The Local Reaction to Unauthorized Mexican Migration to the US*

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Abstract

We study the political and socioeconomic impacts of unauthorized Mexican migration to the United States. Our shift-share identification strategy combines variation in migration inflows and migrant networks using novel administrative data to capture unauthorized migration. With rich county-level data, we document conservative electoral, legislative, and policy responses. Recent unauthorized migration increases the vote share of Republican candidates and induces more support for conservative legislation. It decreases total public expenditure, prompting reallocation away from education toward support for law and order. Three mechanisms partially explain these effects: job losses in "migrant-intensive" sectors; White flight and residential sorting; and higher out-group bias. **JEL Codes:** D72, F22, H7, H53, J61, J15

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1 Introduction

In 2014 American national politics turned renewed focus on immigration. Republican candidates for the United States House of Representatives barely mentioned issues related to immigration in the 2012 elections, but by 2014 nearly 20% campaigned on immigration issues, largely opposed to immigration.[1] The renewed interest targeted unauthorized migrants, characterizing them as largely criminals who endanger the safety of Americans. A decade later, more than half of all Americans believed that immigration should be decreased, a level not seen since the months immediately following the terrorist attacks in September 2001.[2]

While unauthorized migration has captured the political attention of Americans nationally, it is not clear how the presence of unauthorized migrants at the local level translates into political behavior. People could be responding to inflows somewhere else in the country, away from their home. It may be that the impacts of unauthorized migrants are similar to "low-skilled" migrants and refugees documented in other contexts (Otto and Steinhardt, 2014; Barone et al., 2016; Halla et al., 2017; Dinas et al., 2019; Edo et al., 2019; Tabellini, 2020; Mayda et al., 2022a,b). Areas with higher unauthorized migration might indeed observe more conservative attitudes and support for the Republican Party. Alternatively, close interactions with unauthorized migrants could be politically neutral or even shift attitudes to the left, similar to sustained contact with refugees and asylum seekers in Denmark and Austria (Dustmann et al., 2019; Steinmayr, 2021). Both of these kinds of interactions could be at work in the United States. In states like Georgia, migrants arrived throughout the period, and the state has become more liberal, while in a state like Ohio, unauthorized migrants arrived, and the state has become more conservative.

To illuminate these relationships, we study the response of US citizens to inflows of unauthorized Mexican migrants between 2010 and 2020. We focus on the Republican candidate's vote share in congressional elections, the voting behavior of House members, and public spending at the local level. We identify the response by predicting plausibly exogenous inflows of migrants using a confidential dataset on over 14 million consular identification cards issued to 7.4 million probably unauthorized Mexican citizens living in the US between 2002 and 2020. While versions of this data have been used (Allen et al., 2018; Caballero et al., 2018; Dinarte Diaz et al., 2022; Albert and Monras, 2022), we are the first to combine it with rich US administrative and survey data to estimate the political and socioeconomic effects of unauthorized Mexican migration to the US.

Since there is no systematic data on unauthorized Mexican migrants, the related literature has relied on indirect sources, like the American Community Survey (ACS) or the

[1] As measured by campaign ads, see Frangipane (2022).

[2] As the Gallup Immigration poll indicates.

Current Population Survey (CPS) (Warren and Passel, 1987; Passel and Cohn, 2015; Borjas, 2017; Borjas and Cassidy, 2019)—which approximates unauthorized migrants using, among others, low education attainment—or apprehensions at the border (Hanson and Spilimbergo, 1999). These samples are not representative. Not all unauthorized migrants are poorly educated, and those who are captured at the border may differ from those who enter successfully. Recent evidence suggests that these proxies are imprecise (Cascio et al., 2024). The consular data captures the population of interest better. Unable to obtain a formal identification from their US state of residence like authorized migrants, unauthorized migrants resort to the Mexican consular network to get a formal ID, necessary to carry out daily activities, such as banking. Like scholars with similar data, we assume that most consular cardholders are unauthorized migrants. Reassuringly, this data matches available estimates of the unauthorized Mexican population in the US (see details in Section 2.2).

Another advantage of the consular data is that it helps to address selection bias. Migration decisions are not random. Migrants may choose to settle in places that are politically welcoming or have economic opportunities. Simply comparing changes in vote shares, public expenditures, or unemployment rates between counties with higher and lower migration inflows would also capture the effect of other relevant county-level variables and trends like political polarization or de-industrialization. The geographic granularity and coverage of the consular data allow us to circumvent this bias.

Leveraging the Mexican municipality of origin and US county of residence of cardholders, we construct pre-existing migrant networks to use in two shift-share strategies. Our preferred shift-share specification uses a leave-one-out (LOO) approach. That is, we interact the initial municipality-county shares with migration inflows from every Mexican municipality to the US, net of those migrants who actually established residence in the core-based statistical area (CBSA) of each county.[3] Our second, more demanding, strategy exploits exogenous shocks by leveraging push factors from Mexico. We use the same initial municipality-county shares but predict migration flows based on time-varying Mexican municipality characteristics. Both shift-share strategies identify the effect of migrants who go to counties because of their migrant network. The compliers are people who settle in counties where they have strong networks, not necessarily where economic conditions are promising or the marginal product of labor is highest.

The identifying assumption is that the predicted number of migrants impacts the outcomes of interest only by its effect on observed migration. Since we exploit within county and state-period variation, we assume that the US county-level characteristics that attracted

[3]There are over 900 CBSAs in the US. On average, each CBSA includes around two counties. Rural counties do not belong to any CBSA. In those cases, we leave the county itself out.

Mexicans from particular municipalities in the pre-period do not affect the evolution of economic, political, and social characteristics of the county in later periods. We argue that the shifters used in both strategies—leaving a CBSA out and predicting migration from time-varying municipality characteristics—are exogenous to the trajectory of the outcomes of interest (Goldsmith-Pinkham et al., 2020; Borusyak et al., 2022). We present evidence that counters concerns of pre-trends, differential trends, and nonrandom exposure to migration (Borusyak and Hull, 2023). Moreover, the results are qualitatively unchanged when controlling for inflows of authorized migrants and "low-skilled" migrants, suggesting that our population of interest, and not others, causes the observed conservative reaction.

Our main results establish a conservative response in individual voting, Representatives' behavior, and fiscal policy. Inflows of unauthorized migrants increase the vote share for Republican candidates in House elections and increase legislators' support for conservative legislation. They also reduce local public spending, shifting it from education to law-and-order. A change in the inflow of migrants equivalent to moving from the first to third quartile (an increase of 0.1 percent relative to the county population) increases the Republican candidate's vote in midterm House elections by 0.9 percentage points. Between 2010 and 2018, 107 races, counted by county, were decided by smaller margins. Since there is not a significant increase in Republican registration nor greater identification with the Republican party, the increase probably comes from votes among independents and changes in the composition of the electorate. Our findings suggest that when specifically measuring the impact of unauthorized migration in the US, the political impact is larger, but substantially similar to responses to other populations in other settings.(Barone et al., 2016; Harmon, 2018; Dinas et al., 2019; Rozo and Vargas, 2021; Mayda et al., 2022a).

Moreover, House members in districts with larger migrant flows favor more conservative legislation. A 0.1 percentage point increase in the inflow of migrants moves the House members 0.04 units to the right,[4] a distance equivalent to the one that separated former Speakers of the House John Boehner (0.513) and Paul Ryan (0.556).

The impacts on public spending are consistent with the Republican agenda. A smaller government and a focus on law and order are two pillars of contemporary American conservatism. A 0.1 percentage point increase in the inflow of migrants reduces total direct spending (per capita) by 0.4% and education spending (per child) by 0.5%. These results align with theories suggesting that lower spending in response to migration operates through coordination failures, heterogeneity of preferences, and out-group bias (Alesina et al., 1999; Hanson et al., 2007; Facchini and Mayda, 2009; Card et al., 2012; Hainmueller and Hopkins, 2014; Alesina et al., 2022; Derenoncourt, 2022).

[4]As measured by the first dimension of a member's DW-NOMINATE score.

To uncover underlying mechanisms, we examine outcomes related to economic activity, demography, and values, using the same identification strategy. In the economic analysis, we find that migration inflows reduce formal employment in construction and hospitality and leisure—"migrant-intensive" sectors—increase poverty, and marginally reduce median household income. We also observe residential sorting. Migration inflows increase out-migration and lower the number of non-poor and White residents. Finally, the inflows cause a rise in out-group bias—captured by the relative universalism index (Enke, 2020). We do not find statistically significant effects on average employment, unemployment, wages or partisanship.[5]

Taken together, these results provide evidence in favor of both cultural and economic reasons behind the backlash against migration. While we cannot detail the timing behind the response, our interpretation is that there are three different yet related responses. (1) Unauthorized migrants may displace some competing US workers in industries with large informal sectors that are easily accessible to those without work authorization, like construction and hospitality. Largely displaced workers find employment in sectors like manufacturing, which are less "migrant-intensive" and have more formal employment. This job switching explains the lack of effects on unemployment and total formal employment. The temporary formal job loss probably prompts the transitory increase in poverty. (2) Citizens or established residents develop less favorable opinions of migrants. Economic grievance explains only a part of the political impacts, though. Our evidence on voter turnout demographics, consistent with Hopkins et al. (2024), suggests the strongest reaction probably does not come from the most economically affected group. Those most likely to be affected by job loss (Hispanic Americans) do not turn out to vote in larger numbers; they cannot be the ones driving the conservative reaction. (3) Instead, similar to the impact of the Great Migration (Boustan, 2010; Tabellini, 2018; Shertzer and Walsh, 2019; Derenoncourt, 2022), a subset of the population, particularly White residents, drive the reaction. They leave their places of residence. The out-migration of White, non-poor, non-Republican voters suggests that the compositional effects are as important as the conservative shift among those who stay.

Finally, we suggest policy implications. Many of the documented effects of unauthorized migration are more modest in counties with a stronger social safety net, measured by more progressive taxation structure, more redistribution relative to poverty, and a higher minimum wage. In these counties, the effect of unauthorized migration on the vote share of

[5] In earlier drafts, we explored whether an increase in crime could explain the conservative response. We did not find any evidence supporting this hypothesis. Given the well-documented data problems on sparsely populated areas (Gonçalves et al., 2024) in the context of immigration and the simultaneous increase in spending for policing and courts documented here, our preliminary approach was incomplete. Ongoing work investigates the relationship among unauthorized migration, crime, and policing in more depth.

the Republican candidates is between a half and a third smaller[6], and there is job gain in construction, only a marginal increase in poverty, and a decrease in out-group bias. Our interpretation is that these counties are better able to compensate those who lose economically. Progressive taxation does not mute the effect on out-migration; that is, residential sorting occurs regardless of the redistributive policies of a county.

This research contributes to multiple strands of literature. It is the first to analyze the political and social impacts of Mexican migration, whether authorized or not, to the US. The literature does analyze the labor market impacts of these inflows. Scholars have found that inflows of Mexican migrants have either no overall economic impact or small negative impacts in certain sectors and regions (Blau and Mackie, 2017; Clemens et al., 2018; Monras, 2020). Our results reinforce these findings.

This paper expands the literature on the political impacts of contemporary immigration to the US by documenting the effect of unauthorized Mexican migrants—the largest group of unauthorized migrants (Ward and Batalova, 2023)—across political activity: voting, legislating, and implementing policy. Two recent articles explore similar topics. Mayda et al. (2022a) study the impact of changes in the stock of "high-skilled" and "low-skilled" immigrants on political outcomes and find that the former shifts voters to the Democrats, while the latter shifts voters to the Republicans. Mayda et al. (2022b) also find that local expenditures decrease with higher "low-skilled" immigration and increase with higher "high-skilled" immigration.[7]

We add to this work by focusing analysis on inflows of unauthorized Mexican migrants. This group is spread across the US and is the focus of political contention. Political rhetoric is centered on the immigration status of migrants, not on their skill level. Unauthorized migrants are empirically and theoretically distinct from authorized migrants, refugees, or "low-skilled" migrants. Among the differences, unauthorized migrants experience higher barriers to formal employment and are not necessarily poorly educated. Given the barriers to employment, it may be that there is substantial overlap in economic responses to "low-skilled" migrants and unauthorized Mexican migrants. However it is not clear a priori that there should be the same political or social responses to these two groups. Low-skilled migrants may hail from anywhere in the world and are a culturally and linguistically diverse group. Meanwhile, political and social responses to unauthorized Mexican migrants are responses to a group that is linguistically and culturally distinct and that has a long history

[6] As Section 6.4 shows, with a back-of-the-envelope calculation, at most around a quarter of the political effect of migration could be attributed to job loss.

[7] Baerg et al. (2018) note that support for the Republican Party in Georgia tends to be higher in counties with higher shares of unauthorized migrants. Hill et al. (2019) note that changes in the shares of Hispanics between 2012 and 2016 negatively correlate with changes in the Republican vote.

of migrating to the US for jobs.

Our paper illuminates the mechanisms through which unauthorized migration influxes prompt conservative political shifts. We highlight the role of economic and cultural factors and the link between them and demographic change. We identify three channels informed by recent reviews (Rodrik, 2021; Alesina and Tabellini, 2024). First, despite aggregate gains in the long run, migration causes labor market frictions. Migrants compete with existing workers with similar skills, resulting in marginally higher unemployment or lower wages in the short run for these groups (Cortes, 2008; Burstein et al., 2020). Politicians may play to the worse-off group of voters by promoting policies against migrants (Müller and Schwarz, 2023; Hopkins et al., 2024).

Second, migrants' otherness prompts exclusionary attitudes (Brader et al., 2008), potentially offsetting more welcoming attitudes arising from increased contact (Enos, 2014). Established residents may prefer lower redistribution to ethnically different people (Alesina et al., 1999; Alesina and Giuliano, 2011) or they may want to preserve their power in a polarized environment (Bazzi et al., 2019). Established residents who are unwilling to interact with newcomers or prefer to preserve the composition of their communities (Card et al., 2012) might decide to move, causing demographic change. Boustan (2010) and Shertzer and Walsh (2019) document the movement of White people from northern US states out of their communities as a response to the Black Great Migration. Since those who left were comparatively wealthier, White flight caused a decline in revenues and public expenditures (Tabellini, 2018).

Third, migrants change attitudes or (mis)perceptions. Established residents may assign negative characteristics to migrants (Hainmueller and Hopkins, 2014; Alesina et al., 2022). Negative perceptions include the belief that migrants threaten residents' jobs (Ajzenman et al., 2022), increase crime (Ajzenman et al., 2023), or do not contribute economically to a community. These attitudes lead citizens to vote for anti-migrant politicians and policies. The media can enhance negative perceptions of migrants (Abrajano and Hajnal, 2017; Couttenier et al., 2021; Djourelova, 2023) and politicians may leverage these perceptions to gain office (Alsan and Yang, 2024; Baccini and Weymouth, 2021; Rozo and Vargas, 2021). The logic of why established residents acquire these negative associations is similar to a class of explanations for immigrant backlash in political and social psychology driven by intergroup threat (Riek et al., 2006; Mutz, 2018). Economic vulnerability further enhances the perception of threat (Dustmann et al., 2019; Margalit, 2019).

This paper helps to unpack the relative importance of these theories. We find evidence that economic factors are only partially responsible for the conservative political reaction and that preferences, unrelated to economic factors, are relevant. The economic impacts we estimate are small, concentrated in a few sectors and demographic groups, and do not translate into an average decline in employment, suggesting that there is job switching, and that there are economic winners. Furthermore, turnout of White voters increased, despite this group not being the most economically affected. Nevertheless, due to a lack of data, we cannot rule out that perceived threat motivates the conservative response.

The paper presents preliminary evidence on policy to counter backlash. Our findings suggest that a more robust safety net can protect existing workers affected by migration, reduce out-group bias, and curtail support for reactive politics and policies. These results contribute to the literature linking safety nets and political attitudes, both in wealthy (Fetzer, 2019) and developing countries (Zhou et al., 2023).

In the remainder of the paper, we introduce a novel dataset and demonstrate its appropriateness for our research question. We explain our shift-share instruments and examine the key identifying assumption. We establish that flows of migrants shift voters toward the Republican Party and drive more conservative public spending, and discuss the robustness of our results. The penultimate section explores and compares three sets of mechanisms, highlighting economic disruptions, demographic change, and a shift in values. The last section explores heterogeneous effects across measures of the social safety net and discusses implications.

2 Data

Since the mid-1800s, the Mexican government has offered identification cards to its citizens living in the US, regardless of their immigration status (Laglagaron, 2010; Márquez Lartigue, 2021). With the Patriot Act in 2001, requirements for identification became more stringent in the US, so migrants without immigration authorization had even more limited access to US-issued identification cards, making them virtually unable to access some basic services, such as banking or housing (Bruno and Storrs, 2005; Mathema, 2015). In 2002, the Mexican government responded by strengthening 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 plastic card with several authentication mechanisms (Bruno and Storrs, 2005). 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 35 USD. IDs are valid for five years, and the renewal process is identical. There are no immigration status requirements. While there is no data, anecdotal evidence suggests that nearly everyone with the necessary documentation is able to get the ID. Issuing consular IDs is central to consular activities, so much so that most of the personnel working in the consular network is employed issuing either passports or consular IDs.

The updated administrative database is the source of our data. The dataset has information 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. 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 anonymized demographic information 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 and thus identify newcomers vis a vis renewals. This number has no relevant meaning nor is it linked in any form to other demographic information. The original data consists of 16.7 million observations corresponding to 8.8 million individuals.

2.1 Constructing Migration Flows with Consular Data

To estimate the number of new unauthorized Mexican migrants from the consular data, it is necessary to differentiate between renewals and first-timers and make assumptions about the likelihood that an average newcomer applies for an ID and remains in the same county. The assumptions to identify newcomers are weak. In contrast, the assumptions to determine who stays in the county are strong because it is impossible to differentiate those who do not renew from those who leave. Our consular dataset is better suited to study flows than stocks of migrants.

We construct the flows of newcomers in a series of steps. First, we count the number of new cards in 4-year intervals, 2007–10, 2011–14, and 2015–18.[8] This frequency is convenient because it allows us to observe the total inflows 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 remain at least in the same state over those five years (Allen et al., 2018; Caballero et al., 2018). Second, for each period, we classify as newcomers the people who got an ID for the first time in a new core-based statistical area (CBSA), a geographical unit 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 2.13 million newcomers arrived between 2007 and 2010, 1.3 million between 2011 and 2014, and 0.95 million between 2015 and 2018. Figure A1 shows that these numbers are consistent with other common estimates (Passel

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

and Cohn, 2018; Baker, 2021; Wassink and Massey, 2022; Ward and Batalova, 2023).

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 (2010, 2014, or 2018) using population estimates from the US Census. Figure 1 shows the national distribution of unauthorized Mexican migrants as a fraction of county population. The average is 0.69% for the first period, 0.4% for the second, and 0.28% for the third. Between 2007 and 2020, there were migrants in 2,674 US counties,[9] 88% of the total.

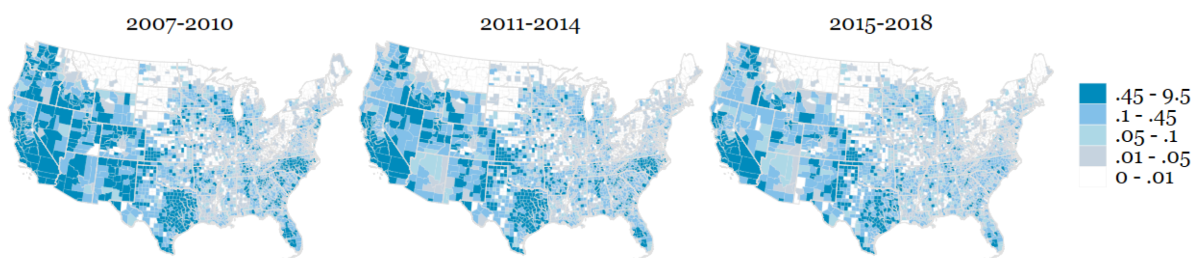

Figure 1: Observed recent unauthorized Mexican migrants as percent of county population.

Sources: SRE and US Census Bureau, Population Division

2.2 Validating Migration Flows Created with Consular Data

Because migrants with valid visas or work authorization have access to identification from US authorities, the working assumption among scholars using versions of the consular data is that it captures predominantly unauthorized migrants (Bhandari et al., 2021; Albert and Monras, 2022). To validate it, these scholars have compared it with estimates obtained from ACS and CPS using variations of the "residual method," which relies on criteria like country of birth, educational attainment, year of arrival in the US, US citizenship, and reception of governments benefits (Warren and Passel, 1987; Passel and Cohn, 2015; Borjas, 2017; Allen et al., 2018; Caballero et al., 2018; Baker, 2021). Cascio et al. (2024) shows, however, that the residual method is not accurate since the estimates are not very sensitive to immigration reforms. Moreover, the assumptions made in constructing the population matter significantly (Van Hook et al., 2021). In Appendix A.2 we show that our data correlates quantitatively and qualitatively with estimates created following Allen et al. (2018), as described before. The correlations are weaker if, like Mayda et al. (2022a,b) we restrict the sample to those with no completed high school education.

Another potential concern is that, even if the consular data measures unauthorized migrants, it also captures authorized migrants. If that were the case, it would be hard to

[9]To protect people living in areas with very few migrants, we only consider counties with more than 10 migrants from 2002 to 2020. We exclude Alaska because counties changed over this period.

determine whether the effects we observe are due to authorized or unauthorized migration. In Table A2, we explore this possibility. Using the detailed data from the 441 counties covered by ACS 5-year data in Integrated Public Use Microdata Series (IPUMS), we regress the estimate of unauthorized Mexican migration described before and an analogous estimate of authorized migration on our preferred instrument. The correlation between our instrument and the estimate of unauthorized migrants is strong, whereas the relationship between the instrument and the estimate of authorized migrants is weak and not statistically significant.[10]

Selection bias at the county level is another threat to the validity of the data to construct unauthorized migration *networks* across counties. The potential problem is that, regardless of the number of migrants in a county, differences in the policy environment could affect the incentive to request an ID. Those policy environments could, in turn, reflect the evolution of our outcomes of interest. We assume that migrants get consular IDs to access basic services, like banking, and to send remittances to Mexico, almost regardless of the local policy environment. Appendix A.4 tests this assumption by observing the evolution of IDs after some states made driver's licenses IDs available to unauthorized migrants and counties activated Secure Communities, a program where local police submit individuals to federal authorities for deportation review. Demand seems responsive to the driver's license changes but only in the short run, and unresponsive to Secure Communities. While we cannot test for policy changes between 2002 and 2006, the years used to construct the networks, these two results suggest that demand is rather inelastic to the local policy context 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 Mexican migrants do. Importantly, given our identification strategy, the local contemporary policy environment does not affect our instrument, as we rely on past networks and national inflows of migrants. The robustness exercise shows that the initial shares by themselves cannot account for the observed results. Last, our specifications control for state-by-period fixed effects.

