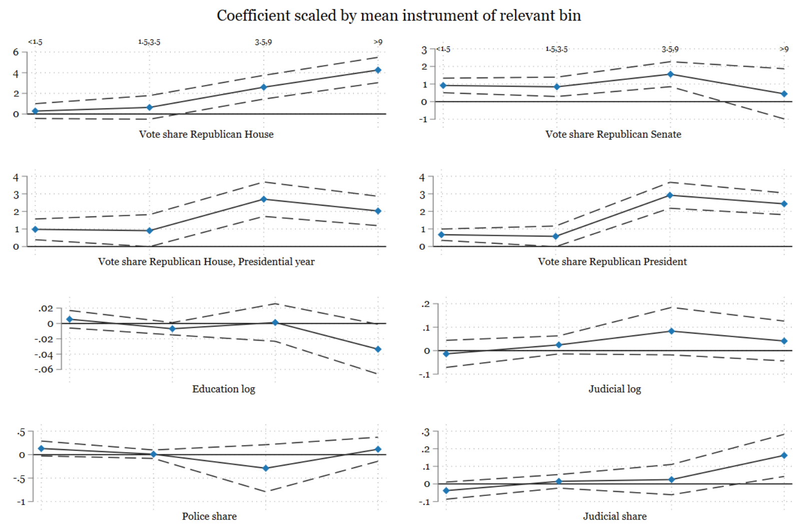

Figure 11: Heterogeneous impacts by Mexican population

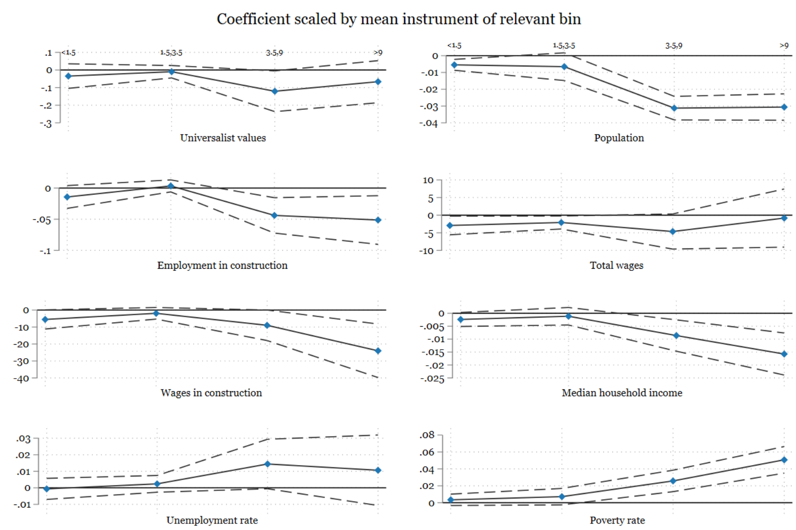

Figure 12: Heterogeneous impacts by Mexican population, mechanisms

Appendix J Short vs long term effects

Table 13: Long vs short-term effects

	Midterms	Pres year		Log pc		Share of Expend	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	House	House	Pres	D. Exp	Educ	Police	Judicial
A. Reduced form, baseline, no period 1							
Instrument	7.42*	-3.47	-2.31	0.24***	0.06**	0.01	-0.26**
	(3.62)	(4.16)	(1.88)	(0.05)	(0.02)	(0.20)	(0.10)
B. Short vs long-term							
Instrument	-3.20	-25.00**	-14.50*	0.38	-0.07	-4.05***	-1.25**
	(9.65)	(8.84)	(6.27)	(0.37)	(0.17)	(0.94)	(0.39)
Lagged instrument	5.31	10.76*	6.14*	-0.09	0.08	2.43***	0.59**
	(4.81)	(4.32)	(2.82)	(0.23)	(0.10)	(0.55)	(0.23)

Dependent variables in columns 1–3 are the vote share for Republicans in different federal elections. Dependent variables in column 4–5 are the log of per capita (per child population in column 4) expenditure. Columns 6–7 are the share of direct expenditure. All estimations are reduced form. Sources: Dave Leip's US Election Data; Annual Survey of State and Local Government Finances; US Census; ACS-5; USDA's Economic Research Service; Peter K. Schott's Data; County Business Patterns; Acemoglu et al. (2016). and QCEW. Panel 1 is the baseline estimation without period 1. Panel 2 implements the Jaegger et al. (2018) correction to identify short-term vs long-term effects. Standard errors clustered at CBSA level, except for columns 4–7 (robust standard errors). Estimations control for county and state-period FE, except for columns 4–7 (only state fixed effects). Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