2.3 Dependent Variables

We use two sets of dependent variables in our primary analysis. First, we examine the impact of migrant flows on the vote share for the Republican candidates in elections for the House of Representatives. The electoral data comes from Dave Leip's Atlas of U.S. Presidential Elections (Leip, 2022). We focus on elections in the midterm years of 2010, 2014, and 2018. Our focus is on midterm elections for conceptual reasons and because of limits in our

[10]Table A2 indicates that a marginal increase in the LOO instrument is associated with an expected increase of 0.503 in the proxy of unauthorized migrants—F statistic 198. In contrast, a marginal increase in the LOO instrument is associated with an expected increase of less than 0.001 in the proxy of authorized migrants—F statistic less than 0.01.

data. Our interest is in understanding local political activities, and House elections capture who a local population selects to represent them in the national government. Furthermore, presidential elections often impact down ballot races and reflect individual characteristics as much as party characteristics. Because of the timing of our migration data, presidential elections occur in the middle of the following period, while midterm House elections occur at the end of the current period. This means that the relationship we estimate for presidential election years is in part a response to flows we attribute to the next period. Generally, our findings about presidential years are consistent with those in midterm elections, but are smaller and less robust.

Second, we study legislators' revealed preferences with DW-NOMINATE scores, a measure of a House member's revealed policy preferences on a left-right continuum based on roll-call voting (Lewis et al., 2021; Canen et al., 2020). These scores reflect roll-call voting of members of the House. We focus on the first dimension of scores that measure a candidate's economic or redistributive preferences over their time in office (Poole and Rosenthal, 2000). In our secondary analysis, we explore the second or social dimension of these scores which includes racism or xenophobia, as well as the newer Nokken Poole scores. The Nokken Poole scores are designed to be more dynamic, allowing representatives' scores to vary year to year. We follow Ferrara et al. (2024)'s 'crosswalk' from the county level analysis to the congressional district level. We use the boundaries corresponding to the elections of 2010, 2014, and 2018 and keep counties that, using the m2 weight, are more than 85% inside a congressional district or that provide more than 85% of the population of the district. We keep districts for which we found more than 85% of their population and for which there is at least one period with unauthorized migration. This process yields an unbalanced panel of 598 congressional districts.

Third, we examine public good provision with county-level revenue and expenditures, and focus on spending in public education and policing and the judiciary (we combine these and call it law and order). The data comes from the Annual Survey of State and Local Government Finances. Our goal is to investigate changes in policy at the local level, regardless of the specific government agencies that carry them out. Therefore, we aggregate all local expenditures, by topic, within each county. This includes spending by the county government, cities, townships, special districts, and school districts.

We focus on 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. On average, in our sample, 40% of the total direct expenditures within counties is for education. One limitation of this dataset is that, except for school districts, it surveys all the local agencies only in years that end in 2 and 7. For the rest of the years, the estimates are based on a subsample of the most populous areas (Annual Survey of State and Local Government Finances, 2010). We use data for 2012 and 2017 and estimate the effect of newcomers in the period 2007–10 on expenditures in 2012 and of newcomers in the period 2011–14 on expenditures in 2017. Table A3 presents descriptive statistics for these variables, the instruments, and the endogenous variable.

3 Empirical strategy

To estimate the effects of unauthorized migration on political outcomes, we require a source of exogenous variation. Comparing counties with more and fewer unauthorized migrants would provide biased estimates, since the number of migrants that counties receive is potentially endogenous. For example, migrants may select into places that are more economically promising or more friendly toward migrants (Cadena and Kovak, 2016), and these factors in turn may vary with our outcomes of interest. To address this bias, we use two shift-share strategies that differ with respect to the measurement of the shifters.

Shift-share strategies predict treatment by combining a measure of initial exposure that varies cross-sectionally, the *shares*, with an aggregated shock that varies in time, the *shift*. In our setting, initial county exposure is given by the networks of unauthorized migrants coming from different Mexican municipalities.

Our two shift-share strategies rely on the same shares but use different shifters. The main strategy uses, in the spirit of Tabellini (2020), a leave-one-out approach. Namely, the shifters are the inflows of all migrants arriving in the US from a Mexican municipality m, excluding the flows to the county of interest. The second strategy, in contrast, predicts migration flows from every Mexican municipality m using time-varying push factors, likely exogenous shocks outside the US.

Equation 1 details the second stage estimation, common to both strategies:

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

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 unauthorized Mexican migrants as a 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, common to both strategies:

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

where Z_{cst} is the shift-share instrument, with either leave-one-out or push factor shifters. ϕ_c

are county fixed effects and π_{st} are state-period fixed effects.

The first step for both strategies is to construct the endogenous variable, the observed number of migrants, as defined in Section 2.1. We count the unique new consular IDs in every US county during each of the periods 2007–10, 2011–14, and 2015–18.[11]

The second step, again common for both strategies, is to create pre-period shares using the first five years of data (2002–2006). We count all the individuals who 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, California, 7.5% in Ventura County, California, and 5.8% in Milwaukee County, Wisconsin.

For the leave-one-out strategy, the next step is to multiply the original fraction of migrants from municipality m living in county c by the total number of migrants from municipality m who entered the US during that period, net of those who eventually settled in that county's CBSA. This is the leave-one-out component. There are a few counties that do not belong to any CBSA. For those, we leave out only 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 instrument. For example, we count 550 people moving from Alvarado to 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×(550–52)), 39.8 (0.075×(550–21)), and 26.6 (0.058×(550–93)) respectively.

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

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

[11] We drop individuals who get a new ID in the same period in a different county of the same CBSA because we cannot rule out a simple change of address.

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 who migrated to county's CBSA.

For the push factor strategy, we follow Munshi (2003) and predict the observed number of migrants from Mexican municipality m during each four-year period t using time-varying variables—historical and contemporary climate and precipitation; infant, child, and maternal deaths and death rates; poverty and social development indicators—and indicators of economic activity, like the number of firms and total production. To avoid over-fitting, we select the most relevant predictors using LASSO. Since the number of migrants is censored at zero, we estimate a Poisson regression. Appendix A.7 describes the variables used for this instrument in detail. Equation 4 describes this "zero stage" exercise.

$$PredictedMigrants_{mt} = \alpha_0 + \mathbf{X}_{mt} + \xi_{mt} \tag{4}$$

where X_{mt} is the battery of municipality time-varying variables.

The instrument is given by interacting the predicted number of migrants from m in period t with the original pre-period shares. Equation 5 describes the instrument.

$$Z_{cst} = \frac{1}{\widehat{P}_{cst}} \sum_{m} Sh_{mcs,2006} * Predicted\widehat{M}igrants_{mt} \tag{5}$$

where all the terms are just as in equation 3. Continuing with the example, we predict 781.3 people from Alvarado. The predicted migration in each of the main destination counties is LA 72.1 (0.092×781.3), Ventura 58.7 (0.075×781.3), and Milwaukee 45.4 (0.058×781.3).

Following recent developments in the literature (Blandhol et al., 2022), we do not control parametrically for covariates; we include county and state-by-period fixed effects. Map A6 displays the variation we exploit.

3.1 Identifying Assumptions

To provide causal estimates, at least one of the components of shift-share designs must be exogenous (Goldsmith-Pinkham et al., 2020; Borusyak et al., 2022). Since we have panel data and exploit only within county variation, the exogeneity in this setting relates to changes in the outcomes, rather than to their levels. Our assumption is that the shifters in both strategies are exogenous. We argue that by excluding the CBSA of the county of interest or by using Mexican municipality push factors, the constructed shifter is uncorrelated with any unobserved factors in the residuals. For the leave-one-out instrument, a key component of this assumption is that the numbers of migrants are not spatially correlated among CBSAs

(Borusyak and Hull, 2023).[12]

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). We assume that the observed differences in the variables of interest are solely due to the instrument via the endogenous variable. Namely, conditional on county and state-by-period fixed effects, predicted migration affects the evolution of political outcomes only through observed migration.

There are two main threats to identification. (1) 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 were on different trajectories or if the initial shares had persistent effects. (2) 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 between counties were markedly different, (b) the composition of the Mexican shares was correlated with the outcomes 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 had stronger networks in more conservative US counties and the people from southern Mexico, with a comparable population, had 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.

We provide evidence to allay these concerns. For pre-trends, we analyze the association between the change in the instruments and the change lagged outcomes. For differential trends, we interact key pre-period characteristics with period indicators. We construct a simulated counterfactual instrument, as proposed by Borusyak and Hull (2023) to deal with possible non-random exposure.[13] Controlling by this simulated variable helps to examine whether the results are solely driven by the initial shares. Our main results are largely robust as displayed in Table B2.

Finally, we analyze the concentration of migrant networks by county. The predicted Mexican migrant composition in counties is not excessively concentrated. Taken all periods together, the top 50 sending municipalities account only for 31% of all predicted migrants and have migrants living in over 560 counties. The average county has predicted migrants from

[12] The potential for spatial correlation is why we leave out the CBSA, rather than only the county itself. While there is some spatial auto-correlation in the number of newcomers among counties (Moran's I between .44 and .3), the correlation among CBSAs is lower (Moran's I between .21 and .18). As Table B2 shows, our results are robust to controlling for a spatial lag.

[13] We explain further in Subsection 5 and B.1.

around 95 municipalities. In Figure B1 we calculate the Rotemberg weights, as suggested by Goldsmith-Pinkham et al. (2020). The top 17 Mexican municipalities account for only 30% of the positive weight in the instrument.

3.2 First Stage

The stability of the migration patterns results in a strong first stage. For comparison purposes, Table 1 presents four instruments. Column 1 shows the results for the least conservative instrument. Nearly identical to the leave-one-out instrument, this one leaves out the county rather than the CBSA. The instrument in Column 3 leaves the whole state out.

Column 2 presents our preferred instrument, the leave-one-CBSA-out. Conditional on county fixed effects and state-by-period fixed effects, a 1 percentage point increase in the leave-one-out instrument is associated with a 1.16 percentage point increase in the observed share of newcomers. The F-statistic of the instrument is 822. Column 4 displays the results of the push factor shift-share instrument. Despite exploiting variation in Mexican municipalities, the first stage is also strong—F-statistic of 625. Throughout the rest of the paper, we use the leave-one-CBSA-out as our preferred specification, but also present the push factor instrument.

The Local Average Treatment Effect (LATE) that these instruments identify is specific. Our estimand is the effect of flows of Mexican newcomers who migrate to counties where they have networks, not the effect of random inflows. The migrants we study are those who settle in areas where people from their municipalities settled in the past. By construction, this strategy does not capture the effect of the most economically efficient migration—towards locations where the real wage is highest.

Table 1: First stage

	(1) LOO, county out	(2) LOO, CBSA out	(3) LOO, state out	(4) Push factors
Newcomers, percent population	1.116^{***} (0.038)	1.160^{***} (0.040)	1.315^{***} (0.067)	1.338^{***} (0.053)
Observations	8019	8019	8019	8019
F statistic	880	822	386	625
Mean of Dep. Var	0.463	0.463	0.463	0.463
Mean of Ind. Var	0.421	0.404	0.318	0.442

Column 1 displays the results for a leave-one-out (LOO) shift-share regressor that leaves the county 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 emigration flows by municipality using push factors like poverty and homicide rates, economic activity, and variation in temperature and precipitation. Standard errors clustered at the CBSA level. All regressions have county and state-period fixed effects, and are weighted by predicted population. Stars indicate *p<0.1,**p<0.05,***p<0.01.

4 Main Results

In this section, we examine the impact of recent unauthorized Mexican migration on electoral behavior, legislative votes, and fiscal policy. Flows of recent unauthorized migrants prompt more voters to choose Republican candidates. Voters' chosen legislators vote more conservatively in Washington because of the migrant flows, and local spending shifts in ways consistent with the fiscal conservatism and the law-and-order policies of the Republican party.

4.1 Electoral Behavior

Table 2 displays the estimated impacts of unauthorized migrant arrivals on Republican candidate vote share in elections for members of the House of Representatives. The first two columns estimate the impacts on vote share for the Republican candidate in midterm house elections and presidential year elections, respectively. We study midterm and presidential years separately because midterm elections occur at the end of our periods. Presidential elections occur two years later. The timing means that our estimate of arrivals does incorporate two additional years of flows that may be influencing presidential year elections. These elections may also not reflect local politics. Presidential candidates influence House elections in presidential years. The estimates may reflect individual rather than party differences (Campbell, 1987; Knight, 2017).

Throughout the paper, we interpret effects in terms of a change in inflows of 0.1 percentage points (pp), equivalent to going from the top of the first to the top of the third quartile. The observed change is negative, as migration decreased in the period studied. For ease of interpretation we focus on the effect of increases. Below the standard errors of the estimations, we report standardized coefficients and effects of flows equivalent to an interquartile range change ($\hat{\beta} \times P(75) - P(25)$). 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 inflows of unauthorized migrants. The impact of a 0.1 pp increase is more informative. Throughout the paper, each table presents three sets of estimates. In Panel A, we show Ordinary Least Squares (OLS) estimates for a baseline comparison. Panel B displays second-stage estimates from our preferred specification, the leave-one-out shift-share (LOO) instrument. Panel C displays second-stage estimates from the push factors instrument. We focus on population-weighted estimates because these estimates are often more precise and robust than the unweighted estimates, and more informative about the effects on the country as a whole.

Panel A of Table 2 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 IV estimates. The House midterm relationship is the largest (Column 1). A 1 percentage point increase in unauthorized migrants is associated with a 6.51 point increase in the share of votes that go to Republicans. Presidential year relationships for House candidates are smaller in magnitude but remain statistically significant.[14]

Table 2: Political effects of arrival of unauthorized Mexican migrants in House elections 2010-2020

	Share		Score
	(1) Midterm	(2) Presidential Year	(3) DW-NOMINATE
A. OLS			
Newcomers, pct. pop.	6.51***	2.81***	0.33**
	(0.87)	(1.06)	(0.15)
B. 2SLS LOO			
Newcomers, pct. pop.	8.50***	3.49***	0.40**
	(1.03)	(1.21)	(0.15)
Std. Coefficient	0.26	0.10	0.47
$\hat{\beta} * P(75) - P(25)$	0.91	0.37	0.04
C. 2SLS Push Factors			
Newcomers, pct. pop.	7.77***	3.28***	0.31*
	(1.14)	(1.27)	(0.16)
Std. Coefficient	0.24	0.10	0.37
$\hat{\beta} * P(75) - P(25)$	0.83	0.35	0.03
Observations	7995	8015	524
Dep. Var., Mean	48.16	47.24	0.06
Dep. Var., Sd	19.44	19.92	0.46
Ind. Var., Mean	0.46	0.46	0.53
Ind. Var., Sd	0.59	0.59	0.57
Inst. Loo, Mean	0.41	0.40	0.39
Inst. Loo, Sd	0.55	0.55	0.45

Dependent variables in Columns 1 and 2 are share of Republican vote. Dependent variable in column 3 is the DW-NOMINATE score. Sources: Dave Leip's U.S. Election Data, Lewis et al. (2021), Ferrara et al. (2024). 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. Instruments are as described in Section 3. All regressions have county and state-period fixed effects (congressional fixed effects in Column 3), and are weighted by predicted population. Standard errors are clustered at the CBSA level (congressional level in Column 3). Stars indicate *p<0.1,**p<0.05,***p<0.01.

Panels B and C indicate that an inflow of unauthorized migrants is causally linked to electoral behavior. In House midterm elections, a 0.1 pp increase in the flow of migrants, using the LOO instrument, causes a 0.91 point increase in vote share for Republicans (Panel B, Column 1, std coeff: 0.26). In presidential years, it causes a 0.37 point increase in vote share for Republican House candidates (Panel B, Column 3, std coeff: 0.10). In turn, a 0.1

[14]We examine the presidential election in a separate analysis. The results are more comparable to House candidates in presidential years, but they are less robust.

pp increase, using the push factor instrument, causes a 0.83 point increase in Republican vote share in House midterm elections (Panel C, Column 1, std coeff: 0.24). Generally, estimates from the instruments are statistically indistinguishable from each other. Table C1 presents the effect of unauthorized Mexican migration on Presidential elections, Senate elections, and Gubernatorial elections. Unauthorized migration increases the vote share of the Republican Party in all these cases.

These effects are consistent, yet larger with electoral reactions in other settings (Barone et al., 2016; Edo et al., 2019; Tabellini, 2020). Mayda et al. (2022a) estimate that a 1 pp. increase in the share of "low-skilled" migration increases the vote share for the Republican candidates, in all federal elections between 1990 and 2016, by 4.5 pp. Our estimates are twice as large. As compared to low skilled migrants, the arrival of unauthorized Mexican migrants may have different job market, social, or political impacts for reasons as varied as a lower reservation wages or more targeted negative media coverage. We analyze changes in flows, not stocks. Finally, we focus on a time period in which the Republican party has turned more anti-immigration.

These political effects could reflect higher Republican turnout, changes in partisan preferences, or a shift in the composition of the electorate. We use available data to unpack some of these factors indirectly. We examine the effect of flows of newcomers on registered Republicans (for a subset of states) in Table 7. We find evidence consistent with changes in the composition of the electorate, but not partisan preference or Republican turnout. We do observe a small, less robust increase in Republican registration. Together these results suggests that partisan alignment does not explain the change in voting behavior. Republican candidates accumulate more votes, probably because of independents choosing them, but not because of large numbers joining the party.

Even though the impact on Republican vote share is large, inflows of migrants are small relative to population. These may not alter the outcome of any House election or the composition of the House. Our effect could be coming from already secure Republican counties. To examine this question we estimate the impact of unauthorized migrants on midterm House elections according to past political behavior of the county—whether the Republican candidate won that county in the 2006 House election, the Republican vote share in the 2006 House election, and the closeness of the 2006 election. Figure D1 presents the effect of the interaction between the LOO instrument (in reduced form estimation) with the corresponding indicator of previous political behavior. The interactions are small in magnitude, not statistically significant, and do not indicate any pattern. The results suggest that recent unauthorized Mexican migrants move county vote share to the right regardless of past partisan leaning.

It could also be that races are not sufficiently close that migrant flows determine the election. Between 2010 and 2018, 107 of the midterm races we study at the county-level were decided by less than 0.91 pp. Generalizing from counties to congressional districts is difficult, but the case of Florida-2 indicates the potential for impact. In the 2014 election, the congressional district comprised voters from 14 counties, 12 of which were totally inside the district.[15] The combined inflow of new migrants in this group of 12 counties decreased by 0.036 pp relative to 2010. The predicted effect of the drop is around 0.31 pp (-0.036×8.5) or roughly 765 fewer votes for Republicans. The Democratic candidate, Gwen Graham, won that race by 0.8 pp, around 2,200 votes. Our estimates suggest that a third of the difference can be explained by a reduction in the arrival of new unauthorized migrants. (Ferrara et al., 2024)

4.2 Legislative Change

We examine whether members of the House of Representatives are more conservative because of unauthorized migrant flows. Even though a larger proportion of the vote is going to the Republican party, this does not mean that voters are sending more conservative representatives to Washington to legislate. Even if the shift in vote share was decisive in some close elections, this might not change the activity of representatives in Congress at all. We find evidence that unauthorized migrant flows cause a shift to the right in DW-NOMINATE scores, a measure of a House member's revealed policy preferences on a left-right continuum based on roll-call voting.

The third column of Table 2 displays the result of the analysis of DW-NOMINATE scores. We focus on the first dimension of the DW-NOMINATE scores, which captures economic voting (i.e. redistribution). A larger number reflects more conservative orientation or a preference for less redistribution and smaller government. In order to study the Representatives' behavior we must analyze our data at the Congressional district level. The change in level of analysis means that we must use the leave-one-state-out instrument, since congressional districts can be larger than a county, and use congressional district, rather than county fixed effects. These changes decrease the number of observations and the statistical power we have to examine the member's behavior. The push factor instrument is constructed as before. The first stage of both instruments is in Table A5.

Unauthorized migrant flows increase House members' DW-NOMINATE scores, meaning that representatives vote for less financial redistribution in Congress. For the leave-one-out instrument, we observe a positive, statistically significant shift in favor of more conservative legislative. A 1 pp increase in the share of unauthorized migrants in the districts results

[15] The other counties provided around 2.5% of the population of the district (Ferrara et al., 2024).

in a shift of a representative's score of 0.04 units toward more conservative. This shift is equivalent to about one-tenth of a standard deviation of the variation in scores among house members during the period studied. It is approximately equal to the distance between former Republican Speaker of the House John Boehner (0.513) and his successor, Paul Ryan (0.556). These results reveal that unauthorized migrant flows not only shift voters to the right, but also shift the elected representatives to the right.[16]

4.3 Policy Change

To explore whether the observed effects on federal elections reflect local conservative response, we study the impact of new arrivals on county-level public expenditures. Examining public spending allows us to explore whether the inflow of new unauthorized migrants reduces the provision of local public goods, as per the predictions of Alesina et al. (1999). We are also 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. This exercise helps to connect preferences in national elections with changes in local policy.[17]

Table 3 presents the effects of inflows of new unauthorized migrants on revenue and expenditures. The results present a pattern similar to that of the impact on voting. The OLS estimates provide a baseline suggesting a bias toward zero (Panel A), consistent with the hypothesis that migrants self-select into more politically welcoming areas. Second stage estimates (Panels B and C) are larger in magnitude and, in general, precisely estimated. Direct expenditure (Column 2) decreases in response to recent inflows of unauthorized migrants. This shift is consistent with conservative policy and a preference for less redistribution. A 0.1 pp rise in the flow of unauthorized migrants reduces direct expenditure per person by 0.4% (Panel B, Column 2, std coeff: -0.07). Since many local governments have balanced budget requirements, expenditures and revenues should move together (Ebel et al., 2012). Indeed, as Column 1 indicates, a 0.1 pp increase in the inflow of unauthorized Mexican migrants reduces revenues per person by 0.3% (Column 1, std coeff: -0.05).

It is hard to infer from these two estimates whether the conservative reaction reflects a policy preference. An alternative explanation could be that both revenues and expenditures decrease as a result of an external, seemingly unrelated factor, like a drop in intergovernmen-

[16]We do additional analysis with House members voting (reported in Table C3). The second dimension of DW-NOMINATE scores captures social roll-call voting, including voting related to racism or xenophobia. We find no effect of unauthorized migrant flows on the second dimension. Researchers often discount the salience of this dimension following the civil rights movement in the 1960s. The null result may reflect the lack of relevance of the measure or indicate that Representatives do not change their behavior on this dimension. We also analyze the impact of migration inflows on Nokken Poole scores, which allow a representative's score to vary over time. These results are largely consistent with DW-NOMINATE results.

[17]Migration policy may reflect national or international political objectives (Camarena, 2022), but voters seem to hold central governments accountable for local migration dynamics (Kreibaum, 2016).

tal transfers. In Table C4 we present the impact of unauthorized migration on the different components of local revenues. We find a decline in own source revenue, especially income tax, and no effect on intergovernmental transfers, which suggests an impact on local policy decisions.

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

		Expenditures (log per person 2010 USD)		
	(1) Revenue	(2) Total	(3) Education	(4) Law and Order
A. OLS				
Newcomers, pct. pop.	-0.02*	-0.02*	-0.03**	0.02
	(0.01)	(0.01)	(0.01)	(0.02)
B. 2SLS LOO				
Newcomers, pct. pop.	-0.03**	-0.04***	-0.05***	0.05**
	(0.01)	(0.02)	(0.02)	(0.02)
Std. Coefficient	-0.05	-0.07	-0.08	0.08
$\hat{\beta} * P(75) - P(25)$	-0.003	-0.004	-0.005	0.006
C. 2SLS Push Factors				
Newcomers, pct. pop.	-0.03**	-0.04**	-0.03**	0.04*
	(0.01)	(0.02)	(0.01)	(0.02)
Std. Coefficient	-0.05	-0.08	-0.05	0.06
$\hat{\beta} * P(75) - P(25)$	-0.003	-0.005	-0.004	0.004
Observations	5338	5338	5334	5338
Dep. Var., Mean	8.48	8.44	8.86	5.71
Dep. Var., Sd	0.38	0.38	0.43	0.47
Ind. Var., Mean	0.55	0.55	0.55	0.55
Ind. Var., Sd	0.68	0.68	0.68	0.68
Inst. Loo, Mean	0.49	0.49	0.49	0.49
Inst. Loo, Sd	0.63	0.63	0.63	0.63

Dependent variables in Columns 1–5 are in log 2010 dollars per person, 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, and other categories as described in section 3. Education expenditures include all public education expenditures of the county. Law and order refers to police and judicial expenditures. Police expenditures include city police spending in a county and 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. Instruments are as described in Section 3. All regressions have county and state-period fixed effects, and are weighted by predicted population. Standard errors are clustered at the CBSA level. Stars indicate *p<0.1,**p<0.05,***p<0.01.

As hypothesized by Alesina et al. (1999), this decrease in spending masks heterogeneity between categories. Migration inflows generate a reallocation across local public goods, away from "productive expenditures." An inter-quartile range change in the flow of migrants prompts a 0.5% reduction in spending per child on public education (Column 3, std coeff:

-0.08), and increases in spending per person on policing and courts (0.5%). In response to new unauthorized migrants, local politicians limit spending on public education and invest in security. These allocations are consistent with the small government and law-and-order stance of the Republican party.

Since local revenue and expenditure processes vary considerably within and between states (Martell and Greenwade, 2012), our interpretation is that these results are consistent with Republican fiscal policy in favor of smaller government and greater spending to support law and order. Voters who shift toward Republican candidates for Congress probably also shift toward Republicans down the ballot. This means more Republican candidates in local and state positions. These politicians can make changes on the margin in the short run. The changes in education, police, and judiciary spending we identify probably reflect Republicans' collective efforts. To compare, Mayda et al. (2022b) find that a 1 pp increase in the population of "low-skilled" immigrants to the US between 1990 and 2010 reduced local per capita revenues and expenditures by 2.7 and 1.8% respectively.[18] They do not find significant effects on spending in education or policing and courts.[19]

Our findings on public spending are consistent with the ethnic heterogeneity and polarization (Alesina et al., 1999; Bazzi et al., 2019), compositional amenities (Card et al., 2012) and out-group bias (Riek et al., 2006; Derenoncourt, 2022; Ajzenman et al., 2023) mechanisms. Unauthorized migration causes divestment in education, the largest local-level productive expenditure, suggesting that residents may prefer to limit redistribution to the out-group. The effects on the relative investment in policing and the judiciary could indicate an increased perception of threat.

5 Robustness Checks

Our empirical strategy relies on the assumption that, conditional on the fixed effects, the observed impacts are generated by the instrument via the endogenous variable. We assume that counties with more predicted migrants were not already on a different trend due to, for example, persistent impacts of the initial shares or the evolution of other confounding variables (Goldsmith-Pinkham et al., 2020; Borusyak et al., 2022).

To explore pre-trends, we regress the pre-period change in the main outcomes, suggested mechanisms, and other socioeconomic variables on the changes in the instruments in our periods of analysis. As figure 2 indicates, there is no statistically significant association between the instrument and either of the main variables. Further, there is no statistically

[18] A 1 pp inflow of unauthorized Mexican migrants would correspond to a drop of 2.8% in per capita revenues and 4.1% drop in per capita direct expenditure.

[19] Like the earlier comparisons, this difference may be due to differences in the parts of the study design, that is, populations of study and type of independent variable.

significant association with the variables explored in the mechanisms, except for people in poverty. However, the association is in the opposite direction and, with the push factors instrument is not robust to multiple hypothesis testing. The only significant and consistent association is with a measure of the China shock (Autor et al., 2020), again in the opposite direction to our instruments. To assuage the concerns that exposure to the China shock, and not our instruments, is driving the effect, all the successive robustness checks control for a contemporaneous measure of the China shock. Table B1 presents the results.

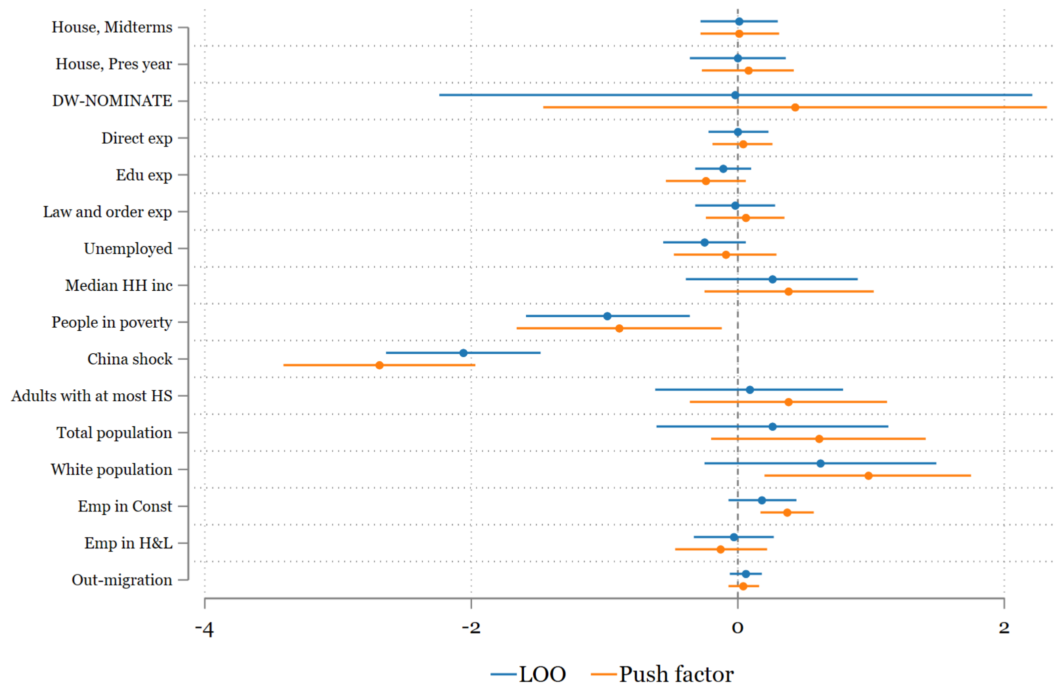

Figure 2: Depicted is the 95% confidence interval of regressing the pre-period changes of selected variables (standardized) on the change of the instruments between each of the three periods. Estimations control for state by period fixed effects and are weighted by population. Standard errors clustered at the CBSA level (congressional district for DW-NOMINATE). Sources: Dave Leip's U.S. Election Data, Lewis et al. (2021), Annual Survey of State and Local Government Finances, Quarterly Census of Employment and Wages (QCEW), US Census Bureau: Population Division, Acemoglu et al. (2016), Small Area Income and Poverty Estimates (SAIPE) Program, US Department of Commerce: Bureau of Economic Analysis, Peter K. Schott's Data; County Business Patterns.

Second, Table B2 explores whether the effects are being driven by differential trends, rather than by our instrument, by introducing control for the interaction of key pre-period characteristics with time by period indicators, notably for the China shock in 2006 and the number of people in poverty in 2000. To control for the evolution of stocks, we use the share of the Mexican population without US citizenship and the share of Hispanic population in 2000 (the last census that recorded citizenship status). Our results are robust to these controls.

Third, we explore whether migration exposure was non-random. This would occur if migrants from certain Mexican municipalities had simultaneously sorted into politically biased counties and had migrated at systematically different rates. Following the correction proposed by Borusyak and Hull (2023), we construct a counterfactual instrument by taking the average of 2,000 simulated instruments, created by multiplying the original shares by each of 2,000 permutations of the LOO shifters from other county-municipality dyads in the same period. The results remain significant.

Fourth, our results remain significant while controlling for a spatial lag (an average of neighboring counties), the share of Hispanics in the county, and a crude estimate of the stock of unauthorized migrants.[20] Results are also robust to excluding outliers and not weighting by using predicted population. Fifth, in Table B5, we show that our confidence intervals are largely unchanged when implementing a correction similar to the one proposed by Adão et al. (2019) to account for a potential correlation of the residuals between counties with comparable initial shares.

As discussed in Section 2.2, unauthorized migrants are empirically and theoretically different from "low-skilled" or authorized migrants. Since these groups overlap, though, another potential concern is that the effects we identify could be caused by these other groups, rather than by unauthorized migrants. To interrogate this possibility, we control directly for the measure used by Mayda et al. (2022a,b). Table B3 shows that whether we control by "low-skilled" Mexican migrants or all "low-skilled" migrants, our main results are qualitatively unchanged. The conservative reaction is driven by unauthorized migration, not "low-skilled" migrants. This may suggest that the effects that Mayda et al. (2022a) document are an imperfect proxy of the reaction to unauthorized migration. We examine likely-authorized Mexican migration in a similar exercise. The evidence suggests that the conservative reaction is not driven by likely-authorized migrants either. The conservative response seems to be to unauthorized migrants, not other migrant populations.

6 Mechanisms

There are several explanations consistent with the conservative electoral and policy responses documented above. Following the literature, we study three sets of outcomes to illuminate underlying mechanisms. In Subsection 6.1, we explore the effect of flows of unauthorized migrants on formal employment and wages. The goal is to identify whether migrants have

[20] This is the share of residents who were born in Mexico the year before our periods started, obtained from the Social Explorer repository of ACS 5. While it captures authorized and unauthorized migrants (and is noisier for smaller counties), it correlates strongly with the instrument ($\rho \approx 0.86$).

displaced existing workers and pushed down wages in specific industries that typically employ unauthorized migrants (i.e., partial equilibrium effects). Existing literature shows that Mexican migration generates either zero or small general equilibrium effects, since migrants contribute to economic activity beyond undercutting workers in partial equilibrium (Blau and Mackie, 2017; Clemens et al., 2018; Monras, 2020). Therefore, in Subsection 6.2, we study aggregated economic indicators (i.e., general equilibrium effects). We focus on median household income, unemployment, and poverty. In Subsection 6.3, we examine demographic changes, partisanship, and universalist values to estimate whether, as the electoral results suggest, migrants affect the demographic composition of the county, via residential sorting and internal migration, as well as the cultural/ideological preferences of residents. For demographic variables, we examine the effect on out-migration and adult population and voters, both total and White, separately. For values, we study partisan identity and the relative importance of universalist versus communal values. This variable, obtained from Enke (2020) replication files, aims to capture people's beliefs about the relative moral emphasis on the in-group as compared to the rest of the people. Those inclined toward universalism emphasize equal treatment, regardless of relationship, whereas those inclined toward communalism emphasize loyalty to members within their group. To the extent possible, the dependent variables are logged. However, in some cases, such as when we are studying indices, we use different functional forms. All the variables are summarized in Table A4.[21]

6.1 Employment and Wages by Sector

Labor market theories suggest that migrants can, even if marginally, decrease employment and wages among similarly skilled workers (Peri and Sparber, 2009; Borjas, 2013; Blau and Mackie, 2017). Politicians may in turn promise anti-migrant policy to attract those who lost out in the labor market. To test if flows of unauthorized migrants generate economic losses in formal employment or wages, which could explain the conservative shift, we use the Quarterly Census of Employment and Wages (QCEW). This source reports the annual average employment and weekly wages for multiple sectors and super-sectors across the US. We examine total average annual employment and wages and break out the super-sectors of hospitality and leisure (H&L), construction, and manufacturing.

The employment variables are measured in (log) per working-age person (ages 15–64) and the wages correspond to (log) 2010 dollars. The QCEW estimates are based on states' unem-

[21] There are two additional mechanisms we cannot explore due to data limitations. (1) The threat perception hypothesis, namely, that established residents are hostile to migrants due to a perceived threat (Hainmueller and Hopkins, 2014): We unpack relationships related to threat in a separate project on migration inflows and crime. (2) The role of political entrepreneurs: Since the flow of unauthorized migrants affects vote shares and attitudes simultaneously, we cannot examine how much the conservative reaction may be fueled by anti-immigrant discourse, like Djourelova (2023).

ployment insurance data and surveys from employers. They explicitly exclude "*self-employed workers, most agricultural workers on small farms...some domestic workers.*" Thus, the data mostly reflects formal employment.

Columns 1–5 of Table 4 present the results on formal employment. Inflows of unauthorized Mexican migrants have an imprecisely estimated zero effect on total formal employment. On average, employment levels for working-age people are not shifted by the arrival of migrants. This null effect, however, masks heterogeneity, as some sectors observe a decrease and others an increase, as unauthorized migrants begin informal employment. A 0.1 pp increase in the inflow of unauthorized migrants reduces formal employment in the construction industry by 0.6% (Panel B, Column 2, std coeff: -0.07) and by 0.2% in H&L (Panel B, Column 4, std coeff: -0.03). At the same time, migrant flows increase formal employment in manufacturing by 0.8% (Panel B, Column 3) leaving the main effect on total employment a precise zero (Panel B, Column 1). We estimate a drop in formal agricultural jobs. The agricultural estimates are unstable probably because most agricultural workers are excluded from the dataset.

These findings indicate a reallocation of jobs, away from construction and hospitality to manufacturing. Construction and hospitality are two of the industries with the highest estimated concentration of unauthorized migrants (Passel and Cohn, 2015; Svajlenka, 2020). Day labor in construction is often available to Mexican newcomers. Contingent work has low barriers to entry, and Mexican communities use informal organizations to facilitate day labor, which is disproportionately in construction (Valenzuela, 2003). In contrast, while it employs an estimated large share of unauthorized migrants, manufacturing is an industry that is likely to advantage those in formal employment. Our interpretation is that since their reservation wage is lower and their outside options are worse, unauthorized migrants are more willing to accept informal jobs (Kossoudji and Cobb-Clark, 2002; Ortega and Hsin, 2022), which are more common in the sectors of construction and hospitality.

Columns 5–8 of Table 4 present the results on wages (in logs). We do not observe evidence of wage changes in response to unauthorized migration. The impact on wages across all sectors is an imprecisely measured zero. By sector, the estimates are small and none are statistically significant. Economic theory would suggest that wages and employment should move in opposite directions. Key to our interpretation is the fact that the data captures primarily formal employment.

To further understand which demographic group might be losing or switching jobs, Table C8 replicates the analysis with data from ACS 5. The publicly available IPUMS version (Ruggles et al., 2022) covers only a subset of counties. To overcome this challenge, we recompute the flow of migrants and the instrument at the level of a Public Use Microdata

Area (PUMA), a geography covering several counties. Hence, we analyze the effect of inflows of migrants at the level of a period-locality (county if available in IPUMS, or PUMA if not available in IPUMS); the first stage is in Table A5. Unlike the QCEW, ACS 5 records people employed rather than employment levels and, in principle, includes both formal and informal jobs.[22] The main takeaway from this exercise is that the populations who are most likely to compete with unauthorized Mexican migrants in specific sectors are probably the ones negatively affected; we observe fewer White people with up to a high school education employed in hospitality. Importantly, we do not see consistent effects on construction. The job gains in manufacturing are concentrated among White individuals with more than a high school education.

Table C8 helps us to explore the argument of Dustmann et al. (2025) as well. The economic effects of migration could operate at the regional (in our case county or PUMA) level, or they could operate at the level of the individual worker. Table C8 illuminates that decomposition. The effect for "stayers"—persons who lived in a county or PUMA and remained there for the following year–[23] resembles the effects for employment presented in Table 4 and employed people in Table C7: fewer people employed in construction (imprecisely estimated) and H&L, and more employed in manufacturing. In contrast, there is evidence, although noisy, of job gains among regional newcomers, around 15% of the working-age population in the average location—which potentially includes international migrants. Job loss in "migrant-intensive" sectors seems to operate at the worker-level.

Our labor market analysis is consistent with the literature on the impact of "low-skilled" migration on labor market outcomes, which documents small short-term reductions in wages and employment, limited to a few "migrant-intensive" sectors or demographic groups most likely to compete with migrants (Hanson, 2009; Blau and Mackie, 2017; Clemens and Hunt, 2019; Monras, 2020). The employment decreases in construction and H&L are an order of magnitude smaller than the drop in manufacturing employment due to the China shock (Autor et al., 2013).[24] Although we document equally sizable job gains, the results suggest that there are some economic losers in counties that receive unauthorized migrant flows, and at first glance, these individuals are not compensated accordingly. Economic losses, especially for White voters in the H&L sector, may account for some voters favoring conservative politicians. However, job loss is only a part of the explanation for the political reaction.

[22]While losing precision due to smaller sample sizes and different dependent variables (employed people instead of formal employment overall), Table C7 shows that the effects are consistent: fewer people employed in construction and H&L and more in manufacturing.

[23]In an ideal setting, "stayers" would be classified as people who lived in that location for over 4 years, the duration of our periods. However, the publicly available IPUMS version does not have that data.

[24]Going from percentile 25 to percentile 75 of exposure to the China shock reduced employment in manufacturing by 4.5 percent between 2000 and 2007.

Table 4: Effect of arrival of unauthorized Mexican migrants on employment among working age population and weekly wages (2010-19)

	Employment, (log per working age person)				Weekly Wages (log 2010 USD)			
	(1) Total	(2) Constr	(3) Manufact	(4) Hosp & leis	(5) Total	(6) Constr	(7) Manufact	(8) Hosp & leis
			A. OLS					
Newcomers, pct pop.	0.01	-0.03*	0.08***	-0.01**	0.01	-0.00	0.00	0.01
	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
			B. 2SLS LOO					
Newcomers, pct pop.	-0.00	-0.05***	0.08***	-0.02**	0.00	-0.01	0.01	-0.00
	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Std. Coefficient	-0.00	-0.07	0.06	-0.03	0.01	-0.02	0.02	-0.00
$\hat{\beta} * P(75) - P(25)$	-0.000	-0.006	0.008	-0.002	0.000	-0.001	0.001	-0.000
			C. 2SLS Push Factors					
Newcomers, pct pop.	-0.00	-0.04*	0.09***	-0.03***	-0.00	-0.01	-0.01	0.01
	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Std. Coefficient	-0.00	-0.06	0.08	-0.04	-0.00	-0.02	-0.02	0.01
$\hat{\beta} * P(75) - P(25)$	-0.000	-0.004	0.010	-0.003	-0.000	-0.001	-0.001	0.001
Observations	8004	7491	7441	7936	8007	7494	7444	7939
Dep. Var., Mean	-0.67	-3.62	-3.07	-2.75	6.73	6.87	6.98	5.85
Dep. Var., Sd	0.34	0.45	0.73	0.43	0.26	0.23	0.28	0.31
Ind. Var., Mean	0.46	0.46	0.46	0.46	0.46	0.46	0.46	0.46
Ind. Var., Sd	0.59	0.59	0.59	0.59	0.59	0.59	0.59	0.59
Inst. Loo, Mean	0.40	0.41	0.41	0.40	0.40	0.41	0.41	0.40
Inst. Loo, Sd	0.55	0.55	0.55	0.55	0.55	0.55	0.55	0.55

Dependent variables in Columns 1–5 are the log of average annual employment divided by working age population. Dependent variables in Columns 6–10 are the log of annual average weekly wages in 2010 USD. Sources: Quarterly Census of Employment and Wages (QCEW) and US Census Bureau: Population Division. 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 county and state-period fixed effects, and are weighted by predicted population. Standard errors clustered at the CBSA level. Stars indicate *p<0.1,**p<0.05,***p<0.01.

6.2 General Economic Effects

We now turn to examining whether migration inflows affect general equilibrium indicators. We start by estimating the effects on poverty, a potential consequence of job loss, and a key driver of economic grievance (Hopkins et al., 2024). Column 1 of Table 5 displays the effects on the natural logarithm of people in poverty in the county, obtained from the Small Area Income and Poverty Estimates Program (SAIPE). As Panels B and C indicate, we identify a marginal increase in the number of people in poverty. A 0.1 pp rise in the inflow of unauthorized migrants, estimated with either instrument, raises the number of impoverished people by 0.7% (Panels B and C, Column 1, std coeff: 0.02).[25] Similar to the analysis of job loss, Table C9 explores changes in poverty by demographic groups using the ACS 5. The results are consistent with the argument of competing groups of workers. The groups with statistically significant impacts on poverty are Hispanic Americans and regional newcomers.

Column 2 of Table 5 displays the impact of inflows on unemployment. In contrast to employment, the unemployment (log of unemployed people) captures formal and informal work since it measures people from the labor force who are jobless and are actively looking for jobs. Consistent with the impact on average wages, inflows of migrants have a small, positive, and statistically insignificant effect on unemployment.

Column 3 Table 5 presents the impact on (log) county median household income. A 0.1 pp rise in the inflow of newcomers, estimated with the push factor instrument, decreases median household income by 0.3% (Panel C, Column 3, std coeff: 0.06). The effects are smaller, but less precisely estimated with the LOO instrument. Our interpretation is that these declines reflect two distinct forces. (1) Since we do not identify a statistically significant effect on average wages, unemployment, and formal employment, the decline in median household income partially reflects a reduction in formal economic activity, as opposed to a reduction in economic activity in general. (2) As the next section shows, relatively wealthier residents leave their counties of residence in response to the arrival of newcomers, bringing median household income down. That is, there is both a compositional and a direct effect. The impact on poverty illustrates this point. Table C6 estimates the effect on the county-level

[25] SAIPE's poverty estimates could include the new migrants themselves, as they are calculated with data from ACS, tax returns, the previous Census, and SNAP (Supplemental Nutrition Assistance Program) beneficiaries. To approximate the effect of migration inflows on poverty among US citizens, Table C6 presents the effect on the number of SNAP recipients. SNAP is among the most responsive federal entitlement programs, and it is unavailable to unauthorized migrants. Participation among non-citizens is minimal, and since the arrivals we are studying are from the previous four years, it is unlikely that they have US-born citizen children who qualify (USDA Food and Nutrition Service, SNAP: Guidance on Non-Citizen Eligibility). A disadvantage is that participation is voluntary, so it does not fully capture poverty among citizens. We observe an increase in recipients, which suggests that the rise in poverty does not solely reflect migrants themselves.