Table 14: Long vs short-term effects

	Emp (log)		Wages	Log of rate	Log	Log pc	Values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Const	Hosp and leis	Agric	Poverty	Pop	Out-mig	Universalist
A. Reduced form, no period 1							
Instrument	0.22**	0.26***	7.01	0.13**	2.09***	-0.22***	0.15**
	(0.07)	(0.05)	(21.70)	(0.05)	(0.24)	(0.05)	(0.06)
B. Short vs long-term							
Instrument	-0.53	0.61	-135.18	1.07***	-0.15	-0.69**	-0.04
	(0.51)	(0.45)	(116.92)	(0.29)	(1.11)	(0.23)	(0.32)
Lagged instrument	0.45	-0.21	86.16	-0.56**	1.34*	0.28*	0.12
	(0.30)	(0.28)	(72.06)	(0.17)	(0.68)	(0.13)	(0.19)

Dependent variables in columns 1–2 are the the log of average annual employment divided by working age population. Dependent variable in column 3 is the annual average weekly wages in 2010 USD. Column 4 is the log of poverty rate the year after the end of the period. The dependent variable in Column 5 is the log of county population. The dependent variable in Column 6 is out-migration, calculated as the log of out-migration divided by county population. Column 7 is the average county value, as reported by Enke (2020). Panel 1 is the baseline estimation without period 1. Panel 2 implements the Jaegger et al. (2018) correction to identify short term vs long term effects. Standard errors clustered at CBSA level. Estimations control for county and state-period FE. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

Appendix K Robustness checks for mechanisms

Table 15: Robustness checks for mechanisms

	Emp (log)		Wages	Log of rate	Log	Log pc	Values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Const	Hosp and leis	Agric	Poverty	Pop	Out-mig	Universalist
A. Reduced form, baseline							
Instrument	-0.06**	-0.02*	-49.93*	0.11***	-0.04***	0.04***	-0.16**
	(0.02)	(0.01)	(22.42)	(0.02)	(0.01)	(0.01)	(0.05)
B. Lagged outcome (LO)							
Instrument	0.03	-0.01	45.35*	-0.07**	-0.02	0.13***	
	(0.02)	(0.02)	(20.05)	(0.03)	(0.02)	(0.03)	
C. Mex non-citizen, sh							
Instrument	-0.14***	0.01	-34.68	0.13***	-0.07***	0.04	-0.21**
	(0.03)	(0.01)	(25.17)	(0.02)	(0.01)	(0.02)	(0.07)
D. Adult HS completion							
Instrument	-0.07***	-0.02	-44.29*	0.12***	-0.05***	0.04**	-0.19***
	(0.02)	(0.01)	(22.12)	(0.02)	(0.01)	(0.01)	(0.05)
E. China shock							
Instrument	-0.06**	-0.02	-54.66*	0.10***	-0.03***	0.05***	-0.12*
	(0.02)	(0.01)	(23.29)	(0.02)	(0.01)	(0.01)	(0.05)
F. Simulated instrument							
Instrument	-0.17***	0.02	-27.03	0.10***	-0.06***	0.08*	-0.28**
	(0.04)	(0.03)	(36.31)	(0.02)	(0.02)	(0.04)	(0.10)
G. Stock Mex foreign							
Instrument	-0.07**	-0.02	-50.64*	0.11***	-0.04***	0.04**	-0.11*
	(0.02)	(0.01)	(24.01)	(0.02)	(0.01)	(0.01)	(0.05)
H. No-outliers							
Instrument	-0.07*	-0.04**	-61.81*	0.14***	-0.06***	0.06***	-0.19**
	(0.03)	(0.01)	(30.17)	(0.02)	(0.01)	(0.02)	(0.07)
I. LOO-State							
Instrument	-0.08**	-0.03**	-66.91**	0.14***	-0.05***	0.06***	-0.20***
	(0.02)	(0.01)	(25.32)	(0.02)	(0.01)	(0.01)	(0.06)
J. No pop weights							
Instrument	-0.09**	-0.01	-9.83	0.13***	-0.08***	0.01	-0.25**
	(0.03)	(0.01)	(13.70)	(0.01)	(0.01)	(0.03)	(0.09)

Dependent variables in columns 1–2 are the the log of average annual employment divided by working age population. Dependent variable in column 3 is the annual average weekly wages in 2010 USD. Column 4 is the log of poverty rate the year after the end of the period. The dependent variable in Column 5 is the log of county population. The dependent variable in Column 6 is out-migration, calculated as the log of out-migration divided by county population. Column 7 is the average county value, as reported by Enke (2020). All estimations are reduced form. Sources: Dave Leip's US Election Data; Annual Survey of State and Local Government Finances; US Census; ACS-5; USDA's Economic Research Service; Peter K. Schott's Data; County Business Patterns; Acemoglu et al. (2016). Enke (2020). and QCEW. Panel 1 is reduced form estimation. Panel 2 estimates effect on lagged dependent variables. Panel 3 controls for pre-2007 features interacted with period dummies. Panel 4 includes simulated instrument following Borusyak and Hull (2020). Panel 5 controls for the stock of Mexican non-citizens at the beginning of period. Panel 6 excludes outliers, uses the LOO state instrument, and does not use predicted population weights. Standard errors clustered at CBSA level. Estimations control for county and state-period FE. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