Table 5: Socioeconomic effects of arrival of unauthorized Mexican migrants (2010-19)

	County Economy (log)		
	(1) People in poverty	(2) Unemployed people	(3) Median household income
A. OLS			
Newcomers, pct. pop.	0.04***	-0.03**	-0.02
	(0.01)	(0.01)	(0.01)
B. 2SLS LOO			
Newcomers, pct. pop.	0.07***	-0.01	-0.02*
	(0.01)	(0.02)	(0.01)
Std. Coefficient	0.02	-0.00	-0.05
$\hat{\beta} * P(75) - P(25)$	0.007	-0.001	-0.002
C. 2SLS Push Factors			
Newcomers, pct. pop.	0.07***	0.00	-0.03**
	(0.01)	(0.02)	(0.01)
Std. Coefficient	0.02	0.00	-0.06
$\hat{\beta} * P(75) - P(25)$	0.007	0.000	-0.003
Observations	8019	8019	8019
Dep. Var., Mean	10.89	9.30	10.88
Dep. Var., Sd	1.63	1.68	0.26
Ind. Var., Mean	0.46	0.46	0.46
Ind. Var., Sd	0.59	0.59	0.59
Inst. Loo, Mean	0.40	0.40	0.40
Inst. Loo, Sd	0.55	0.55	0.55

Dependent variables are the log of people in poverty, unemployed people, and median household income (in 2010 USD). Sources: Small Area Income and Poverty Estimates (SAIPE) Program, and Local Area Unemployment Statistics (LAUS). 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 county and state-period fixed effects, and are weighted by predicted population. Standard errors clustered at the CBSA level. Stars indicate *p<0.1,**p<0.05,***p<0.01.

poverty rate —as opposed to the number of poor people. A 0.1 pp increase in the inflow of newcomers, estimated with either instrument, raises the poverty rate by 0.17 percentage points (Panels B and C, Column 3, std coeff: 0.18 and 0.17). As evidenced by the magnitude of the standardized coefficient, this effect is significantly larger than that of the number of poor people. Comparing the coefficients relative to the dependent variable mean yields a similar conclusion. The impact on people in poverty is 0.006 of the mean; the impact on the poverty rate is 0.11.

The findings on the general economic effects of the arrival of newcomers depict three complementary results: people at the bottom of the income distribution become marginally poorer, relatively wealthier residents migrate, and formal economic activity declines. These results are consistent with the conservative electoral and policy responses documented. However, it is not possible to determine to what extent they are the cause or the consequence of conservative policy. A reduction in total public expenditure at the local level could explain the rise in the number of poor people, but could also be explained by rising poverty.

6.3 Demographic Changes and Values

Other non-economic hypotheses for the conservative reaction are that the population changes in response to unauthorized migrant arrivals or that their policy preferences and values shift. Simply by virtue of their otherness, the arrival of newcomers could trigger exclusionary attitudes from established residents. These attitudes could be reflected either in new opinions or values or, more profoundly, in the decision to move out of the county. The resulting conservative response would be the consequence of the emigration of relatively left-leaning voters and/or a change in policy preferences.

Using US Census data and the voter registered file L2, we examine how population changes in response to unauthorized migration. We explore the total adult population, the White population, the number of White registered voters, and out-migration.[26] To examine values, we use the county-level index of the relative importance of universalist values versus communal values created by Enke (2020) from YourMorals.org. The index is available for the three periods, but only for a subset of the counties we study. We track partisanship with The Cooperative Election Study (CES), which surveys political preferences on a yearly basis and covers almost all counties, and with actual party identification for a subset of states which requires registered voters to be affiliated with a political party.

Columns 1 and 2 of Table 6 display the effects on the adult population. We observe a decline in the total adult population, driven by White population. A 0.1 pp rise in the flow

[26]Since the census systematizes data from the ACS 5 on county-to-county demographic flows, we construct out-migration from the differences between 2007 and 2011, 2011 and 2015, and 2015 and 2019.

Table 6: Effect of arrival of unauthorized Mexican migrants on demographic composition (2010-19)

	Adult Population (log)			Share	
	(1) Total population	(2) White population	(3) White voters	(4) White voters	(5) Out-migration
	A. OLS				
Newcomers, pct. pop.	-0.03***	-0.02***	0.06**	0.04***	1.26**
	(0.01)	(0.01)	(0.03)	(0.01)	(0.58)
	B. 2SLS LOO				
Newcomers, pct. pop.	-0.03***	-0.03***	-0.06	0.08***	1.59***
	(0.01)	(0.01)	(0.07)	(0.02)	(0.61)
Std. Coefficient	-0.01	-0.01	-0.02	0.28	0.06
$\hat{\beta} * P(75) - P(25)$	-0.004	-0.003	-0.006	0.008	0.170
	C. 2SLS Push Factors				
Newcomers, pct. pop.	-0.03***	-0.02**	-0.12	0.11***	1.34**
	(0.01)	(0.01)	(0.08)	(0.02)	(0.60)
Std. Coefficient	-0.01	-0.01	-0.03	0.40	0.05
$\hat{\beta} * P(75) - P(25)$	-0.003	-0.002	-0.013	0.012	0.144
Observations	8019	8019	5346	5346	8017
Dep. Var., Mean	12.62	12.36	11.24	0.36	55.16
Dep. Var., Sd	1.59	1.52	1.43	0.12	17.00
Ind. Var., Mean	0.46	0.46	0.35	0.35	0.46
Ind. Var., Sd	0.59	0.59	0.42	0.42	0.59
Inst. Loo, Mean	0.40	0.40	0.31	0.31	0.40
Inst. Loo, Sd	0.55	0.55	0.39	0.39	0.55

Dependent variable in Columns 1–3 are the log of total adult population, adult White population, and White voters who voted in House midterm elections. Dependent variable in Column 4 is the ratio of White voters who voted in House midterm elections to White adult population. Dependent variable in Column 5 is out-migration, calculated as the number of out-migrants (in thousands) divided by county population. Sources: US Census Bureau: Population Division and Small Area, US Census Bureau: 2007-2011, 2011-2015, and 2015-2019 American Community Surveys 5, and L2 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. Source: SRE. Instrument is as described in Section 3. All regressions have county and state-period fixed effects, and are weighted by predicted population. Standard errors clustered at the CBSA level. Stars indicate *p<0.1,**p<0.05,***p<0.01.

of newcomers causes a 0.4% decrease in the adult county population (Panel B, Column 1, std coeff: -0.01). This decline is mostly explained by the adult White population, which is reduced by 0.3% (Panel B, Column 3, std coeff: -0.01). In Columns 3 and 4, we look at White registered voters. While their absolute numbers decrease (imprecisely estimated in Column 3), their share as a proportion of the adult population increases.[27] In a secondary analysis, we analyze the effects on turnout by race and ethnicity (See appendix Table C11). In response to unauthorized migrant inflows, White turnout, as a share of the electorate, increases, while Black and Hispanic turnout decrease, both in percentage terms and as a share of the electorate. Column 5 shows that some of the population decline is attributable to out-migration. A 0.1 pp rise in the flow of newcomers causes an increase of 0.17 out-migrants per 1,000 inhabitants (Panel B, Column 2, std coeff: 0.06). All together, these results reinforce the idea that a change in the composition of the electorate explains the increase in vote share for Republican candidates. Consistent with White flight, we observe decline of the White population, explained by out-migration. However Whites become a larger proportion of registered voters. White turnout increases and Black and Hispanic turnout declines.

Table 7 explores the effects on partisan affiliation and orientation toward the out-group. There is little evidence that people are more likely to identify as Republican in response to new unauthorized arrivals. There is a null effect of new unauthorized arrivals on Republican registration rate with the LOO instrument (Panel B, Column 1), and a small and noisy estimate of an effect with the push factor instrument (Panel C, Column 1). The results are consistent with evidence from survey data: there is no measurable effect of unauthorized migrant arrivals on the share of adults who identify themselves as Republican (Column 2). Taken together, the findings suggest that people do not register or identify with the Republican party to any greater extent because of unauthorized migration. In a secondary analysis on the Democratic party there are sharper results (see appendix Table C2). In midterm House elections, registration with the Democratic party and the percentage of votes for the Democratic candidate both decline. This final result suggests that some of the conservative reaction to unauthorized migration reflects departures from the Democratic party more than it does an embracing of a Republican identity.

Residents in counties with more unauthorized migration also display a change in values (Column 3). We study individuals' universalist values to capture openness to and regard for the out-group. More universalist values indicate that a person is concerned equally with the welfare of all individuals. By contrast, people with more communal values assign a greater

[27] In Table C10, we perform the same exercise with the Hispanic population and voters. The results indicate a marginal increase in Hispanic population but lower turnout, both in absolute and relative terms.

weight to the welfare of in-group members relative to out-group members. Counties become less universalist in response to the arrival of newcomers. A 0.1 pp increase in the flow of newcomers shifts counties 0.014 standardized units toward less universalist (Panel B, Column 5, std coeff: -0.16). This result is the most direct indication that some of the shift to the right occurs because migration inflows trigger out-group bias. 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 erodes trust, makes coordination more difficult, and reduces people's interest in universal redistribution (Alesina et al., 1999).

Table 7: Effect of arrival of unauthorized Mexican migrants on values (2010-19)

	Log	Share	Index
	(1) Registered Republicans	(2) Republican ID	(3) Universalist values
A. OLS			
Newcomers, pct. pop.	0.01	0.00	-0.09**
	(0.02)	(0.02)	(0.04)
B. 2SLS LOO			
Newcomers, pct. pop.	0.01	0.01	-0.13***
	(0.02)	(0.02)	(0.04)
Std. Coefficient	0.01	0.03	-0.16
$\hat{\beta} * P(75) - P(25)$	0.001	0.001	-0.014
C. 2SLS Push Factors			
Newcomers, pct. pop.	0.03*	-0.01	-0.16***
	(0.02)	(0.02)	(0.04)
Std. Coefficient	0.01	-0.03	-0.19
$\hat{\beta} * P(75) - P(25)$	0.004	-0.001	-0.017
Observations	3367	5301	5712
Dep. Var., Mean	11.34	0.35	0.15
Dep. Var., Sd	1.36	0.21	0.50
Ind. Var., Mean	0.47	0.47	0.47
Ind. Var., Sd	0.58	0.59	0.60
Inst. Loo, Mean	0.43	0.41	0.41
Inst. Loo, Sd	0.57	0.56	0.56

Dependent variable in Column 1 is the log of registered Republican voters. Dependent variable in Column 2 is share of county residents who describe themselves as Republican. Dependent variable in Column 3 is the index of universalist values, taken from by Enke (2020). Sources: Enke, (2020), Schaffner et al. (2023), and Dave Leip's U.S, 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. Source: SRE. Instrument is as described in Section 3. All regressions have county and state-period fixed effects, and are weighted by predicted population. Standard errors clustered at the CBSA level. Stars indicate *p<0.1,**p<0.05,***p<0.01.

6.4 Relative importance of the mechanisms

The standardized coefficients and the voting behavior of the demographic groups likely affected in the labor market are useful to assess the relative importance of the different mechanisms. To approximate the contributions, we do back-of-the-envelope calculations. On average, during the midterm elections we analyze, around 42% of voting-age citizens voted. Assuming that each lost job belonged to one person, that *everyone* who lost jobs voted Republican, and that people ages 15 to 64 are roughly 75% of the electorate, job loss would explain only about one-fourth of the observed effect. Assuming that *everyone* who fell into poverty voted Republican, poverty would only explain about one-third of the effect. If *all* the adults who left the county were Republican, we would underestimate the conservative shift of the old electorate by one-seventh. If all the adults who left the county were Democrats, we would be overestimating the conservative shift of the old electorate in that same proportion. None of these effects alone, even under weak assumptions, could account for the conservative reaction.[28]

6.5 Robustness for mechanisms

Table B6 displays robustness checks on the mechanisms. The results are largely robust to controlling for differential trends, a simulated instrument, a lagged instrument, proxies of the stock of Mexican-born and Hispanic people, and other factors.

7 Taxation and the Social Safety Net

We have demonstrated that the inflow of unauthorized Mexican migrants increases the vote share for Republican candidates in federal House elections, prompts more conservative legislative behavior, reduces total public expenditure, and reallocates expenditure away from education toward law-and-order budgets. The effects seem partially driven by a drop in formal employment in exposed sectors and an increase in poverty, residential sorting, and a rise in out-group bias.

This section explores the extent of impact variation across policy environments. The link between fiscal policy, via redistribution, and political behavior has been documented in other settings. Fetzer (2019) finds that experiencing welfare cuts is associated with support for Brexit and the far right in the UK. Similarly, our hypothesis is that counties with more progressive tax structures or a larger safety net are better able to mitigate economic shocks

[28] Job loss effects ($1/4 \approx 0.072 \times 0.75 \times 0.42/0.085$) are from 5.2% in construction and 2% in H&L as observed in Columns 2 and 4 of Panel B, Table 4. Poverty effects ($1/3 \approx 0.07 \times 0.42/0.085$) are 7% per Column 1 of Table 5. The effects are similar if we restrict the analysis to poverty among those 18 and older. The effect on total population ($1/7 \approx 0.03 \times 0.42/0.085$) is 3% per Column 1 of Table 6.

and compensate those who lose economically; hence, the impact of migration inflows should be lower. We find suggestive evidence that this is the case.

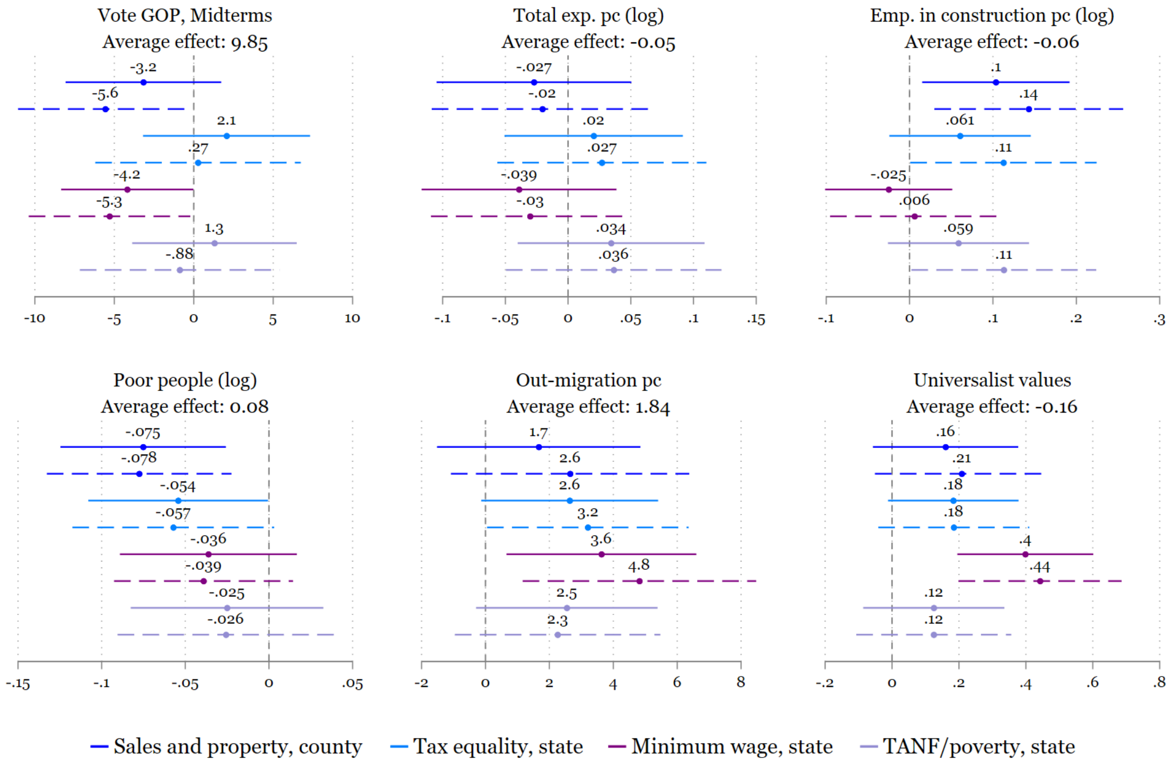

Figure 3: Heterogeneous effects by tax progressivity/strength of the safety net.

Displayed are the 95% coefficient intervals of the interaction between the instrument and dummy indicating above median values (below median for the sales and property tax revenue). Estimations are reduced form. Solid lines correspond to the LOO instrument and dashed lines to the push factors instrument. Sources: Annual Survey of State and Local Government Finances, Dave Leip's U.S. Election Data, QCEW, Enke, (2020), US Census Bureau: Population Division and Small Area, US Census Bureau: 2011, 2015, and 2019 ACS 5, SAIPE Program, US Department of Commerce: Bureau of Economic Analysis, Davis et al. (2009), US Department of Labor, Wage and Hour Division, and Shrivastava and Thompson (2021).

We categorize counties based on four (pre-period) proxies of the system's ability to compensate people who lose economically: (1) the share of their own revenue generated from sales and property tax—important sources of revenues for local governments in the US, but largely regressive (Davis et al., 2009; Berry, 2021), (2) an index of tax equality (Davis et al., 2009), (3) the share of the low-income population covered by the federal program Temporary Assistance to Needy Families (TANF) (Shrivastava and Thompson, 2021), [29] and (4) the minimum wage, which, although not an explicit measure of redistribution, is associated

[29] As with Medicaid, states set the operational rules, i.e. generosity of the program.

with higher incomes at the bottom of the distribution and lower poverty rates (Cengiz et al., 2019; Dustmann et al., 2021). Proxies 2 through 4 vary at the state-level.

Figure 3 displays the differential effects of unauthorized Mexican migration on the main political and socioeconomic variables for above median values of progressive taxation and strength of the social safety net. Counties above the median (with more progressive taxation and robust safety nets) have more muted shifts to the right and modest socioeconomic impacts. Although the differences are not always statistically significant, the direction is consistent and the magnitude is meaningful.

Counties with more progressive taxation/stronger safety net appear to compensate those who lose economically, lessening the negative labor market and welfare impacts of unauthorized migration. The increase for Republicans in midterm elections is reduced by between half and a third in these counties, roughly equivalent to the share of electoral effect attributable to economic factors in Section 6.4. The effects on formal job losses in construction, increases in poverty, and reduction in universalism are partially or fully reversed. In contrast, these counties do not see smaller effects on out-migration. In fact, the impact is exacerbated; out-migration increases. This exception supports the hypothesis that residential sorting does not respond to economic factors, but rather to other motivations like compositional amenities. Section D shows the results in regression format. Otherwise, the political and socioeconomic impacts are not systematically heterogeneous.[30]

8 Conclusion

We began this paper by asking how much the national conservative reaction to unauthorized migration reflects the local political response to the actual unauthorized migrant flows. Our evidence shows that people respond to new unauthorized migrants in their local area. We estimate the impact of recent unauthorized Mexican migration on local political and socioeconomic outcomes using two different shift-share strategies. In response to the arrival of newcomers, county vote share for the Republican candidates increases in House elections. Local government agencies reduce total expenditure, divest in education, and increase relative spending in support of law and order. We contend that three socioeconomic channels partially explain this conservative reaction. (1) This migration creates small, but differential, labor market disruptions and leads to an increase in the county poverty rate. However, there is no evidence that the people who are directly economically affected are driving the

[30] To explore whether the demographic composition of the counties matters, we examine whether there is a differential impact in counties with higher shares of Hispanic, White, or Black populations. To explore whether long-term economic transformation exacerbates the effects, we test if counties with an above-median reduction in employment in manufacturing between 1990 and 2005 observe a differential effect. Figure D2 plots the coefficients of the interaction terms. We do not detect any systematic difference.

conservative response. (2) The new arrivals drive population loss, especially among White residents, explained by out-migration. This residential sorting seems to be unrelated to economic factors and suggests that changes in the composition of the electorate are important for understanding political effects. (3) Established residents display more out-group bias.

The battery of robustness checks supports a causal interpretation of the evidence. Both the main political effects and the mechanisms are robust to conditioning on differential pre-trends, a counterfactual instrument, a proxy of the stock of migrants, and spatial lags, as well as not weighting by predicted population, and removing outliers. We do not find a statistically significant association between the instruments and none of the lagged outcomes, which supports the parallel trends assumption. Controlling for different types of recent "low-skill" migrants and likely-authorized Mexican migrants does not change our results, suggesting, as the media environment indicates, that the conservative reaction is driven by unauthorized migration, not by other populations.

These results contribute to a growing literature on the backlash against migrants from developing countries. While responses to different groups of migrants in the US have been studied, this study is a novel effort to estimate the impacts of unauthorized migrants—the group that has been the target of ire among Republican leaders. This effort is first to study the political impacts of unauthorized migration throughout a whole country. Studying this group directly is important because unauthorized migrants have distinctive characteristics. For example, they cannot vote, but they are largely long-term residents; they are fully employed, but largely in informal sectors (Svajlenka, 2020).

Unlike most existing studies that focus either on the political and electoral effects or on the fiscal effects of immigration, we examine and draw links among voting behavior, legislative choices, and fiscal policy. The conservative reaction is consistent with the impacts of refugees and poorly educated migrants, especially from developing countries, in Europe and the US. In contrast to Rozo and Vargas (2021), we do not observe that the reaction is explained by the radicalization of some citizens. Rather, we identify patterns consistent with support for the Republican party among those not registering or identifying with the party.

We disentangle the roles and interactions of economic, social, and political factors in explaining the right-wing reaction to migration (Alesina and Tabellini, 2024). The social and economic forces that explain the political reaction to migrants are similar to those that shape internal migration and race in the US (Boustan, 2010; Shertzer and Walsh, 2019; Derenoncourt, 2022). Our evidence suggests that unauthorized migrant flows create economic grievance, despite prompting only limited formal job loss and a modest increase in the number of people below the poverty line. This grievance could explain a decrease in universalism values, but it does not seem to explain residential sorting. The effects we find on universal values are a novel finding in the literature on the political economy of migration, and an explicit response to Alesina and Tabellini (2024)'s suggestion to explore the role of migration on moral values.

A county's taxation and redistributive policy are sources of heterogeneity. The political and socioeconomic impacts of unauthorized migrants seem to be more concentrated in counties that have the least capacity or willingness for redistribution, as they are less able to compensate those who lose economically with the arrival of unauthorized migrants. Ironically, in these places, the arrival of unauthorized migrants prompts declines in universalist values away from redistribution and the kinds of policies that elsewhere mute the negative impacts of unauthorized migrants. A county's taxation and redistribution enhance, rather than hinder, out-migration, suggesting that residential sorting does not respond to the same policy levers.

From the standardized coefficients and back-of-the-envelope calculations, we observe that the effects of migration on the political outcomes are larger than the effects from the proposed mechanisms. The conservative reaction to unauthorized Mexican migrants cannot be fully explained by the channels we have explored. Future research should document the role of additional channels, like rhetoric, (mis)perceptions, and the motivations of those who select into running for office.

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Online Appendix

A Supplementary information

A.1 Evolution of flows and stocks of unauthorized Mexican migrants

Between 2002 and 2020, 7.4 million people obtained a consular ID. To compare, according to the 2000 US Census, the last one with a citizenship question, there were 4.8 million unauthorized Mexican migrants in the United States. (Office of Policy and Planning, 2003). According to the US Department of Homeland Security, there were 5.4 million unauthorized Mexican migrants in 2018 (Baker, 2021). The Migration Policy Institute calculates that the number was 5.3 million in 2019 (Ward and Batalova, 2023).

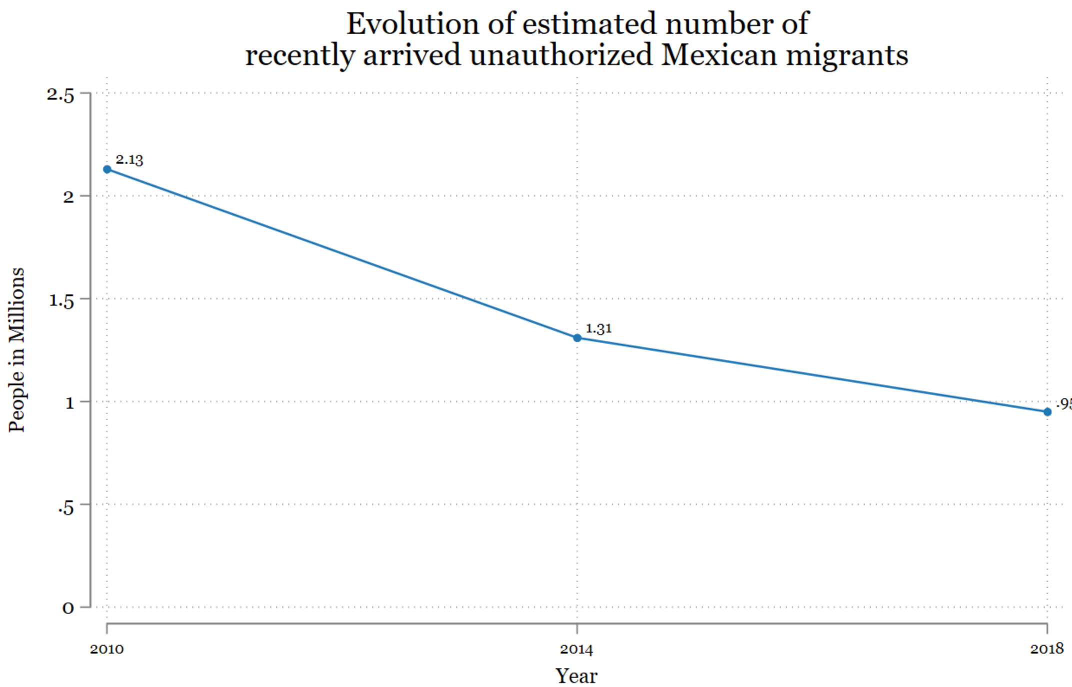

Figure A1: The figure plots the evolution of inflows of recent unauthorized migrants. Passel and Cohn (2018) estimates the stock of all unauthorized migrants to have declined from 11.4 million in 2010 to 10.7 million in 2016, and the subset who are Mexican from 6.2 million in 2010 to 5.4 million in 2016.

A.2 Comparison of likely unauthorized Mexican migrants' characteristics

There are 441 counties in ACS 5 with detailed demographic characteristics for likely unauthorized Mexican migrants for the period we study. We compare the distribution of these characteristics in those counties with the distributions from the consular data. The only substantive difference relates to age. This is not surprising. Children rarely apply for consular IDs. In our sample, less than 2% of cardholders are under 18.

Table A1: Summary statistics of selected variables for newcomer Mexican unauthorized migrants using ACS 5 and Consular data

	(1) ACS 5 likely unauthorized	(2) ACS 5 low-skill	(3) Consular data same counties	(4) Consular data full sample
Female	0.41	0.42	0.41	0.41
	(0.49)	(0.49)	(0.49)	(0.49)
Never married/single	0.49	0.43	0.46	0.46
	(0.50)	(0.49)	(0.50)	(0.50)
Age	30.04	34.20	32.48	32.38
	(10.64)	(14.76)	(11.88)	(11.76)
Observations	45818	28882	3681865	4380791
Number of Counties	441	414	472	2683

Sources: SRE (2022) and Ruggles et al. (2023). The sample in Column 1 is composed of people born in Mexico, who arrived in the US less than five years before with at most high school education, without US citizenship, between 16 and 64 years old. The sample in Column 2 is composed of people born in Mexico who arrived in the US less than five years before, 18 years or older, with no completed high school education. The Consular sample is comprised of unique new observations per period per CBSA.

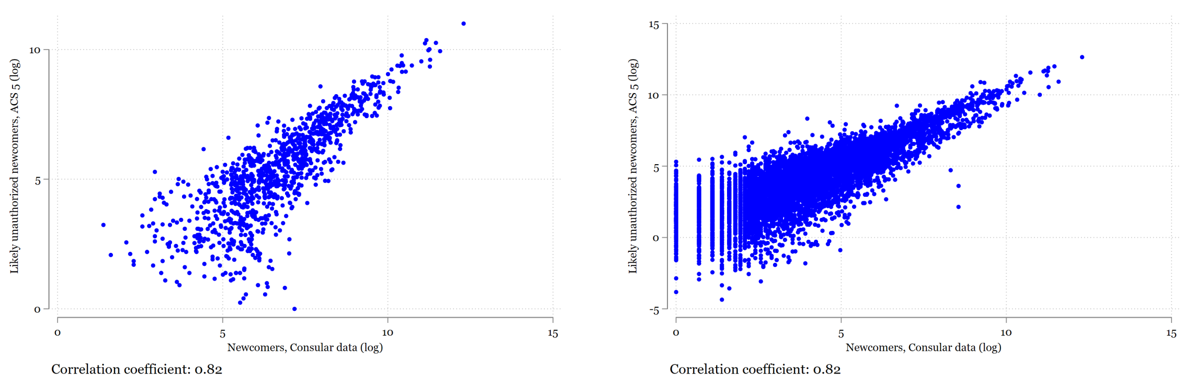

Figure A2: The left panel uses data for the 2,674 counties covered by ACS 5, Social Explorer, and our data. The right panel uses data for the 441 counties covered by ACS 5, IPUMS, where there is at least one likely unauthorized Mexican migrant, and our data. The association is weaker in areas with few migrants, probably due to low precision from the Social Explorer data. The unit of observation is a county-period (2010, 2014, and 2018).

A.3 Authorized vs. unauthorized migrants

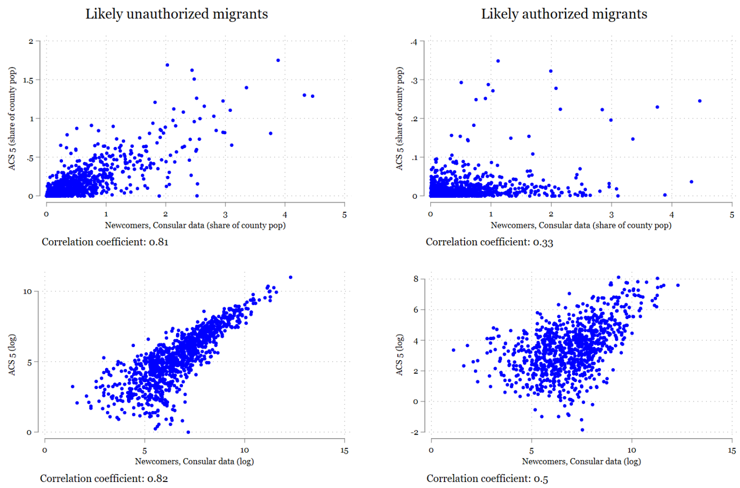

Figure A3: Estimates of Mexican migrants, ACS 5 vs consular data (2010, 2014, and 2018)

Table A2: Correlation between the LOO instrument and ACS 5 estimates of unauthorized and authorized recent Mexican migration

	(1) Unauthorized	(2) Authorized
LOO instrument	0.503***	0.001
	(0.036)	(0.005)
Observations	1253	1253
F statistic	198	0
Mean of Dep. Var	0.175	0.018
Mean of Ind. Var	0.404	0.404
Push factors instrument	0.548***	-0.001
	(0.044)	(0.005)
Observations	1253	1253
F statistic	157	0
Mean of Dep. Var	0.175	0.018
Mean of Ind. Var	0.440	0.440

ACS 5 samples include people born in Mexico, ages 16–64, who arrived in the US less than five years before. The likely-unauthorized migrants have at most high school education and do not have US citizenship; the likely-authorized are either naturalized or non-citizens, but have at least one year of college education. The consular sample includes unique new observations per period per CBSA. Estimations control for county and state-year fixed effects, and are weighted by predicted county population. Variables are share of county population. Standard errors clustered at the county level. Sources: SRE, Ruggles et al. (2023).

A.4 Exploring changes in demand of Consular IDs

Secure Communities (SC) 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 individuals eligible for deportation. In turn, ICE would request that an individual be held on a detainer to start a deportation process. It is the largest immigration program during our period of study, and it was implemented locally. Secure Communities could have discouraged migrants from applying for a consular ID. With the ID, it would be obvious to local authorities that the cardholder is a foreign national, maybe prompting authorities to submit fingerprints. We carry out six event study designs to explore whether consular IDs are responsive to the rollout.[31] The rollout was progressive, but not entirely random. The event studies differ by the period of analysis and the use of controls identified in previous studies (Alsan and Yang, 2024; East et al., 2022). Figure A4 displays the evolution of take-up rates across specifications. While sensitive, the results are consistently not statistically significant.

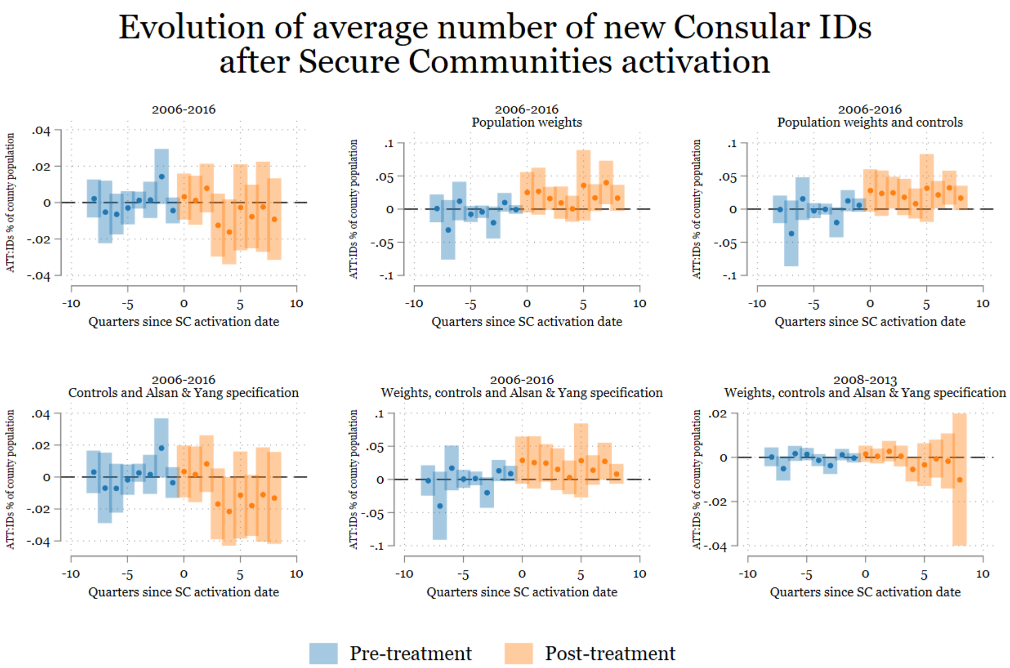

Figure A4: SC was implemented from October 2008 to September 2013, so analysis ranges from the first quarter in 2006 to the fourth quarter in 2016. Always-control counties (98 out of 2678) in the first estimation (no controls) are those that adopted the program last. The second estimation builds by using population weights. The third controls for distance to the Mexican border and share of Hispanic population. The fourth follows Alsan and Yang (2024) and also excludes border counties and the states of Massachusetts, New York, and Illinois. The fifth uses population weights on the fourth estimation. The sixth uses weights and controls, like the fifth, but restricts the periods of analysis to 2008–2013. All estimations restrict the results to eight quarters after the activation of the program.

[31] All the event study estimations in this appendix follow Callaway, B. and Sant'Anna, P. H. (2021). Difference-in-Differences with multiple time periods. *Journal of Econometrics*, 225(2):200–230.

As of 2018, 12 states and DC allowed unauthorized migrants to get a diver's license, as compared with only 3 before 2012. [32] We implement an event study to test if, between 2013 and 2016, states that modified their regulations observed an uptick in consular cards issued. As Figure A5 shows, a jump lasting three quarters, a time frame much shorter than our periods is observed.

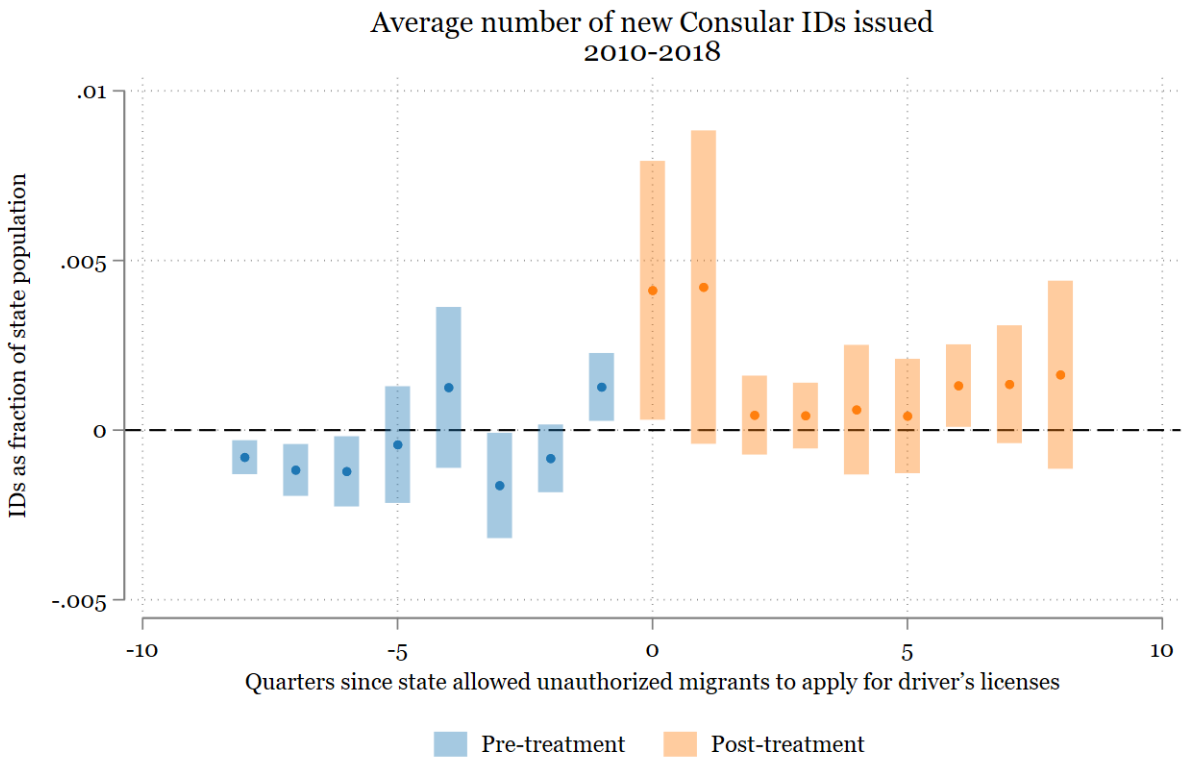

Figure A5: Change in demand of consular IDs after driver's license regulations

[32]NCSL Immigrant Policy Project (2021). States Offering Driver's Licenses to Immigrants.

A.5 Summary statistics, main variables

Table A3: Summary statistics for the main variables

	Mean	Std	Min	Max	Obs	Counties	Data relative to end of periods
Newcomers, population fraction	.462	.591	0	9.406	8022	2674	0
Instrument, leave CBSA out	.404	.551	0	3.823	8022	2674	0
Instrument, leave state out	.318	.411	0	2.79	8022	2674	0
Instrument, push factors	.44	.595	0	3.651	8022	2674	0
Population, 000s	116.5	348.1	.4	10061.5	8022	2674	0
Vote share GOP House, midterm	48.2	19.4	0	100	7995	2673	0
Vote share GOP House, Pres	47.2	19.9	0	100	8015	2673	2
DW-NOMINATE score	.06	.46	-.78	.91	607	261	0
Total revenue, pc (log)	8.48	.39	6.89	11.09	5340	2670	2.5
Total (dir exp), pc (log)	8.44	.39	6.84	11.08	5340	2670	2.5
Edu (dir exp), pc 0-19 (log)	8.84	.65	-3.24	11.6	5337	2669	2.5
Law and order (dir exp), pc (log)	5.71	.47	1.91	8.45	5340	2670	2.5

Columns 1–6 are mean, standard deviation, minimum, maximum, number of country-period observations, and 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 that it is for 2011, 2015, and 2019; 2.5 indicates it is a 2.5 year average after the end of our periods. All estimates (except population) are weighted by county population. Sources: SRE, Dave Leip's U.S. Election Data; Lewis et al. (2021); Annual Survey of State and Local Government Finances, and US Census Bureau: Population Division.

A.6 Summary statistics, mechanisms

Table A4: Summary statistics for the mechanisms

	Mean	Std	Min	Max	Obs	Counties	Data relative to end of periods
Total emp, pc 15-64 (log)	-.67	.34	-3.97	1.71	8008	2671	1
Construction emp, pc (log)	-3.62	.45	-7.07	.42	7560	2596	1
Manufacturing emp, pc (log)	-3.08	.74	-8.03	-.33	7496	2556	1
Leisure emp, pc (log)	-2.75	.43	-7.26	.23	7947	2661	1
Weekly avg wages, log 2010 USD	6.74	.27	5.75	7.78	8011	2672	1
Weekly wages, construction (log)	6.87	.23	5.43	7.77	7563	2597	1
Weekly wages, manufacturing (log)	6.98	.28	4.91	8.23	7499	2557	1
Weekly wages, leisure (log)	5.85	.31	4.39	6.95	7950	2662	1
People in poverty (log)	10.89	1.63	4.04	14.4	8022	2674	1
Unemployed people (log)	9.3	1.68	2.08	13.31	8022	2674	1
Median HH income (log)	10.88	.26	9.97	11.79	8022	2674	1
Adult population (log)	12.63	1.59	5.66	15.85	8022	2674	1
Adult White pop (log)	12.36	1.52	5.59	15.49	8022	2674	1
White voters midterm, log	11.24	1.43	1.1	13.92	5348	2674	0
White voters midterm, share	.36	.12	0	1.26	5348	2674	0
Out-migration per 1000 people	55.24	17.06	8.63	300.25	8020	2674	1
Registered Republicans (log)	11.3	1.4	4.7	13.9	3436	1193	0
Republican ID	.35	.22	0	1	5700	2297	1
Relative importance univ values	.152	.497	-3.803	3.482	5802	2096	.5

Columns 1–6 are mean, standard deviation, minimum, maximum, number of country-period observations, and number of unique counties. Column 7 reflects the year for which we have data relative to our periods: 1 indicates that it is for 2011, 2015, and 2019; 1.5 (0.5) indicates it is a 1.5 (0.5) year average after the end of our periods. Estimates weighted by county population. Employment and wages come from the QCEW. Poverty and median household income come from the SAIPE Program. Unemployment comes from the LAUS Program. Population variables come from the U.S Census Bureau, Population Division. White/European voters come from L2 data. Registered Republicans come from Dave Leip's U.S. data. Republican ID comes from Schaffner et al. (2023). Out-migration comes from the US Census based on ACS 5. The index of moral universalism comes from Enke et al. (2020). The share of White voters is in one county-year greater than one likely due to measurement error.

Sources

Enke, B. (2020). Moral Values and Voting. *Journal of Political Economy*, 128(10):3679–3729.

L2 Voter Data (2024).

Schaffner, B; Ansolabehere, S; Shih, M. (2023), "Cooperative Election Study Common Content, 2022", https://doi.org/10.7910/DVN/PR4L8P, Harvard Dataverse, V4

US Bureau of Labor Statistics (2022a). Local Area Unemployment Statistics (LAUS). County Estimates.

US Bureau of Labor Statistics (2022b). Quarterly Census of Employment and Wages (QCEW). Annual Averages By County.

U.S. Census Bureau (2022). Small Area Income and Poverty Estimates (SAIPE) Program.

U.S. Census Bureau; American Community Survey (2022). 5-Year Estimates (2011, 2015 and 2019).

U.S. Census Bureau, Population Division (2022). County Characteristics Population Estimates.

A.7 Data for push factors

Our second identification strategy predicts emigration from each municipality in each period. We regress the number of migrants on a set of time-varying variables. We use the fitted values as the shifters. The time-varying variables come from four datasets.

(1) The University of Delaware's temperature and precipitation data We calculated the mean yearly temperature and precipitation of each data point within Mexico from 1950 to 2017. We then calculated the mean and the standard deviation for every period (2007–10, 2011–14 and 2015–17). For municipalities with more than one data point, we used the average of all the points within it. For municipalities with no data, we calculate the values of neighboring stations. The variables are mean and deviation of precipitation and temperature, as well as deviation from historic values.

(2) The National Institute of Statistics and Geography's (INEGI) yearly deaths data 2005-2020 For every municipality, we calculated the number of general deaths, neonatal deaths, infant deaths, maternal deaths, and homicides, both in levels and in shares of municipality population. Our dataset has mean municipal values for 2,493 municipalities––Mexico City's information comes at the delegation level, but we average the values—for each period (2007-10, 2011-14 and 2015-18).

(3) INEGI's Economic Census for 2009, 2014 and 2019 Every five years, INEGI gathers data about the economic activity of each municipality corresponding to the previous year. Among others, the dataset has information on total investment, total production, number of employed people, wages, and stocks for different subsectors. We constructed municipal totals for every year and obtained the per capita indicators.

(4) The National Council of Social Policy Evaluation's (CONEVAL) poverty and underdevelopment estimates We use two datasets both covering the years 2010, 2015, and 2020. 1) We use the dataset of poverty indicators. For every municipality, we use data on the rates of poverty and extreme poverty, as well as indicators of underdevelopment in education, health, and housing.

2) We use the dataset of underdevelopment estimates. Among other things, it provides information about the share of adults who don't know how to read and write, and children who don't go to school, as well as households without basic health-protective measures, concrete floors, toilets, electricity, and washing machines.