Appendix L Robustness checks of null results

Table 16: Robustness checks for null results

	Emp (log)		Wages		Log		
	(1) Total	(2) Agric	(3) Total	(4) Leis and hosp	(5) GDP pc	(6) Med HH inc	(7) Unemp rate
A. Reduced form, baseline							
Instrument	-0.00	-0.11	-2.06	-9.35	-0.03	-0.03	0.04
	(0.01)	(0.09)	(21.62)	(7.42)	(0.02)	(0.02)	(0.03)
B. Mex non-citizen, sh							
Instrument	-0.01	-0.28**	-46.13*	-21.43**	-0.04	-0.04*	0.17***
	(0.01)	(0.09)	(19.20)	(6.60)	(0.02)	(0.02)	(0.04)
C. Adult HS completion							
Instrument	-0.00	-0.11	-7.69	-9.79	-0.03	-0.03	0.05
	(0.01)	(0.09)	(20.35)	(7.06)	(0.02)	(0.02)	(0.03)
D. China shock							
Instrument	-0.00	-0.11	5.71	-4.84	-0.02	-0.02	0.03
	(0.01)	(0.09)	(22.20)	(7.36)	(0.02)	(0.02)	(0.03)
E. Simulated instrument							
Instrument	-0.02	-0.39***	19.43	7.68	0.00	-0.01	0.09
	(0.02)	(0.10)	(45.08)	(11.50)	(0.03)	(0.02)	(0.07)
F. Stock Mex foreign							
Instrument	-0.00	-0.13	0.39	-10.02	-0.02	-0.02	0.03
	(0.01)	(0.08)	(21.13)	(7.51)	(0.02)	(0.02)	(0.03)
G. No-outliers							
Instrument	-0.00	0.03	-9.73	-16.06*	-0.03	-0.04*	0.05
	(0.01)	(0.07)	(26.92)	(7.23)	(0.03)	(0.02)	(0.03)
H. LOO-State							
Instrument	-0.00	-0.11	-9.66	-16.11*	-0.04*	-0.04*	0.08**
	(0.01)	(0.13)	(21.17)	(7.74)	(0.02)	(0.02)	(0.02)
I. No pop weights							
Instrument	0.01	0.05	-15.14*	-3.95	-0.02	-0.04***	0.05*
	(0.01)	(0.06)	(6.29)	(3.13)	(0.02)	(0.01)	(0.02)

Dependent variables in columns 1–2 are the the log of average annual employment divided by working age population. Dependent variable in column 3–4 are the annual average weekly wages in 2010 USD. Column 4 is the log of poverty rate the year after the end of the period. Dependent variables in columns 5–7 log of: GDP per capita (in 2012 USD), median household income (in 2010 USD), and unemployment rate. All estimations are reduced form. Sources: Dave Leip's US Election Data; US Census; ACS-5; USDA's Economic Research Service; Peter K. Schott's Data; County Business Patterns; Acemoglu et al. (2016). and QCEW. Panel 1 is reduced form estimation. Panel 2 controls for pre-2007 features interacted with period dummies. Panel 3 includes simulated instrument following Borusyak and Hull (2020). Panel 4 controls for the stock of Mexican non-citizens at the beginning of period. Panel 5 excludes outliers, uses the LOO state instrument and does not use predicted population weights. Standard errors clustered at CBSA level. Estimations control for county and state-period FE. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

Table 17: Robustness checks for null results (cont)

	Crime (log pc)			Preferences (scaled)		
	(1) All	(2) Violent	(3) Property	(4) Republican (id)	(5) Ideology (cons)	(6) Turnout
A. Reduced form, baseline						
Instrument	-0.03	-0.01	-0.01	0.03	0.03	-0.81
	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.96)
B. Mex non-citizen, sh						
Instrument	-0.04	-0.01	-0.04	0.05	0.05	-1.61
	(0.03)	(0.04)	(0.04)	(0.06)	(0.05)	(1.45)
C. Adult HS completion						
Instrument	-0.03	-0.01	-0.01	0.03	0.03	0.03
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
D. China shock						
Instrument	-0.01	-0.03	0.02	0.03	0.03	-0.33
	(0.03)	(0.04)	(0.03)	(0.04)	(0.04)	(1.02)
E. Simulated instrument						
Instrument	0.13**	-0.01	0.18**	0.05	0.03	-1.99
	(0.05)	(0.07)	(0.06)	(0.09)	(0.06)	(2.45)
F. Stock Mex foreign						
Instrument	-0.01	0.00	-0.00	0.03	0.03	-0.53
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.98)
G. No-outliers						
Instrument	-0.02	-0.00	-0.01	0.08	0.07	0.89
	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.76)
H. LOO-State						
Instrument	-0.05	-0.01	-0.03	0.04	0.04	-0.97
	(0.03)	(0.04)	(0.04)	(0.05)	(0.04)	(1.03)
I. No pop weights						
Instrument	0.02	0.04	0.02	0.05	0.06	-1.57**
	(0.05)	(0.06)	(0.05)	(0.04)	(0.04)	(0.57)

Dependent variables in columns 1-3 are 2-year averages of the log of per capita total crime, violent crime index, and property crime index. All estimations are reduced form. Dependent variables in columns 4–5 are average county preferences, normalized and shrunk following Enke (2020). The dependent variable in column 6 is turnout in House midterm elections, as share of registered voters. Sources: Enke (2020); Cooperative Election Study; Jacob Kaplan's Concatenated Files: Uniform Crime Reporting Program Data: Offenses Known and Clearances by Arrest (Return A), 1960–2020; US Census; ACS-5; USDA's Economic Research Service; Peter K. Schott's Data; County Business Patterns; Acemoglu et al. (2016). Panel 1 is reduced form estimation. Panel 2 controls for pre-2007 features interacted with period dummies. Panel 3 includes simulated instrument following Borusyak and Hull (2020). Panel 4 controls for the stock of Mexican non-citizens at the beginning of period. Panel 5 excludes outliers, uses the LOO state instrument, and does not use predicted population weights. Standard errors clustered at CBSA level. Estimations control for county and state-period FE. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

Appendix M Alternative standard errors

Adão et al. (2019) show that in shift-share designs, standard errors are correlated with initial share composition. They argue that accounting for such association is more accurate than using heteroskedastic standard errors or geographically clustered standard errors, as we do.

Both Stata and R have commands to implement their proposed correction. However, they cannot easily accommodate a large set of fixed effects. Therefore, we implement a correction inspired by them, but easier to implement. The basis of such correction is cluster analysis. We use different techniques to group counties based on the values of their 2,439 initial shares. We vary the number of clusters (from 200 to 1000) and the technique to construct them: we use both kmeans and hierarchical clustering (single). Finally, we cluster the standard errors at the level of such groups. The main problem with this approach is that we have several groups with one county (which is similar to our main estimation, where we cluster the standard errors at the CBSA level) and one group with close to a thousand counties. To provide different, more balanced groups, we obtain, via principal components analysis, the 10 first components of the 2,439 shares. We use these components in two ways. On the one hand, we carry out cluster analysis in those factors only. On the other hand, we create another group based only on the values of the first component. We do not use cluster analysis in this case, but rather divide the sample into 500 equally sized groups. This approach forms more intuitive groups. For example, the last one of them is composed of Los Angeles County, Cook County (Chicago), Orange County, Harris County (Houston), and Maricopa County (Phoenix).

Tables 18 presents the 2SLS results of these approaches with the main results. None of them consistently changes the significance.

Table 18: Alternative standard error calculation

	Midterms	Pres year		Log pc		Share of Expend	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	House	House	Pres	D. Exp	Educ	Police	Judicial
A. Baseline							
Newcomers, pct. pop.	8.491***	3.486**	4.419***	-0.042**	-0.050**	0.418**	0.264**
	(1.034)	(1.211)	(0.708)	(0.016)	(0.017)	(0.144)	(0.101)
Clustered at state-level							
Newcomers, pct. pop.	8.496***	3.489	4.426**	-0.042**	-0.050**	0.419*	0.265*
	(1.123)	(1.854)	(1.273)	(0.014)	(0.016)	(0.177)	(0.124)
Eicker Huber White							
Newcomers, pct. pop.	8.491***	3.486**	4.419***	-0.042**	-0.050**	0.418**	0.264*
	(1.050)	(1.198)	(0.626)	(0.016)	(0.016)	(0.139)	(0.109)
PCA 1							
Newcomers, pct. pop.	8.475***	3.464**	4.400***	-0.042***	-0.050**	0.417***	0.265*
	(1.068)	(1.112)	(0.599)	(0.012)	(0.017)	(0.111)	(0.113)
Kmeans, 200 (pca)							
Newcomers, pct. pop.	8.475***	3.464	4.400**	-0.042**	-0.050**	0.417**	0.265*
	(1.806)	(2.095)	(1.625)	(0.016)	(0.016)	(0.141)	(0.116)
Kmeans, 400 (pca)							
Newcomers, pct. pop.	8.475***	3.464	4.400***	-0.042**	-0.050**	0.417**	0.265*
	(1.528)	(1.776)	(1.289)	(0.016)	(0.016)	(0.146)	(0.112)
Kmeans, 600 (pca)							
Newcomers, pct. pop.	8.475***	3.464*	4.400***	-0.042**	-0.050**	0.417**	0.265*
	(1.402)	(1.613)	(1.151)	(0.016)	(0.016)	(0.141)	(0.110)
Kmeans, 800 (pca)							
Newcomers, pct. pop.	8.475***	3.464*	4.400***	-0.042**	-0.050**	0.417**	0.265*
	(1.318)	(1.518)	(1.077)	(0.016)	(0.016)	(0.139)	(0.110)
Kmeans, 1000 (pca)							
Newcomers, pct. pop.	8.475***	3.464*	4.400***	-0.042**	-0.050**	0.417**	0.265*
	(1.264)	(1.446)	(0.993)	(0.016)	(0.016)	(0.141)	(0.110)
Kmeans, 1000 (all shares)							
Newcomers, pct. pop.	8.475***	3.464*	4.400***	-0.042**	-0.050**	0.417**	0.265*
	(1.398)	(1.518)	(1.129)	(0.016)	(0.016)	(0.142)	(0.110)
Hierarchical, 800 (all shares)							
Newcomers, pct. pop.	8.475***	3.464	4.400**	-0.042**	-0.050**	0.417**	0.265*
	(1.941)	(2.177)	(1.677)	(0.015)	(0.017)	(0.135)	(0.119)
Kmeans, 800 (all shares)							
Newcomers, pct. pop.	8.475***	3.464*	4.400***	-0.042**	-0.050**	0.417**	0.265*
	(1.515)	(1.703)	(1.279)	(0.016)	(0.016)	(0.138)	(0.112)