We combined these push factors, created square terms to model non-linear associations, and merged them with the data on the number of migrants from each municipality in each period. Since the variable of interest is censored at zero, we predict observed migration using a Poisson regression. To avoid over-fitting, we implemented a Lasso correction. Out of the 54 variables included in the regression, 26 were selected.

Sources

Consejo Nacional de la Evaluación de la Política de Desarrollo Social (CONEVAL) (2023a). Pobreza a Nivel Municipio 2010–2020.

Consejo Nacional de la Evaluación de la Política de Desarrollo Social (CONEVAL) (2023b). Índice de Rezago Social 2020. Nivel nacional, estatal, municipal y localidad.
Instituto Nacional de Estadística y Geografía (2022). Censos Económicos (2009, 2014 and 2019).
Instituto Nacional de Estadística y Geografía (2023). Defunciones por homicidios.
NOAA PSL and University of Delaware (2022). Terrestrial Air Temperature and Precipitation: 1900-2014 Gridded Monthly Time Series.

A.8 First-stage in different levels

Table A5: First stage for congressional districts and localities (Counties and PUMA)

	(1) LOO	(2) Push Factors
Congressional district		
Instrument	1.340***	1.375***
	(0.108)	(0.117)
Observations	524	524
Localities (Counties and PUMA)		
Instrument	1.174***	1.344***
	(0.062)	(0.079)
Observations	2564	2564

Sources: Lewis et al. (2021), Ferrara et al. (2024), and Ruggles et al. (2023). 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 locality and state-period fixed effects, and are weighted by predicted population. Standard errors clustered at the locality level. Stars indicate *p<0.1,**p<0.05,***p<0.01.

A.9 Map of variation employed

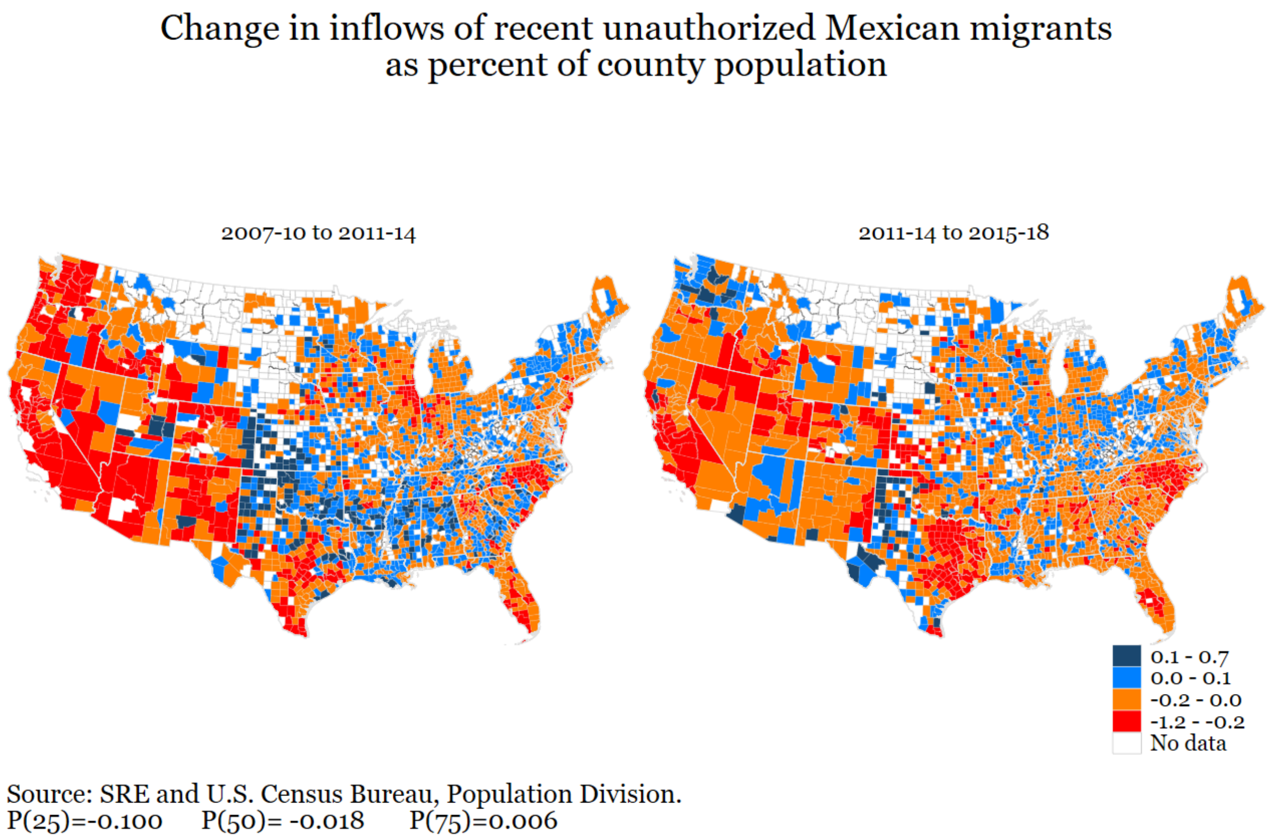

Figure A6: Identifying variation: within state-period county changes

B Robustness checks

B.1 Main robustness checks

Table B1: Correlation of Instruments with Lagged Outcomes

	Leave-one-out	Push factor
GOP Share Midterms	0.17	0.23
	(2.57)	(2.62)
GOP Share Presidential Year	0.00	1.11
	(2.65)	(2.54)
DW-NOMINATE	-0.002	0.056
	(0.147)	(0.125)
Direct expenditures (log pc)	0.00	0.01
	(0.02)	(0.02)
Education expenditures (log pc)	-0.02	-0.04
	(0.02)	(0.03)
Law and order (log pc)	-0.01	0.02
	(0.04)	(0.04)
Unemployed people (log)	-0.06	-0.02
	(0.04)	(0.05)
Median HH income (log)	0.01	0.02
	(0.02)	(0.02)
People in poverty (log)	-0.12***	-0.11**
	(0.04)	(0.05)
China shock	-0.0091***	-0.0119***
	(0.0013)	(0.0016)
Adults with at most HS (log)	0.01	0.06
	(0.06)	(0.06)
Total adults(log)	0.02	0.04
	(0.03)	(0.03)
White adults (log)	0.04	0.06**
	(0.03)	(0.02)
Employment in construction (log)	0.05	0.11***
	(0.04)	(0.03)
Employment in hospitality (log)	-0.01	-0.03
	(0.03)	(0.04)
Out-migration (pc)	0.00	0.00
	(0.00)	(0.00)

Displayed are coefficients from regressing the change in the dependent variable during the pre-period on each of the two changes in the instruments in the period of analysis. The value for GOP share midterms, DW-NOMINATE, and the China shock corresponds to the 2002-2006 change. The value for GOP share presidential corresponds to the 2004-2008 change. The values for direct, education, and law and order expenditures correspond to the 2002-2007 change. The values for unemployed people, median household income, people in poverty, total and White adult population, and employment in construction and in hospitality correspond to the 2000-2005 change. The value for out-migration corresponds to the 2009-2011 change. The value for adults with at most high school corresponds to the 2000-2011 change. Sources: Dave Leip's U.S. Election Data, Lewis et al. (2021), Ferrara et al. (2024), Annual Survey of State and Local Government Finances, SAIPE, LAUS, US Census; USDA's Economic Research Service, Peter K. Schott's Data, County Business Patterns, Acemoglu et al. (2016), and QCEW. Estimations control for state by period fixed effects and are weighted by population. Standard errors clustered at the CBSA level (district for DW-NOMINATE). Stars indicate *p<0.1,**p<0.05,***p<0.01.

Table B2: Robustness checks

	Vote Share (GOP)	Score	Expenditure, log pc		
	(1) House Midterms	(2) DW-NOMINATE	(3) Total	(4) Education	(5) Law and order
A. Reduced form, baseline					
Instrument	9.24***	0.55***	-0.05***	-0.07***	0.07***
	(1.26)	(0.21)	(0.02)	(0.02)	(0.03)
B. Mex non-citizen, sh					
Instrument	9.27***	0.49**	-0.04*	-0.07***	0.07**
	(1.37)	(0.20)	(0.03)	(0.02)	(0.03)
C. Hispanics, sh					
Instrument	9.87***	0.55***	-0.05**	-0.07***	0.07***
	(1.39)	(0.21)	(0.02)	(0.02)	(0.02)
D. Adult HS completion, sh					
Instrument	8.66***	0.51*	-0.05***	-0.07***	0.07***
	(1.39)	(0.29)	(0.02)	(0.02)	(0.03)
E. People in poverty					
Instrument	6.39***	0.59**	-0.05***	-0.08***	0.07***
	(1.51)	(0.26)	(0.02)	(0.02)	(0.02)
F. China shock					
Instrument	8.35***	0.49**	-0.05***	-0.07***	0.07***
	(1.33)	(0.21)	(0.02)	(0.02)	(0.03)
G. Simulated instrument					
Instrument	9.42***	0.72***	-0.05**	-0.07***	0.08***
	(1.32)	(0.20)	(0.02)	(0.02)	(0.03)
H. Spatial lag					
Instrument	7.63***	0.55***	-0.05**	-0.05*	0.05**
	(1.50)	(0.20)	(0.02)	(0.03)	(0.02)
I. Stock Mex foreign					
Instrument	9.44***	0.52**	-0.05***	-0.07***	0.08***
	(1.30)	(0.20)	(0.02)	(0.02)	(0.02)
J. Stock Hispanics					
Instrument	8.46***	0.52***	-0.05**	-0.07***	0.08***
	(1.22)	(0.20)	(0.02)	(0.02)	(0.03)
K. No-outliers					
Instrument	10.25***	0.40**	-0.06**	-0.09***	0.09***
	(1.50)	(0.20)	(0.03)	(0.02)	(0.03)
L. No pop weights					
Instrument	10.34***	0.47**	-0.04*	-0.07***	0.07**
	(1.27)	(0.21)	(0.02)	(0.02)	(0.03)

Dependent variable in Column 1 is the vote share for Republicans in House midterm elections. Dependent variable in Column 2 is the DW-NOMINATE score. Dependent variables in Columns 3–5 are the log 2010 dollars per person (per child in Column 4). All estimations are reduced form. Sources: Dave Leip's U.S. Election Data, Lewis et al. (2021), Ferrara et al. (2024), Annual Survey of State and Local Government Finances, US Census, USDA's Economic Research Service, Peter K. Schott's Data, County Business Patterns, ACS 5 from the Social Explorer, Acemoglu et al. (2016), and QCEW. Panel 1 is reduced form estimation. Panel 2 controls for pre-period features interacted with period dummies. Panel 3 includes a simulated instrument following Borusyak and Hull (2020). Panel 4 controls for the spatial lag of the instrument (values of neighboring counties). Panel 5 controls for the stock of Mexican non-citizens and Hispanics at the beginning of period. Panel 6 excludes outliers and does not use predicted population weights. All regressions control for county and state-period fixed effects (congressional fixed effects in Column 2), and for a contemporaneous measure of the China shock, constructed like Autor et al. (2020), and are also weighted by predicted population, except row L. Standard errors clustered at CBSA level (congressional level in Column 2). Stars indicate *p<0.1,**p<0.05,***p<0.01.

Table B3: Unauthorized, low-skilled, and likely-authorized migrants

	Vote Share (GOP)	Score	Expenditure, log pc		
	(1) House Midterms	(2) DW-NOMINATE	(3) Total	(4) Education	(5) Law and order
No controls					
Instrument	5.00***	0.53**	-0.04	-0.04	0.02
	(1.48)	(0.21)	(0.03)	(0.03)	(0.04)
Observations	1250	524	726	722	726
Low-skilled Mexican					
Instrument	5.12**	0.83**	-0.06	-0.05	0.06
	(2.25)	(0.34)	(0.05)	(0.05)	(0.05)
Observations	1250	524	726	722	726
Low-skilled all					
Instrument	7.62***	0.69**	-0.06	-0.05	0.06
	(2.13)	(0.31)	(0.05)	(0.05)	(0.04)
Observations	1250	524	726	722	726
Likely authorized Mexican					
Instrument	4.99***	0.51**	-0.04	-0.04	0.02
	(1.47)	(0.21)	(0.03)	(0.03)	(0.04)
Observations	1250	524	726	722	726

Dependent variable in Column 1 is the vote share for Republicans in House midterm elections. Dependent variable in Column 2 is the DW-NOMINATE score. Dependent variables in Columns 3–5 are the log 2010 dollars per person (per child in Column 4). Sources: Dave Leip's U.S. Election Data, Lewis et al. (2021); Ferrara et al. (2024), and Annual Survey of State and Local Government Finances. All estimations are reduced form. Panel 1 does not have controls and it is restricted to counties with non-missing values of recent migrants and recent likely authorized Mexican migrants. Panel 2 controls for an estimate of recent "low-skilled" Mexican migrants. Panel 3 controls for an estimate of recent "low-skilled" migrants from around the world. Panel 4 controls for an estimate of likely authorized Mexican migrants. All regressions have county and state-period fixed effects (congressional fixed effects in Column 2), and are weighted by predicted population. Standard errors are clustered at the CBSA level (congressional level in Column 2). Stars indicate *p<0.1,**p<0.05,***p<0.01.

Table B4: Robustness checks

	Vote Share (GOP)	Score	Expenditure, log pc		
	(1) House Midterms	(2) DW-NOMINATE	(3) Total	(4) Education	(5) Law and order
A. Reduced form, baseline					
Instrument	9.51***	0.44*	-0.06***	-0.05***	0.06**
	(1.55)	(0.23)	(0.02)	(0.02)	(0.03)
B. Mex non-citizen, sh					
Instrument	8.45***	0.39*	-0.05**	-0.05**	0.05
	(1.76)	(0.22)	(0.02)	(0.02)	(0.03)
C. Hispanics, sh					
Instrument	10.09***	0.43*	-0.05***	-0.05***	0.06**
	(1.77)	(0.23)	(0.02)	(0.02)	(0.03)
D. Adult HS completion, sh					
Instrument	8.91***	0.35	-0.05***	-0.05***	0.06**
	(1.57)	(0.32)	(0.02)	(0.02)	(0.03)
E. People in poverty					
Instrument	5.20***	0.45	-0.06***	-0.08***	0.06**
	(1.84)	(0.30)	(0.02)	(0.02)	(0.03)
F. China shock					
Instrument	8.25***	0.40*	-0.06***	-0.06***	0.07**
	(1.64)	(0.23)	(0.02)	(0.02)	(0.03)
G. Simulated instrument					
Instrument	9.35***	0.69***	-0.06**	-0.06***	0.07**
	(1.59)	(0.22)	(0.02)	(0.02)	(0.03)
H. Spatial lag					
Instrument	7.09***	0.43*	-0.05**	-0.03	0.03
	(1.69)	(0.23)	(0.02)	(0.03)	(0.03)
I. Stock Mex foreign					
Instrument	9.91***	0.42*	-0.06***	-0.06***	0.07***
	(1.54)	(0.23)	(0.02)	(0.02)	(0.03)
J. Stock Hispanics					
Instrument	8.65***	0.42*	-0.06**	-0.05**	0.07**
	(1.41)	(0.23)	(0.02)	(0.02)	(0.03)
K. No-outliers					
Instrument	11.49***	0.29	-0.08***	-0.08***	0.09**
	(1.65)	(0.21)	(0.03)	(0.02)	(0.04)
L. No pop weights					
Instrument	11.14***	0.37	-0.05*	-0.06**	0.14***
	(1.71)	(0.22)	(0.03)	(0.03)	(0.05)

Dependent variable in Column 1 is the vote share for Republicans in House midterm elections. Dependent variable in Column 2 is the DW-NOMINATE score. Dependent variables in Columns 3–5 are the log 2010 dollars per person (per child in Column 4). All estimations are reduced form. Sources: Dave Leip's U.S. Election Data, Lewis et al. (2021), Ferrara et al. (2024), Annual Survey of State and Local Government Finances, US Census, USDA's Economic Research Service, Peter K. Schott's Data, County Business Patterns, ACS 5 from the Social Explorer, Acemoglu et al. (2016), and QCEW. Panel 1 is reduced form estimation. Panel 2 controls for pre-period features interacted with period dummies. Panel 3 includes a simulated instrument following Borusyak and Hull (2020). Panel 4 controls for the spatial lag of the instrument (values of neighboring counties). Panel 5 controls for the stock of Mexican non-citizens and Hispanics at the beginning of period. Panel 6 excludes outliers and does not use predicted population weights. All regressions control for county and state-period fixed effects (congressional fixed effects in Column 2), and for a contemporaneous measure of the China shock, constructed like Autor et al. (2020), and are also weighted by predicted population, except row L. Standard errors clustered at CBSA level (congressional level in Column 2). Stars indicate *p<0.1,**p<0.05,***p<0.01.

B.2 Alternative standard errors

Adão et al. (2019) show that in shift-share designs, standard errors are correlated with the composition of initial shares and argue that accounting for such association is more accurate than using heteroskedastic or geographically clustered standard errors. Since their Stata and R commands cannot easily accommodate a large set of fixed effects, we implement similar corrections. The first approach is to use cluster analysis and group counties based on the values of their 2,439 shares, varying the number of clusters and the technique to construct them. This results in several groups with one county and one group with around thousand counties. Our preferred approach is to obtain, via principal components analysis, the components of the 2,439 shares. We then create 500 equally-sized groups based on the values of the first component (100 for congressional district analysis). The last group includes Cook, Orange, Harris, Maricopa, and Los Angeles counties. Our results remain robust.

Table B5: Alternative standard error calculation

	Vote Share (GOP)	Score	Expenditure, log per person		
	(1) House Midterms	(2) DW-NOMINATE	(3) Total	(4) Education	(5) Law and order
A. Reduced form, baseline					
Instrument	9.854***	0.529**	-0.049***	-0.059***	0.063**
	(1.215)	(0.207)	(0.018)	(0.021)	(0.025)
Clustered at state-level					
Instrument	9.854***	0.529***	-0.049***	-0.059***	0.063**
	(1.424)	(0.109)	(0.016)	(0.017)	(0.031)
Eicker Huber White					
Instrument	9.854***	0.529**	-0.049***	-0.059***	0.063**
	(1.199)	(0.209)	(0.018)	(0.019)	(0.030)
PCA 1					
Instrument	9.837***	0.529***	-0.050***	-0.059***	0.063***
	(1.251)	(0.177)	(0.015)	(0.021)	(0.022)
Kmeans, 800 (pca)					
Instrument	9.837***	0.529***	-0.050***	-0.059***	0.063**
	(1.498)	(0.185)	(0.018)	(0.019)	(0.030)
Kmeans, 1000 (pca)					
Instrument	9.837***	0.529***	-0.050***	-0.059***	0.063**
	(1.443)	(0.185)	(0.018)	(0.019)	(0.030)
Kmeans, 800 (all shares)					
Instrument	9.837***	0.529***	-0.050***	-0.059***	0.063**
	(1.725)	(0.168)	(0.018)	(0.019)	(0.031)
Kmeans, 1000 (all shares)					
Instrument	9.837***	0.529***	-0.050***	-0.059***	0.063**
	(1.580)	(0.183)	(0.018)	(0.019)	(0.030)

Row 1 is the baseline 2SLS specification. Row 2 clusters the standard errors (SE) at the state level. Row 3 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 (100 for DW-NOMINATE). Rows 5–6 clusters SE at the level of one of 800–1000 groups obtained by classifying counties according to their first 10 components using kmeans (100 and 200 for DW-NOMINATE). Rows 7-8 clusters SEs at the level of one of 800–1000 groups obtained by classifying counties according to their shares using kmeans (100 and 2000 for DW-Nominate). All regressions control for county and state-period fixed effects, and are weighted by predicted population. Stars indicate *p<0.1,**p<0.05,***p<0.01.

B.3 Rotemberg weights

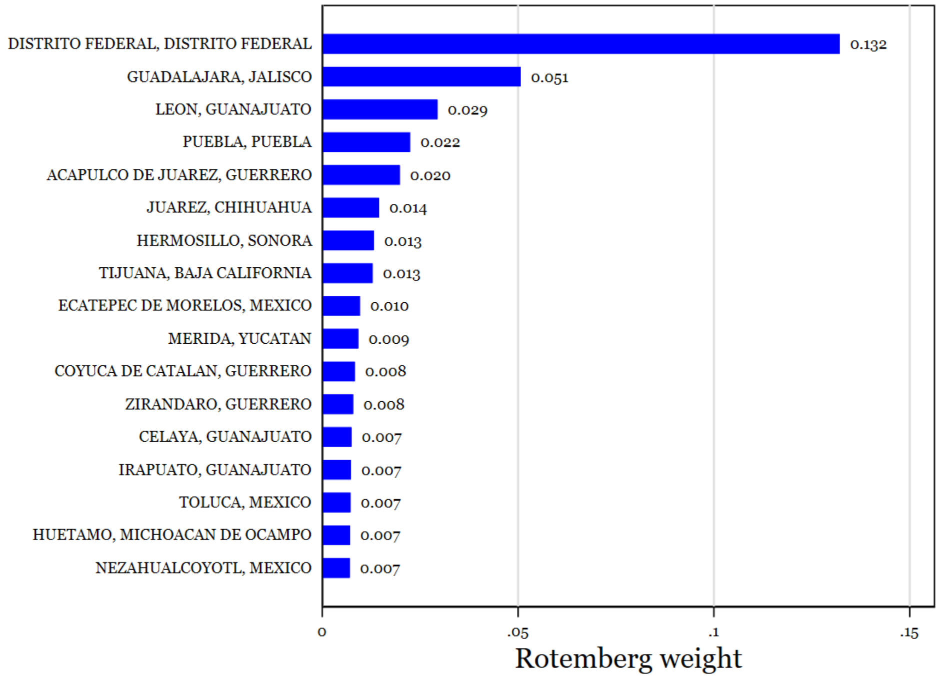

Figure B1: Rotemberg weight for push factor instrument, based on Goldsmith-Pinkham et al. (2020) code.