Row 1 is the baseline 2SLS specification. Row 2 clusters the standard errors (SE) at the state level. Row 2 uses Eicker Huber White SE. Row 4 clusters the SE by the distribution of the first component of all 2,439 shares—obtained after carrying out a principal component analysis. Counties are assigned to one of 500 groups. Rows 5–9 cluster SE at the level of one of 200–1000 groups obtained by classifying counties according to their first 10 components using kmeans. Rows 10–11 cluster SEs at the level of one of 800-1000 groups obtained by classifying counties according to their shares using kmeans. Row 12 clusters SE at the level of 800–1000 groups obtained by classifying counties according to their shares using hierarchical clusters (single linkage). Estimations control for county and state-period fixed effects and weight by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

Appendix N Main results with instrument built with push factors

Table 19: Political effects of arrival of unauthorized Mexican migrants 2010–2018 (push factors instrument)

	Midterms, GOP	Presidential year, GOP		
	(1) House	(2) House	(3) Senate	(4) President
A. OLS				
Newcomers, pct. pop.	6.51***	2.82**	1.74	3.19***
	(0.87)	(1.06)	(1.60)	(0.65)
B. 2SLS				
Newcomers, pct. pop.	11.42**	0.29	12.53	2.45
	(3.67)	(4.86)	(11.70)	(1.82)
Std Coefficient	0.35	0.01	0.40	0.09
C. Reduced form				
Instrument	14.36***	0.37	7.84	3.79
	(4.29)	(6.11)	(7.16)	(2.83)
Std Coefficient	0.41	0.01	0.24	0.13
Inst., Mean	7995	8015	5361	7238
Inst., Sd	0.42	0.42	0.44	0.43
Observations	0.56	0.56	0.58	0.56
Dep. Var., Mean	48.16	47.24	43.10	45.77
Dep. Var., Sd	19.44	19.92	19.38	16.45
Ind. Var., Mean	0.46	0.46	0.48	0.48
Ind. Var., Sd	0.59	0.59	0.62	0.61

Dependent variables are share of Republican vote. Source: Dave Leip's United States Election Data. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population using push factors to predict migration flows. All regressions have period and county fixed effects. Standard errors are clustered at the CBSA level. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

Table 20: Public spending effects of arrival of unauthorized Mexican migrants 2012 and 2017 (push factors instrument)

	Expenditure (log pc 2010 USD)					Share of Expenditure		
	(1) Revenue	(2) Direct exp	(3) Edu	(4) Police	(5) Judicial	(6) Edu	(7) Police	(8) Judicial
A. OLS								
Newcomers, pct. pop.	-0.02	-0.02	-0.03*	0.02	0.08	0.32	0.20	0.13
	(0.01)	(0.01)	(0.01)	(0.02)	(0.06)	(0.42)	(0.12)	(0.09)
B. 2SLS								
Newcomers, pct. pop.	-0.02	0.00	-0.02	0.19	0.53	0.54	0.89	0.86
	(0.05)	(0.07)	(0.08)	(0.11)	(0.36)	(3.12)	(0.49)	(0.50)
Std. Coefficient	-0.03	0.00	-0.04	0.26	0.41	0.03	0.33	0.69
C. Reduced form								
Instrument	-0.02	0.00	-0.02	0.18**	0.51*	0.52	0.85*	0.83***
	(0.05)	(0.06)	(0.08)	(0.06)	(0.21)	(2.94)	(0.41)	(0.23)
Std. Coefficient	-0.03	0.00	-0.03	0.22	0.34	0.03	0.27	0.57
Observations	5340	5340	5330	5338	5296	5330	5338	5296
Dep. Var., Mean	1.57	1.54	1.97	-1.43	-2.95	40.88	5.45	1.40
Dep. Var., Sd	0.39	0.39	0.34	0.50	0.88	11.59	1.85	0.85
Ind. Var., Mean	0.55	0.55	0.55	0.55	0.55	0.55	0.55	0.55
Ind. Var., Sd	0.68	0.68	0.68	0.68	0.68	0.68	0.68	0.68
Inst., Mean	0.44	0.44	0.44	0.44	0.44	0.44	0.44	0.44
Inst., Sd	0.58	0.58	0.58	0.58	0.58	0.58	0.58	0.58