B.4 Robustness checks for mechanisms

Table B6: Robustness checks for mechanisms

	Log					Share	Index
	(1) Construction employment	(2) Hospitality employment	(3) Median household income	(4) People in poverty	(5) White adults	(6) Out-migration	(7) Universalist values
A. Reduced form, baseline							
Instrument	-0.06***	-0.02**	-0.03	0.08***	-0.03***	1.79**	-0.16***
	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.75)	(0.05)
B. Mex non-citizen, sh							
Instrument	-0.14***	0.01	-0.03**	0.06***	-0.04***	2.24*	-0.21***
	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)	(1.17)	(0.07)
C. Hispanics, sh							
Instrument	-0.08***	-0.01	-0.02	0.07***	-0.03***	1.93**	-0.18***
	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.79)	(0.05)
D. Adult HS completion, sh							
Instrument	-0.06***	-0.03**	-0.02	0.08***	-0.03**	1.93***	-0.16***
	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.73)	(0.05)
E. People in poverty							
Instrument	-0.07***	-0.00	-0.01	0.06***	-0.03***	1.71**	-0.09
	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.83)	(0.06)
F. China shock							
Instrument	-0.06***	-0.02*	-0.02	0.08***	-0.03***	2.06***	-0.12**
	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.75)	(0.05)
G. Simulated instrument							
Instrument	-0.06***	-0.02	-0.02	0.07***	-0.03**	1.96**	-0.15**
	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.92)	(0.06)
H. Spatial lag							
Instrument	-0.06**	0.00	-0.02	0.07***	-0.02	1.32	-0.14**
	(0.03)	(0.01)	(0.01)	(0.02)	(0.01)	(0.82)	(0.06)
I. Stock Mex foreign							
Instrument	-0.06***	-0.02**	-0.02	0.07***	-0.03***	1.61**	-0.13**
	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.75)	(0.05)
J. Stock Hispanics							
Instrument	-0.06***	-0.04**	-0.04**	0.09***	-0.03**	0.96	-0.18***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.76)	(0.06)
K. No-outliers							
Instrument	-0.08***	-0.04***	-0.04**	0.11***	-0.04***	1.97*	-0.22***
	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(1.04)	(0.07)
L. No pop weights							
Instrument	-0.08***	-0.01	-0.04***	0.06***	-0.07***	0.78	-0.26***
	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(1.93)	(0.09)

Dependent variables in Columns 1–2 are the the log of average annual employment divided by working age population. Dependent variables in Columns 3–5 are the log of median household income, people in poverty, and White adults. Dependent variable in Column 6 is out-migration, calculated as the number of out-migrants (in thousands) divided by county population. Dependent variable in Column 7 is the average county value, as reported by Enke (2020). All estimations are reduced form. Sources: QCEW, SAIPE, LAUS, US Census Bureau: Population Division and Small Area, US Census Bureau: 2007-2011, 2011-2015, and 2015-2019 American Community Surveys 5; USDA's Economic Research Service, Enke (2020), Peter K. Schott's Data, County Business Patterns, and Acemoglu et al. (2016). Panel 1 is reduced form estimation. Panel 2 controls for pre-period features interacted with period dummies. Panel 3 includes a simulated instrument following Borusyak and Hull (2020). Panel 4 controls for the spatial lag of the instrument (values of neighboring counties). Panel 5 controls for the stock of Mexican non-citizens and Hispanics at the beginning of period. Panel 6 excludes outliers and does not use predicted population weights. All regressions control for county and state-period fixed effects, and for a contemporaneous measure of the China shock, constructed like Autor et al. (2020), and are also weighted by predicted population, except row L. Standard errors clustered at CBSA level. Stars indicate *p<0.1,**p<0.05,***p<0.01.

Table B7: Robustness checks for mechanisms, push factors instrument

	Log					Share	Index
	(1) Construction employment	(2) Hospitality employment	(3) Median household income	(4) People in poverty	(5) White adults	(6) Out-migration	(7) Universalist values
A. Reduced form, baseline							
Instrument	-0.05*	-0.04***	-0.04**	0.09***	-0.03**	1.72**	-0.22***
	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)	(0.86)	(0.06)
B. Mex non-citizen, sh							
Instrument	-0.14***	-0.01	-0.05***	0.08***	-0.04***	1.97*	-0.32***
	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)	(1.16)	(0.08)
C. Hispanics, sh							
Instrument	-0.08***	-0.02*	-0.03**	0.09***	-0.03**	1.85**	-0.26***
	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)	(0.86)	(0.06)
D. Adult HS completion, sh							
Instrument	-0.05*	-0.04***	-0.03**	0.10***	-0.03**	1.83**	-0.22***
	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)	(0.85)	(0.06)
E. People in poverty							
Instrument	-0.07**	-0.01	-0.02	0.08***	-0.03**	1.31	-0.15**
	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)	(0.98)	(0.07)
F. China shock							
Instrument	-0.05*	-0.03**	-0.03*	0.09***	-0.03**	1.98**	-0.18***
	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)	(0.87)	(0.06)
G. Simulated instrument							
Instrument	-0.04	-0.03**	-0.03*	0.08***	-0.02*	1.81*	-0.23***
	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)	(1.06)	(0.07)
H. Spatial lag							
Instrument	-0.05*	-0.00	-0.03**	0.09***	-0.01	0.96	-0.21***
	(0.03)	(0.01)	(0.02)	(0.02)	(0.02)	(1.02)	(0.07)
I. Stock Mex foreign							
Instrument	-0.06**	-0.04***	-0.03*	0.09***	-0.03**	1.43*	-0.19***
	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)	(0.85)	(0.06)
J. Stock Hispanics							
Instrument	-0.05*	-0.05***	-0.05**	0.10***	-0.03**	1.01	-0.23***
	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)	(0.91)	(0.06)
K. No-outliers							
Instrument	-0.09***	-0.05***	-0.07***	0.14***	-0.05***	1.44	-0.29***
	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)	(1.28)	(0.08)
L. No pop weights							
Instrument	-0.10**	-0.01	-0.05***	0.07***	-0.08***	1.22	-0.32***
	(0.04)	(0.02)	(0.01)	(0.01)	(0.01)	(3.20)	(0.11)

Dependent variables in Columns 1–2 are the the log of average annual employment divided by working age population. Dependent variables in Columns 3–5 are the log of median household income, people in poverty, and White adults. Dependent variable in Column 6 is out-migration, calculated as the number of out-migrants (in thousands) divided by county population. Dependent variable in Column 7 is the average county value, as reported by Enke (2020). All estimations are reduced form. Sources: QCEW, SAIPE, LAUS, US Census Bureau: Population Division and Small Area, US Census Bureau: 2007-2011, 2011-2015, and 2015-2019 American Community Surveys 5; USDA's Economic Research Service, Enke (2020), Peter K. Schott's Data, County Business Patterns, and Acemoglu et al. (2016). Panel 1 is reduced form estimation. Panel 2 controls for pre-period features interacted with period dummies. Panel 3 includes a simulated instrument following Borusyak and Hull (2020). Panel 4 controls for the spatial lag of the instrument (values of neighboring counties). Panel 5 controls for the stock of Mexican non-citizens and Hispanics at the beginning of period. Panel 6 excludes outliers and does not use predicted population weights. All regressions control for county and state-period fixed effects, and for a contemporaneous measure of the China shock, constructed like Autor et al. (2020), and are also weighted by predicted population, except row L. Standard errors clustered at CBSA level. Stars indicate *p<0.1,**p<0.05,***p<0.01.

C Additional analysis

C.1 Other elections

Table C1: Political effects of arrival of unauthorized Mexican migrants 2010–2020

	Share			
	(1) Senate, midterm	(2) Senate, presidential Year	(3) President	(4) Governor, midterm year
A. OLS				
Newcomers, pct. pop.	2.24	1.74	3.35***	3.15***
	(1.73)	(1.61)	(0.63)	(0.72)
B. 2SLS LOO				
Newcomers, pct. pop.	4.53*	4.98**	4.63***	4.42***
	(2.33)	(2.07)	(0.70)	(0.96)
Std. Coefficient	0.14	0.16	0.17	0.17
$\hat{\beta} * P(75) - P(25)$	0.49	0.53	0.50	0.47
C. 2SLS Push Factors				
Newcomers, pct. pop.	1.40	2.04	4.82***	5.17***
	(1.66)	(1.56)	(0.72)	(0.94)
Std. Coefficient	0.04	0.07	0.17	0.20
$\hat{\beta} * P(75) - P(25)$	0.15	0.22	0.52	0.55
Observations	5381	5361	8019	6429
Dep. Var., Mean	44.99	43.10	46.04	47.69
Dep. Var., Sd	19.74	19.38	16.46	15.88
Ind. Var., Mean	0.48	0.48	0.46	0.49
Ind. Var., Sd	0.61	0.62	0.59	0.62

Dependent variables are share of Republican vote. Source: Dave Leip's U.S. 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. Instruments are as described in Section 3. All regressions have county and state-period fixed effects, and are weighted by predicted population. Standard errors are clustered at the CBSA level. Stars indicate *p<0.1,**p<0.05,***p<0.01.

C.2 Effects on Republican and Democratic turnout and registration

Table C2: Effect of arrival of unauthorized Mexican migrants on party registration composition (2010–18)

	Log			
	(1) Republican votes House midterm	(2) Democrat votes House midterm	(3) Republican Registration Midterm	(4) Democrat Registration Midterm
A. OLS				
Newcomers, pct. pop.	0.15***	-0.11***	0.01	-0.04**
	(0.03)	(0.03)	(0.02)	(0.02)
B. 2SLS LOO				
Newcomers, pct. pop.	0.20***	-0.17***	0.01	-0.07***
	(0.04)	(0.03)	(0.02)	(0.03)
Std. Coefficient	0.08	-0.06	0.01	-0.03
$\hat{\beta} * P(75) - P(25)$	0.021	-0.018	0.001	-0.007
C. 2SLS Push Factors				
Newcomers, pct. pop.	0.21***	-0.14***	0.03*	-0.08***
	(0.04)	(0.03)	(0.02)	(0.03)
Std. Coefficient	0.09	-0.05	0.01	-0.03
$\hat{\beta} * P(75) - P(25)$	0.023	-0.015	0.004	-0.009
Observations	7868	7502	3367	3367
Dep. Var., Mean	10.82	10.92	11.34	11.77
Dep. Var., Sd	1.39	1.78	1.36	1.57
Ind. Var., Mean	0.46	0.46	0.47	0.47
Ind. Var., Sd	0.59	0.59	0.58	0.58

Dependent variable in Columns 1–2 are the log of total votes for the Republican and Democratic parties in House midterm elections. Dependent variables in Columns 3–4 are the log of total registered Republicans and Democrats in midterm years. Sources: Dave Leip's U.S. 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. Source: SRE. Instrument is as described in Section 3. All regressions have county and state-period fixed effects, and are weighted by predicted population. Standard errors are clustered at the CBSA level. Stars indicate *p<0.1,**p<0.05,***p<0.01.

C.3 Other legislative change variables

Table C3: Political effects of arrival of unauthorized Mexican migrants 2010–2020

	DW-NOMINATE		Nokken Poole	
	(1) First dimension	(2) Second dimension	(3) First dimension	(4) Second dimension
A. OLS				
Newcomers, pct. pop.	0.33**	-0.14	0.34**	-0.11
	(0.15)	(0.10)	(0.15)	(0.09)
B. 2SLS LOO				
Newcomers, pct. pop.	0.40**	-0.06	0.38**	-0.03
	(0.15)	(0.11)	(0.16)	(0.12)
F stat, first stage	155.50	155.50	155.43	155.43
Std. Coefficient	0.47	-0.11	0.45	-0.05
$\hat{\beta} * P(75) - P(25)$	0.04	-0.01	0.04	-0.00
C. 2SLS Push Factors				
Newcomers, pct. pop.	0.31*	-0.06	0.29*	-0.05
	(0.16)	(0.10)	(0.17)	(0.11)
Std. Coefficient	0.37	-0.12	0.34	-0.08
F stat, first stage	139.00	139.00	138.94	138.94
$\hat{\beta} * P(75) - P(25)$	0.03	-0.01	0.03	-0.01
Observations	524	524	523	523
Dep. Var., Mean	0.06	0.02	0.06	0.04
Dep. Var., Sd	0.46	0.29	0.47	0.32
Ind. Var., Mean	0.53	0.53	0.53	0.53
Ind. Var., Sd	0.57	0.57	0.58	0.58

Dependent variables are the first and second dimensions of DW-NOMINATE and Nokken Poole score. Sources: Lewis et al. (2021), and Ferrara et al. (2024). Newcomers are the number of new consular IDs per congressional district per 4-year period as a proportion of predicted population as described in Section 3. Instruments are LOO state and push factors (corresponding to Columns 3 and 4 of table 1). All regressions have congressional district and state-period fixed effects, and are weighted by predicted population. Standard errors are clustered at the congressional district level. Stars indicate *p<0.1,**p<0.05,***p<0.01.

C.4 Effects on revenue

Table C4: Effect of arrival of unauthorized Mexican migrants on local revenues (2012 and 2017)

	Revenue categories (log pc 2010 USD)						Share of Total Revenue				
	(1) Total	(2) Own sources	(3) Total tax	(4) Property tax	(5) Income tax	(6) Inter-gov revenue	(7) Own sources	(8) Total tax	(9) Property tax	(10) Income tax	(11) Inter-gov revenue
A. OLS											
Newcomers, pct. pop.	-0.02*	-0.08***	-0.04***	-0.01	-1.37***	-0.02	-4.25***	-0.79**	0.08	-0.15	0.22
	(0.01)	(0.03)	(0.01)	(0.02)	(0.51)	(0.02)	(1.54)	(0.39)	(0.39)	(0.13)	(0.73)
B. 2SLS LOO											
Newcomers, pct. pop.	-0.03**	-0.04	-0.03*	0.01	-2.06***	-0.03	-1.63	-0.59	0.55	-0.15	-0.27
	(0.01)	(0.03)	(0.02)	(0.02)	(0.59)	(0.03)	(1.66)	(0.44)	(0.43)	(0.12)	(0.76)
Std. Coefficient	-0.05	-0.05	-0.04	0.02	-0.27	-0.05	-0.09	-0.03	0.03	-0.03	-0.02
$\hat{\beta} * P(75) - P(25)$	-0.003	-0.004	-0.003	0.001	-0.221	-0.003	-0.175	-0.063	0.059	-0.016	-0.029
C. 2SLS Push Factors											
Newcomers, pct. pop.	-0.03**	-0.07**	-0.06***	-0.02	-2.09***	-0.01	-3.52*	-1.24***	-0.01	-0.16	0.49
	(0.01)	(0.03)	(0.01)	(0.02)	(0.53)	(0.03)	(1.82)	(0.43)	(0.46)	(0.12)	(0.74)
Std. Coefficient	-0.05	-0.09	-0.08	-0.03	-0.27	-0.02	-0.19	-0.07	-0.00	-0.03	0.03
$\hat{\beta} * P(75) - P(25)$	-0.003	-0.007	-0.006	-0.003	-0.224	-0.001	-0.377	-0.133	-0.001	-0.017	0.052
Observations	5338	5338	5338	5338	1120	5338	5338	5338	5338	5338	5338
Dep. Var., Mean	8.48	7.93	7.41	7.09	4.51	7.41	59.46	36.31	27.58	1.32	36.11
Dep. Var., Sd	0.38	0.47	0.49	0.54	2.64	0.45	12.44	11.95	12.18	3.69	11.15
Ind. Var., Mean	0.55	0.55	0.55	0.55	0.20	0.55	0.55	0.55	0.55	0.55	0.55
Ind. Var., Sd	0.68	0.68	0.68	0.68	0.34	0.68	0.68	0.68	0.68	0.68	0.68
Inst. Loo, Mean	0.49	0.49	0.49	0.49	0.13	0.49	0.49	0.49	0.49	0.49	0.49
Inst. Loo, Sd	0.63	0.63	0.63	0.63	0.21	0.63	0.63	0.63	0.63	0.63	0.63

Dependent variables in Columns 1–6 are in log 2010 dollars per person. Dependent variables in Columns 7–11 are shares of total revenue. Sources: Annual Survey of State and Local Government Finances. Newcomers are the new consular IDs per county per 4-year period as a proportion of predicted population. All regressions control for county and state-period fixed effects, and are weighted by predicted population. Standard errors are clustered at the CBSA level. Stars indicate *p<0.1,**p<0.05,***p<0.01.

C.5 Additional economic effects

Table C5: Effect of arrival of unauthorized Mexican migrants on agriculture employment among working age population and weekly wages (2010-19)

	Employment (log per working age person)	Weekly Wages (log 2010 USD)
	(1) Agriculture	(2) Agriculture
A. OLS		
Newcomers, pct pop.	0.03	-0.03
	(0.06)	(0.02)
B. 2SLS LOO		
Newcomers, pct pop.	-0.08	-0.07**
	(0.08)	(0.03)
Std. Coefficient	-0.03	-0.16
$\hat{\beta} * P(75) - P(25)$	-0.008	-0.007
C. 2SLS Push Factors		
Newcomers, pct pop.	-0.04	-0.06**
	(0.09)	(0.02)
Std. Coefficient	-0.02	-0.15
$\hat{\beta} * P(75) - P(25)$	-0.005	-0.007
Observations	4461	4461
Dep. Var., Mean	-6.58	6.37
Dep. Var., Sd	1.52	0.27
Ind. Var., Mean	0.51	0.51
Ind. Var., Sd	0.63	0.63

Dependent variable in Column 1 is the log of average annual employment divided by working age population. Dependent variable in Column 2 is the log of annual average weekly wages in 2010 USD. Sources: (QCEW) and US Census Bureau: Population Division. 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 county and state-period fixed effects, and are weighted by predicted population. Standard errors clustered at the CBSA level. Stars indicate *p<0.1,**p<0.05,***p<0.01.

Table C6: Socioeconomic effects of arrival of unauthorized Mexican migrants (2010–19)

	Rate		Log
	(1) Poverty	(2) Unemployment	(3) SNAP recipients
A. OLS			
Newcomers, pct. pop.	1.17***	-0.03	0.01
	(0.22)	(0.15)	(0.02)
B. 2SLS LOO			
Newcomers, pct. pop.	1.59***	0.18	0.03*
	(0.26)	(0.18)	(0.02)
Std. Coefficient	0.18	0.04	0.01
$\hat{\beta} * P(75) - P(25)$	0.170	0.019	0.003
C. 2SLS Push Factors			
Newcomers, pct. pop.	1.56***	0.06	0.01
	(0.27)	(0.19)	(0.02)
Std. Coefficient	0.17	0.01	0.00
$\hat{\beta} * P(75) - P(25)$	0.167	0.006	0.001
Observations	8019	8019	8019
Dep. Var., Mean	14.36	6.02	10.81
Dep. Var., Sd	5.31	2.83	1.65
Ind. Var., Mean	0.46	0.46	0.46
Ind. Var., Sd	0.59	0.59	0.59

Dependent variables in Columns 1 and 2 are poverty rate and unemployment rate. Dependent variable in Column 3 is the log of SNAP recipients. Sources: SAIPE, and LAUS. 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 county and state-period fixed effects, and are weighted by predicted population. Standard errors clustered at the CBSA level. Stars indicate *p<0.1,**p<0.05,***p<0.01.

Table C7: Effect of arrival of unauthorized Mexican migrants on employment among working age population (2010–19)

	Employment (log per working age pop)		
	(1) Construction	(2) Manufacturing	(3) Hospitality and Leisure
A. OLS			
Newcomers, pct pop.	-0.013	0.015	-0.014
	(0.012)	(0.016)	(0.009)
B. 2SLS LOO			
Newcomers, pct pop.	-0.003	0.020	-0.016
	(0.013)	(0.015)	(0.011)
Std. Coefficient	-0.007	0.021	-0.040
$\hat{\beta} * P(75) - P(25)$	-0.000	0.002	-0.002
C. 2SLS Push Factors			
Newcomers, pct pop.	-0.001	0.041*	-0.020
	(0.016)	(0.022)	(0.013)
Std. Coefficient	-0.002	0.045	-0.050
$\hat{\beta} * P(75) - P(25)$	-0.000	0.004	-0.002
Observations	2564	2564	2564
Dep. Var., Mean	-3.10	-2.69	-2.74
Dep. Var., Sd	0.25	0.51	0.22
Ind. Var., Mean	0.56	0.56	0.56
Ind. Var., Sd	0.62	0.62	0.62

Dependent variables are the log of people 16–64 who are employed in a particular sector as share of the total population of people 16–64 in that locality. The geographic unit of observation is a county, when available in IPUMS, and a (consistent) PUMA, for counties that are not available. Sources: Ruggles et al. (2023). 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 locality and state-period fixed effects, and are weighted by predicted population. Standard errors clustered at the locality level. Stars indicate *p<0.1,**p<0.05,***p<0.01.

Table C8: Effect of arrival of unauthorized Mexican migrants on employment among working age population by demographic group (2010-19)

	Construction				Manufacturing				Hospitality and leisure			
	(1) White> HS	(2) White<= HS	(3) Regional stayers	(4) Regional newcomers	(5) White> HS	(6) White<= HS	(7) Regional stayers	(8) Regional newcomers	(9) White> HS	(10) White<= HS	(11) Regional stayers	(12) Regional newcomers
A. OLS												
Newcomers, pct pop.	0.028	0.008	-0.022*	0.025	0.036	-0.016	0.013	0.040	0.018	-0.051**	-0.018*	-0.004
	(0.025)	(0.025)	(0.011)	(0.031)	(0.032)	(0.042)	(0.016)	(0.027)	(0.022)	(0.024)	(0.009)	(0.022)
B. 2SLS LOO												
Newcomers, pct pop.	0.032	0.020	-0.017	0.059*	0.039**	-0.026	0.018	0.041	0.019	-0.042*	-0.019*	0.004
	(0.024)	(0.024)	(0.013)	(0.035)	(0.018)	(0.035)	(0.015)	(0.029)	(0.022)	(0.025)	(0.011)	(0.026)
Std. Coefficient	0.068	0.044	-0.039	0.093	0.043	-0.026	0.020	0.044	0.039	-0.087	-0.047	0.009
$\hat{\beta} * P(75) - P(25)$	0.003	0.002	-0.002	0.006	0.004	-0.003	0.002	0.004	0.002	-0.005	-0.002	0.000
C. 2SLS Push Factors												
Newcomers, pct pop.	0.036	0.030	-0.013	0.054	0.066***	-0.015	0.037*	0.074*	0.043	-0.043	-0.024*	0.008
	(0.029)	(0.030)	(0.017)	(0.044)	(0.025)	(0.044)	(0.021)	(0.043)	(0.030)	(0.033)	(0.014)	(0.034)
Std. Coefficient	0.077	0.067	-0.030	0.085	0.073	-0.015	0.040	0.078	0.089	-0.088	-0.057	0.018
$\hat{\beta} * P(75) - P(25)$	0.004	0.003	-0.001	0.006	0.007	-0.002	0.004	0.008	0.005	-0.005	-0.003	0.001
Observations	2564	2564	2564	2560	2564	2564	2564	2564	2564	2564	2564	2564
Dep. Var., Mean	-3.34	-2.74	-3.09	-3.19	-2.74	-2.76	-2.67	-2.82	-2.93	-2.65	-2.80	-2.48
Dep. Var., Sd	0.26	0.25	0.25	0.36	0.51	0.55	0.51	0.53	0.27	0.27	0.23	0.24
Ind. Var., Mean	0.56	0.56	0.56	0.56	0.56	0.56	0.56	0.56	0.56	0.56	0.56	0.56
Ind. Var., Sd	0.62	0.62	0.62	0.62	0.62	0.62	0.62	0.62	0.62	0.62	0.62	0.62
Inst. Loo, Mean	0.52	0.52	0.52	0.52	0.52	0.52	0.52	0.52	0.52	0.52	0.52	0.52
Inst. Loo, Sd	0.59	0.59	0.59	0.59	0.59	0.59	0.59	0.59	0.59	0.59	0.59	0.59

Dependent variables are the log of people 16–64 from the relevant demographic group who are employed in a particular sector as share of the total population (16–64) of that group in that locality. <= high school (up to high school) and > (more than high school high school) are mutually exclusive groups. White people are those who self-identify as White non-Hispanic and are US citizens. Following Dustmann et al. (2025), stayers are those people who lived in the county the year before the survey took place, newcomers are those who did not. The geographic unit of observation is a county, when available in IPUMS, and a (consistent) PUMA, for counties that are not available. Sources: Ruggles et al. (2023). 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 locality and state-period fixed effects, and are weighted by predicted population. Standard errors clustered at the locality level. Stars indicate *p<0.1,**p<0.05,***p<0.01.

Table C9: Effect of arrival of unauthorized Mexican migrants on adult poverty, by demographic group (2010-19)

	Poverty (log of population)					
	(1) Established Mexican migrants	(2) Established non-Mexican migrants	(3) Hispanic US citizens	(4) White US citizens	(5) Regional stayers	(6) Regional newcomers
A. OLS						
Newcomers, pct pop.	0.075*	0.046	0.129***	0.017	0.036	0.103***
	(0.045)	(0.046)	(0.037)	(0.034)	(0.033)	(0.034)
B. 2SLS LOO						
Newcomers, pct pop.	0.079	0.027	0.151***	0.027	0.046	0.122***
	(0.061)	(0.055)	(0.040)	(0.038)	(0.036)	(0.040)
Std. Coefficient	0.020	0.008	0.044	0.014	0.019	0.053
$\hat{\beta} * P(75) - P(25)$	0.008	0.003	0.016	0.003	0.005	0.013
C. 2SLS Push Factors						
Newcomers, pct pop.	0.084	0.043	0.158***	0.025	0.056*	0.118***
	(0.068)	(0.064)	(0.042)	(0.043)	(0.034)	(0.042)
Std. Coefficient	0.021	0.012	0.047	0.013	0.023	0.052
$\hat{\beta} * P(75) - P(25)$	0.009	0.005	0.017	0.003	0.006	0.013
Observations	2212	2549	2564	2564	2564	2564
Dep. Var., Mean	7.56	7.89	9.12	9.84	10.43	9.35
Dep. Var., Sd	2.22	1.94	1.91	1.07	1.33	1.29
Ind. Var., Mean	0.58	0.56	0.56	0.56	0.56	0.56
Ind. Var., Sd	0.63	0.62	0.62	0.62	0.62	0.62

Dependent variables are the log of people 16–64 from the relevant demographic group who, according to their total annual family income, live in poverty. The sample for Column 1(2) is Mexican(non-Mexican) migrants who have been in the US for more than 5 years. White people are those who self-identify as White non-Hispanic and are US citizens. Hispanic people are those who self-identify as Hispanics; the sample is restricted to US citizens. Following Dustmann et al. (2025), stayers are those people who lived in the county the year before the survey took place, newcomers are those who did not. The geographic unit of observation is a county, when available in IPUMS, and a (consistent) PUMA, for counties that are not available. Sources: Ruggles et al. (2023). 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 locality and state-period fixed effects, and are weighted by predicted population. Standard errors clustered at the locality level. Stars indicate *p<0.1,**p<0.05,***p<0.01.

C.6 Changes in Hispanic population and Hispanic turnout

Table C10: Effect of arrival of unauthorized Mexican migrants on demographic composition (2010–19)

	Adult Population (log)		Share
	(1) Hispanic population	(2) Hispanic voters	(3) Hispanic voters
A. OLS			
Newcomers, pct. pop.	0.06***	-0.19***	0.01
	(0.01)	(0.05)	(0.01)
B. 2SLS LOO			
Newcomers, pct. pop.	0.08***	-0.53***	-0.04**
	(0.01)	(0.11)	(0.02)
Std. Coefficient	0.02	-0.09	-0.11
$\hat{\beta} * P(75) - P(25)$	0.009	-0.057	-0.004
C. 2SLS Push Factors			
Newcomers, pct. pop.	0.09***	-0.58***	-0.05**
	(0.01)	(0.16)	(0.02)
Std. Coefficient	0.02	-0.10	-0.13
$\hat{\beta} * P(75) - P(25)$	0.010	-0.062	-0.005
Observations	8019	5342	5346
Dep. Var., Mean	10.22	8.68	0.27
Dep. Var., Sd	2.43	2.42	0.16
Ind. Var., Mean	0.46	0.35	0.35
Ind. Var., Sd	0.59	0.42	0.42

Dependent variables in Columns 1–2 are the log of adult Hispanic population and voters who voted in House midterm elections. Dependent variable in Column 3 is the ratio of Hispanic voters who voted in House midterm elections to Hispanic adult population. Sources: US Census Bureau: Population Division and Small Area, and L2 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. Source: SRE. Instrument is as described in Section 3. All regressions have county and state-period fixed effects, and are weighted by predicted population. Standard errors clustered at the CBSA level. Stars indicate *p<0.1,**p<0.05,***p<0.01.

C.7 Turnout by demographic groups

Table C11: Effect of arrival of unauthorized Mexican migrants on turnout by demographic group (2010–18)

	Log				Share		
	(1) White	(2) Hispanic	(3) Black	(4) East Asian	(5) White	(6) Hispanic	(7) Black
A. OLS							
Newcomers, pct. pop.	0.06**	-0.19***	0.07	-0.09	0.04***	0.01	-0.07***
	(0.03)	(0.05)	(0.16)	(0.05)	(0.01)	(0.01)	(0.02)
B. 2SLS LOO							
Newcomers, pct. pop.	-0.06	-0.53***	-0.72**	-0.54***	0.08***	-0.04**	-0.20***
	(0.07)	(0.11)	(0.34)	(0.14)	(0.02)	(0.02)	(0.05)
Std. Coefficient	-0.02	-0.09	-0.11	-0.10	0.28	-0.11	-0.53
$\hat{\beta} * P(75) - P(25)$	-0.006	-0.057	-0.077	-0.058	0.008	-0.004	-0.021
C. 2SLS Push Factors							
Newcomers, pct. pop.	-0.12	-0.58***	-0.93**	-0.62***	0.11***	-0.05**	-0.24***
	(0.08)	(0.16)	(0.44)	(0.17)	(0.02)	(0.02)	(0.07)
Std. Coefficient	-0.03	-0.10	-0.15	-0.11	0.40	-0.13	-0.64
$\hat{\beta} * P(75) - P(25)$	-0.013	-0.062	-0.100	-0.066	0.012	-0.005	-0.026
Observations	5346	5342	5016	5284	5346	5346	5118
Dep. Var., Mean	11.24	8.68	8.09	7.44	0.36	0.27	0.21
Dep. Var., Sd	1.43	2.42	2.71	2.37	0.12	0.16	0.16
Ind. Var., Mean	0.35	0.35	0.35	0.35	0.35	0.35	0.35
Ind. Var., Sd	0.42	0.42	0.42	0.42	0.42	0.42	0.42

Dependent variable in Columns 1–4 are the log of White, Hispanic, Black, and East Asian voters who voted in House midterm elections. Dependent variables in Columns 5–7 are the ratio of White, Hispanic, and Black voters who voted in House midterm elections out relative to their respective adult populations. Sources: US Census Bureau: Population Division and Small Area, and L2 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. 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<0.1,**p<0.05,***p<0.01.

D Heterogeneity analysis

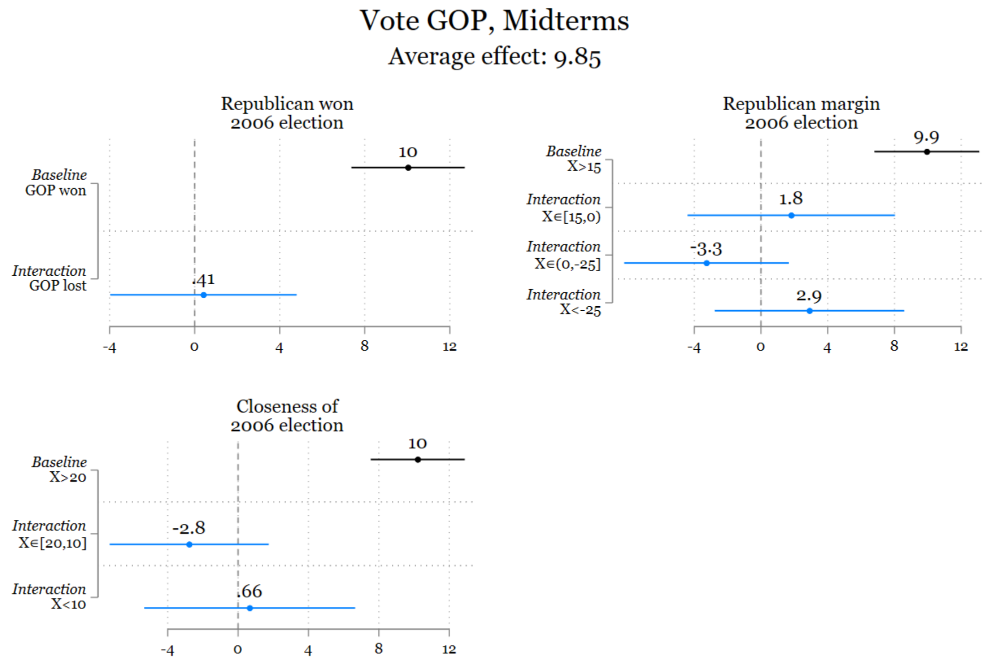

Figure D1: Effects of recent newcomers on the vote share for the Republican candidate in midterm elections by pre-period political behavior.

The first graph presents the differential effect of predicted newcomers based on whether the Republican candidate won the county's vote in the 2006 House election. The baseline category (black line) consists of counties where the Republican candidate won, and the blue line displays the interactive effect in counties where the Republican candidate lost. The second picture presents the differential effect based on the margin between the two parties' candidates in the 2006 House election. The baseline category (black line) comprises counties where the Republicans won by over 15 percentage points. The first group consists of counties where the Republicans won by less than 15 pp. The second group consists of counties where the Democrat won by less than 25 pp. The third group comprises counties where the Democrat won by more than 25 pp. The third picture presents the differential effect based on the competitiveness of the 2006 House election. The omitted category (black line) comprises counties where either party's candidate won by more than 20 pp. The first group consists of counties where either party's candidate won by between 20 and 10 pp. The second group consists of counties where the margin was less than 10 pp. Displayed are the coefficients of the interaction between the LOO instrument and the relevant dummy. Estimations are reduced form. The width of the line displays the 95% confidence interval. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population.

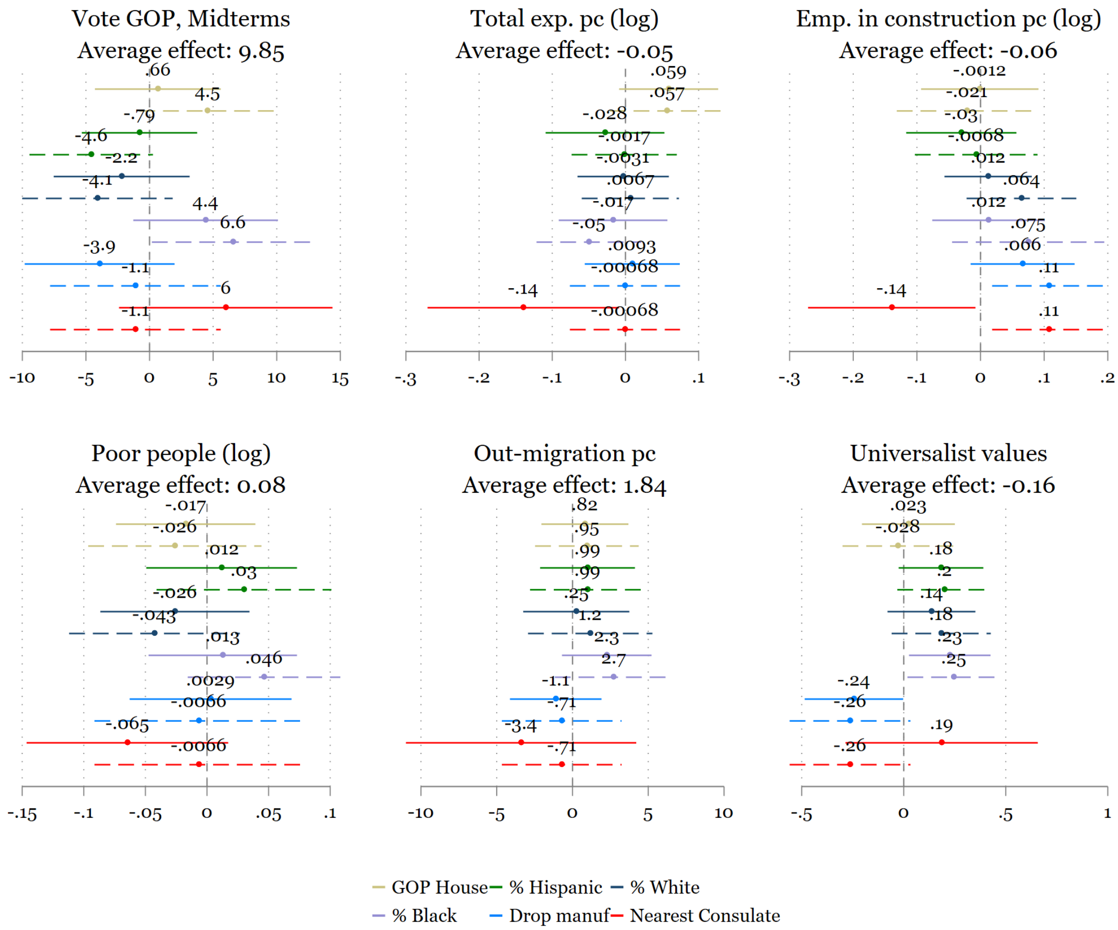

Figure D2: Heterogenous effects by selected characteristics.

Displayed are the 95% coefficient intervals of the interaction between the instruments and dummy indicating above median values of: vote share for the Republican candidate in the House midterm elections of 2006, share of adults who identify as Hispanic, White and Black in 2006, drop in employment in manufacturing (as share of total employment) between 1990 and 2005, and distance to the nearest consulate. Estimations are reduced form. Solid lines correspond to the LOO instrument and dashed lines to the push factors instrument. Sources: Enke (2020), US Census Bureau: Population Division and Small Area, Ruggles et al. (2023), QCEW, SAIPE, US Department of Commerce: Bureau of Economic Analysis, and Annual Survey of State and Local Government Finances. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population.

Table D1: Heterogeneity by robustness of safety net

	Vote share	Log			Share	Index
	(1) GOP House	(2) Total expenditure	(3) Construction employment	(4) People in poverty	(5) Out-migration	(6) Universalist values
A. Ratio of sales and property tax revenue to own revenue						
Instrument	11.81***	-0.03	-0.11***	0.11***	0.93	-0.24***
	(1.67)	(0.02)	(0.04)	(0.02)	(1.15)	(0.08)
Sales and Property, county × Instrument	-3.17	-0.03	0.10**	-0.08***	1.67	0.16
	(2.50)	(0.04)	(0.05)	(0.03)	(1.62)	(0.11)
B. Index of tax inequality (state)						
Instrument	9.09***	-0.06***	-0.08***	0.10***	0.87	-0.23***
	(1.35)	(0.02)	(0.02)	(0.01)	(0.86)	(0.07)
Tax equality index, state × Instrument	2.07	0.02	0.06	-0.05**	2.64*	0.18*
	(2.68)	(0.04)	(0.04)	(0.03)	(1.41)	(0.10)
C. Minimum wage (state)						
Instrument	12.52***	-0.02	-0.05*	0.10***	-0.47	-0.41***
	(1.35)	(0.03)	(0.03)	(0.02)	(1.28)	(0.08)
Minimum wage, state × Instrument	-4.18**	-0.04	-0.02	-0.04	3.63**	0.40***
	(2.12)	(0.04)	(0.04)	(0.03)	(1.51)	(0.10)
D. TANF-poverty ratio (state)						
Instrument	9.37***	-0.06***	-0.08***	0.08***	0.91	-0.20***
	(1.39)	(0.02)	(0.02)	(0.02)	(0.84)	(0.06)
TANF/poverty rate, state × Instrument	1.31	0.03	0.06	-0.03	2.55*	0.12
	(2.64)	(0.04)	(0.04)	(0.03)	(1.45)	(0.11)
Observations	7995	5338	7491	8019	8017	5712
Dep. Var., Mean	48.16	8.44	-3.62	10.89	55.16	0.15
Dep. Var., Sd	19.44	0.38	0.45	1.63	17.00	0.50

Dependent variable in Column 1 is the share of GOP vote in midterm House elections. Dependent variable in Column 2 is the log of direct expenditures per person. Dependent variable in Column 3 is the log of employment in construction per working age population. Dependent variable in Column 4 is the log of the number of poor people. Dependent variable in Column 5 is out-migration per 1,000 inhabitants. Dependent variable in Column 6 is the index of universalist values following Enke (2020). Estimations are reduced form. We interact the instrument and fixed effects with an above-median indicator of the strength of the safety net/redistribution. Panel A interacts the instrument with an indicator of whether the county is above or below the relative contribution of sales and property tax in 2007 (Annual Survey of State and Local Government Finances). Above equals less importance of sales and property tax, which suggests a more progressive fiscal policy. This criteria varies at the county level. Panel B interacts the instrument with an indicator of whether the county is in a state that was above or below the median of an index of tax equality in 2007, according to the Institute for Taxation and Economic Policy (Davis et al., 2009). Above suggests a more progressive fiscal policy. This criteria varies at the state level. Panel C interacts the instrument with an indicator of whether the county is in a state that had a minimum wage, adjusted by price differences, above or below the median in 2007 (US Department of Labor, Wage and Hour Division, 2024). Above suggests a more progressive fiscal policy. This criteria varies at the state level. Panel D interacts the instrument with an indicator of whether the county is in a state that was above or below the median TANF to poverty ratio in 2005-06 (Shrivastava and Thompson, 2021). Above suggests a more progressive fiscal policy. This criteria varies at the state level. Sources: Enke, (2020), US Census Bureau: Population Division and Small Area, Ruggles et al. (2023), QCEW, SAIPE Program, US Department of Commerce: Bureau of Economic Analysis, Annual Survey of State and Local Government Finances, Davis et al. (2009), US Department of Labor, Wage and Hour Division, and Shrivastava and Thompson (2021). Regressions are reduced form. 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, and are weighted by predicted population. Stars indicate *p<0.1,**p<0.05,***p<0.01.

Table D2: Heterogeneity by robustness of safety net

	Vote share	Log			Share	Index
	(1) GOP House	(2) Total expenditure	(3) Construction employment	(4) People in poverty	(5) Out-migration	(6) Universalist values
A. Ratio of sales and property tax revenue to own revenue						
Instrument	13.64***	-0.04*	-0.12***	0.13***	0.27	-0.34***
	(1.78)	(0.02)	(0.04)	(0.02)	(1.34)	(0.09)
Sales and Property, county × Instrument	-5.56**	-0.02	0.14**	-0.08***	2.65	0.21
	(2.80)	(0.05)	(0.06)	(0.03)	(1.90)	(0.13)
B. Index of tax inequality (state)						
Instrument	10.30***	-0.06***	-0.10***	0.11***	0.54	-0.29***
	(1.56)	(0.02)	(0.03)	(0.01)	(1.04)	(0.08)
Tax equality index, state × Instrument	0.27	0.03	0.11**	-0.06*	3.20**	0.18
	(3.30)	(0.04)	(0.06)	(0.03)	(1.61)	(0.11)
C. Minimum wage (state)						
Instrument	13.81***	-0.03	-0.06**	0.11***	-1.30	-0.51***
	(1.79)	(0.03)	(0.03)	(0.02)	(1.63)	(0.10)
Minimum wage, state × Instrument	-5.30**	-0.03	0.01	-0.04	4.82***	0.44***
	(2.59)	(0.04)	(0.05)	(0.03)	(1.86)	(0.12)
D. TANF-poverty ratio (state)						
Instrument	10.75***	-0.07***	-0.10***	0.10***	0.91	-0.27***
	(1.60)	(0.02)	(0.03)	(0.01)	(1.00)	(0.07)
TANF/poverty rate, state × Instrument	-0.88	0.04	0.11**	-0.03	2.26	0.12
	(3.20)	(0.04)	(0.06)	(0.03)	(1.64)	(0.12)
Observations	7995	5338	7491	8019	8017	5712
Dep. Var., Mean	48.16	8.44	-3.62	10.89	55.16	0.15
Dep. Var., Sd	19.44	0.38	0.45	1.63	17.00	0.50

Dependent variable in Column 1 is the share of GOP vote in midterm House elections. Dependent variable in Column 2 is the log of direct expenditures per person. Dependent variable in Column 3 is the log of employment in construction per working age population. Dependent variable in Column 4 is the log of the number of poor people. Dependent variable in Column 5 is out-migration per 1,000 inhabitants. Dependent variable in Column 6 is the index of universalist values following Enke (2020). Estimations are reduced form. We interact the instrument and fixed effects with an above-median indicator of the strength of the safety net/redistribution. Panel A interacts the instrument with an indicator of whether the county is above or below the relative contribution of sales and property tax in 2007 (Annual Survey of State and Local Government Finances). Above equals less importance of sales and property tax, which suggests a more progressive fiscal policy. This criteria varies at the county level. Panel B interacts the instrument with an indicator of whether the county is in a state that was above or below the median of an index of tax equality in 2007, according to the Institute for Taxation and Economic Policy (Davis et al., 2009). Above suggests a more progressive fiscal policy. This criteria varies at the state level. Panel C interacts the instrument with an indicator of whether the county is in a state that had a minimum wage, adjusted by price differences, above or below the median in 2007 (US Department of Labor, Wage and Hour Division, 2024). Above suggests a more progressive fiscal policy. This criteria varies at the state level. Panel D interacts the instrument with an indicator of whether the county is in a state that was above or below the median TANF to poverty ratio in 2005-06 (Shrivastava and Thompson, 2021). Above suggests a more progressive fiscal policy. This criteria varies at the state level. Sources: Enke, (2020), US Census Bureau: Population Division and Small Area, Ruggles et al. (2023), QCEW, SAIPE Program, US Department of Commerce: Bureau of Economic Analysis, Annual Survey of State and Local Government Finances, Davis et al. (2009), US Department of Labor, Wage and Hour Division, and Shrivastava and Thompson (2021). Regressions are reduced form. 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, and are weighted by predicted population. Stars indicate *p<0.1,**p<0.05,***p<0.01.