Dependent variables in columns 1–5 are in log 2010 dollars per capita, except education (column 3) which is per child (population under 19). Dependent variables in columns 6–8 are shares of total direct expenditures. Revenue includes taxes, intergovernmental revenue, current charges, and miscellaneous general revenue. Direct Expenditure includes spending on public education, policing, health, as well as other categories as described in section 3. Education expenditures include all public education expenditures of the county. Police expenditures include city police spending in a county as well as sheriff department spending and local incarceration at county jails. Judicial expenditure includes all county expenditures on the administration of justice including prosecutors, public defense, judges, court administration, and expenses related to the civil court system. Source: Annual Survey of State and Local Government Finances. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population using push factors to predict migration flows. Source: SRE. Instrument is as described in Section 3. All regressions have period and county fixed effects. Standard errors are clustered at the CBSA level. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

Table 21: Effect of arrival of unauthorized Mexican migrants on employment among working age population 2010–2018 (push factors instrument)

	Employment, (log per working age pop)				
	(1) Total	(2) Constr	(3) Manufact	(4) Hosp and leis	(5) Agric
A. OLS					
Newcomers, pct pop.	0.01	-0.03	0.07***	-0.01*	0.03
	(0.01)	(0.02)	(0.02)	(0.01)	(0.06)
B. 2SLS					
Newcomers, pct pop.	0.00	0.00	0.20**	-0.05	-0.25
	(0.02)	(0.05)	(0.07)	(0.03)	(0.28)
Std. Coefficient	0.01	0.00	0.16	-0.08	-0.10
C. Reduced form					
Instrument	0.01	0.00	0.25**	-0.07*	-0.28
	(0.02)	(0.07)	(0.09)	(0.03)	(0.33)
Std. Coefficient	0.01	0.00	0.19	-0.09	-0.11
Observations	8009	7464	7433	7919	4516
Dep. Var., Mean	-0.67	-3.62	-3.07	-2.75	-6.57
Dep. Var., Sd	0.34	0.45	0.74	0.43	1.53
Ind. Var., Mean	0.46	0.46	0.46	0.46	0.52
Ind. Var., Sd	0.59	0.59	0.59	0.59	0.63
Inst., Mean	0.42	0.42	0.42	0.42	0.49
Inst., Sd	0.56	0.56	0.56	0.56	0.60

Dependent variables are the log of average annual employment divided by working age population. Sources: Quarterly Census of Employment and Wages. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population using push factors to predict migration flows. Source: SRE. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

Table 22: Effect of arrival of unauthorized Mexican migrants on weekly wages 2010–2018 (push factors instrument)

	Weekly Wages (2010 USD)				
	(1)	(2)	(3)	(4)	(5)
	Total	Constr	Manufact	Hosp and leis	Agric
A. OLS					
Newcomers, pct pop.	3.98	-14.35	12.37	-4.83	-18.30
	(14.27)	(9.58)	(24.00)	(4.45)	(12.19)
B. 2SLS					
Newcomers, pct pop.	12.35	7.27	51.83	-25.49	-73.49
	(32.41)	(24.89)	(92.20)	(14.00)	(57.57)
StdCoefficient	0.03	0.02	0.09	-0.13	-0.23
C. Reduced form					
Instrument	15.53	9.15	65.37	-32.05	-83.11
	(40.24)	(30.89)	(118.91)	(16.82)	(73.57)
Std. Coefficient	0.03	0.02	0.10	-0.15	-0.25
Observations	8009	7464	7433	7919	4516
Dep. Var., Mean	873.54	992.99	1121.38	366.09	610.58
Dep. Var., Sd	261.64	223.18	355.71	120.17	198.48
Ind. Var., Mean	0.46	0.46	0.46	0.46	0.52
Ind. Var., Sd	0.59	0.59	0.59	0.59	0.63
Inst., Mean	0.42	0.42	0.42	0.42	0.49
Inst., Sd	0.56	0.56	0.56	0.56	0.60

Dependent variables are the annual average weekly wages in 2010 USD. Sources: Quarterly Census of Employment and Wages. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population using push factors to predict migration flows. Source: SRE. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

Table 23: Socioeconomic effects of arrival of unauthorized Mexican migrants 2010–2018 (push factors instrument)

	County Economy (log)				
	(1) GDP pc	(2) Median household income	(3) Unemployment rate	(4) Poverty rate	(5) Poverty rate 2 years later
A. OLS					
Newcomers, pct. pop.	-0.01	-0.02	0.01	0.06***	0.07***
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
B. 2SLS					
Newcomers, pct. pop.	-0.03	-0.00	-0.01	0.13**	0.06
	(0.03)	(0.04)	(0.06)	(0.04)	(0.04)
Std. Coefficient	-0.04	-0.00	-0.01	0.19	0.08
C. Reduced form					
Instrument	-0.04	-0.00	-0.01	0.16***	0.07
	(0.04)	(0.05)	(0.08)	(0.04)	(0.05)
Std. Coefficient	-0.05	-0.00	-0.02	0.23	0.10
Observations	7887	8022	8022	8022	8022
Dep. Var., Mean	3.89	10.88	1.69	2.59	2.57
Dep. Var., Sd	0.44	0.26	0.45	0.39	0.39
Ind. Var., Mean	0.47	0.46	0.46	0.46	0.46
Ind. Var., Sd	0.59	0.59	0.59	0.59	0.59
Inst., Mean	0.42	0.42	0.42	0.42	0.42
Inst., Sd	0.56	0.56	0.56	0.56	0.56

Dependent variables are the log of: GDP per capita (in 2012 USD), median household income (in 2010 USD), unemployment rate, and poverty rate one year and two years after the end of the period. Sources: Small Area Income and Poverty Estimates (SAIPE) Program; US Department of Commerce: Bureau of Economic Analysis. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population using push factors to predict migration flows. Source: SRE. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

Table 24: Values, ideology, and demographic effects of arrival of unauthorized Mexican migrants 2010–2018 (push factors instrument)

	Preferences (scaled)			Pop (log)	Pc log	
	(1) Republican Identity	(2) Ideology Conservative	(3) Universalist Values	(4) Count	(5) Out-migration	(6) Turnout midterms
A. OLS						
Newcomers, pct. pop.	0.03	0.03	-0.09*	-0.02**	0.03**	-0.35
	(0.02)	(0.02)	(0.04)	(0.01)	(0.01)	(0.48)
B. 2SLS						
Newcomers, pct. pop.	-0.02	-0.10	0.00	-0.02	0.01	-0.92
	(0.09)	(0.11)	(0.14)	(0.02)	(0.05)	(1.31)
Std. Coefficient	-0.09	-0.39	0.00	-0.01	0.02	-0.05
C. Reduced form						
Instrument	-0.03	-0.12	0.00	-0.03	0.01	-1.17
	(0.11)	(0.12)	(0.17)	(0.02)	(0.06)	(1.74)
Std. Coefficient	-0.10	-0.46	0.00	-0.01	0.02	-0.06
Observations	2209	2209	5802	8022	8020	7667
Dep. Var., Mean	-0.04	-0.03	0.15	12.93	-2.94	48.23
Dep. Var., Sd	0.17	0.16	0.50	1.59	0.28	11.60
Ind. Var., Mean	0.50	0.50	0.47	0.46	0.46	0.48
Ind. Var., Sd	0.61	0.61	0.60	0.59	0.59	0.60
Inst., Mean	0.48	0.48	0.43	0.42	0.42	0.44
Inst., Sd	0.58	0.58	0.56	0.56	0.56	0.57

Dependent variables in columns 1–3 are average county preferences. The values for columns 1–2 are normalized and shrunk following Enke (2020). The dependent variable in column 4 is the log of county population. The dependent variable in column 5 is out-migration, calculated as the log of out-migration divided by county population. The dependent variable in column 6 is turnout in House midterm elections, as share of registered voters. Sources: Enke (2020); Cooperative Election Study; US Census Bureau: Population Division and Small Area; Dave Leip's United States Election Data; US Census Bureau: 2007–2011, 2011–2015, and 2015–2019 American Community Surveys. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population using push factors to predict migration flows. Source: SRE. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

Table 25: Effects of arrival of unauthorized Mexican migrants on crime (2010–2018) and on immigration enforcement (2008–2013) (push factors instrument)

	Count by foreign population (log)			Rate	Crime (log pc)		
	(1) Submissions	(2) Matches	(3) Removals	(4) Success	(5) All	(6) Violent	(7) Property
	A. OLS						
Newcomers, pct. pop.	0.48***	0.99***	1.00***	0.52***	-0.01	-0.02	0.01
	(0.10)	(0.11)	(0.13)	(0.05)	(0.02)	(0.03)	(0.03)
	B. 2SLS						
Newcomers, pct. pop.	1.45***	2.41***	2.38***	0.99***	-0.15	0.02	-0.18
	(0.18)	(0.25)	(0.26)	(0.10)	(0.11)	(0.09)	(0.16)
Std. Coefficient	0.54	0.70	0.62	0.59	-0.10	0.01	-0.12
	C. Reduced form						
Instrument	1.38***	2.30***	2.27***	0.94***	-0.19	0.02	-0.23
	(0.13)	(0.14)	(0.15)	(0.05)	(0.11)	(0.12)	(0.16)
Std. Coefficient	0.48	0.62	0.56	0.53	-0.12	0.01	-0.14
Observations	7964	7584	6069	7587	7872	7820	7858
Dep. Var., Mean	7.61	4.16	2.22	1.13	-3.48	-5.87	-3.88
Dep. Var., Sd	1.62	2.08	2.31	1.00	0.94	1.03	0.92
Ind. Var., Mean	0.46	0.47	0.48	0.47	0.46	0.46	0.46
Ind. Var., Sd	0.60	0.60	0.61	0.60	0.60	0.60	0.60
Inst., Mean	0.42	0.42	0.44	0.42	0.42	0.42	0.42
Inst., Sd	0.56	0.56	0.57	0.56	0.56	0.56	0.56

Dependent variables in columns 1–3 are submissions, matches and removals from the Secure Communities Program. These variables are calculated proportional to the time Secure Communities was in place in the county between 2008 and 2013 and proportional to the foreign population in the county in 2010. Dependent variable in column 4 is success rate–matched/submissions. Dependent variables in columns 5–7 are two year averages of the log of per capita total crime, violent crime index, and property crime index. Sources: Jacob Kaplan's Concatenated Files: Uniform Crime Reporting Program Data: Offenses Known and Clearances by Arrest (Return A), 1960–2020; ICE: Secure Communities Monthly Statistics through September 30, 2013. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population using push factors to predict migration flows. Source: SRE. Instrument is as described in Section 3. Standard errors are robust for the first four columns and clustered at the CBSA level for the last three. The first four estimations control for state fixed effects and the last three control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

Table 26: Robustness checks (push factors)

	Midterms	Pres year		Log pc		Share of Expend	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	House	House	Pres	D. Exp	Educ	Police	Judicial
A. Reduced form, baseline							
Instrument	14.36***	0.37	3.79	0.00	-0.02	0.85*	0.83***
	(4.29)	(6.11)	(2.83)	(0.06)	(0.08)	(0.41)	(0.23)
B. Lagged outcome (LO)							
Instrument	-4.18	6.53	2.39	0.08	0.08	-0.66	-0.53
	(4.98)	(6.64)	(1.23)	(0.08)	(0.05)	(0.74)	(0.49)
C. Mex non-citizen, sh							
Instrument	10.29*	3.23	5.65	0.02	-0.00	0.69	0.75**
	(4.57)	(5.64)	(3.28)	(0.07)	(0.07)	(0.40)	(0.23)
D. Adult HS completion							
Instrument	19.82***	5.61	10.06***	-0.00	-0.02	0.87*	0.83***
	(4.32)	(4.97)	(2.11)	(0.07)	(0.08)	(0.43)	(0.23)
E. China shock							
Instrument	11.83**	-1.78	1.43	0.00	-0.02	0.86*	0.82***
	(4.15)	(6.20)	(2.73)	(0.06)	(0.08)	(0.42)	(0.23)
F. Simulated instrument							
Instrument	14.83***	0.59	3.78	0.03	-0.01	0.59	0.70**
	(4.09)	(6.15)	(2.66)	(0.07)	(0.08)	(0.44)	(0.23)
G. Stock Mex foreign							
Instrument	13.98***	0.79	3.56	0.00	-0.02	0.86*	0.83***
	(4.22)	(5.99)	(2.92)	(0.06)	(0.08)	(0.40)	(0.23)
H. No-outliers							
Instrument	13.05*	5.11	2.83	0.17	-0.14	0.56	1.10**
	(5.28)	(4.96)	(3.10)	(0.12)	(0.16)	(0.73)	(0.39)
I. No pop weights							
Instrument	10.86***	5.35**	5.21***	0.08	0.08	0.85	1.07***
	(2.11)	(1.84)	(1.36)	(0.12)	(0.16)	(0.63)	(0.29)

Dependent variables in columns 1–3 are the vote share for Republicans in different federal elections. Dependent variables in column 4–5 are the log of per capita (per child population in column 4) expenditure. Columns 6–7 are the share of direct expenditure. All estimations are reduced form. Sources: Dave Leip's US Election Data; Annual Survey of State and Local Government Finances; US Census; ACS-5; USDA's Economic Research Service; Peter K. Schott's Data; County Business Patterns; Acemoglu et al. 2016 and QCEW. Panel 1 is reduced form estimation. Panel 2 estimates effect on lagged dependent variables. Panel 3 controls for pre-2007 features interacted with period dummies. Panel 4 includes simulated instrument following Borusyak and Hull (2020). Panel 5 controls for the stock of Mexican non-citizens at the beginning of period. Panel 6 excludes outliers and does not use predicted population weights. Standard errors clustered at CBSA level. Estimations control for county and state-period FE. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001