The Pearson Institute Discussion Paper No. 2024-14

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THE PEARSON INSTITUTE
FOR THE STUDY AND RESOLUTION OF GLOBAL CONFLICTS

THE LOCAL REACTION TO UNAUTHORIZED MEXICAN MIGRATION TO THE US*

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Current version: October 2024. The latest version is available here.

Abstract

We study the political and socioeconomic impacts of unauthorized Mexican migration to the United States. Our identification strategy relies on two shift-share instruments that combine variation in migration inflows and migrant networks using novel data from 7.4 million likely unauthorized migrants who obtained Mexican consular IDs. We document conservative electoral and policy responses at the level of a US county. Recent unauthorized migration increases the vote share of the Republican Party in federal elections. It decreases total public expenditure, prompting reallocation shifts from education to policing and the administration of justice. Three mechanisms partially explain these effects: economic grievance, reflected in formal job losses in "migrant-intensive" sectors and an associated marginal increase in poverty; White flight and residential sorting; and higher out-group bias. Migration inflows have no discernible impact on average wages, unemployment, or crime. Correlational evidence suggests the effects are smaller in counties with more progressive taxation or a stronger social safety net. These policy levers may facilitate job switching and prevent out-group bias.
JEL Codes: D72, F22, H7, H53, J61, J15

*We thank Dan Black, Steven Block, Leah Boustan, Kyle Emerick, Hilary Hoynes, Matt Gudgeon, Jeff Grogger, Eric Hansen, Dan Hopkins, Cynthia Kinnan, Agustin Markarian, Margaret McMillan, Juan Pereira, Giovanni Peri, Adam Storeygard, Moritz Marbach, Rachel Fordham, and Marco Tabellini for helpful comments. Ernesto is especially grateful to his advisers, Eoin McGuirk, Horacio Larreguy, and Enrico Spolaore. We appreciate Roberto Velasco, Arturo Rocha, and Leonor Ortíz from the Mexican Ministry of Foreign Affairs for helping us with the main dataset. We are grateful to participants at the Tufts EPP Seminar, Humans LACEA, the Midwest Political Science Association Annual Meeting, The Borders and Boundaries Conference at Perry World House, the Harvard Political Economy Workshop, The Economics of Migration Junior seminar, ITAM Political Science seminar, the Loyola University Chicago Political Science Brownbag, WEAI's Graduate Student Workshop, the 2nd Summer School on the "Economics of Migration," NEWEPS-21, Goldman School of Public Policy, 5th EBRD & King's College London Workshop on the Economics and Politics of Migration, and All CA-Mexico Economics of Migration. All errors are our own.

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1 Introduction

What are the electoral and policy responses of wealthy countries to immigration from poorer countries? The weight of evidence points at a conservative, anti-immigrant response.[1] Studies from Europe find that inflows of refugees and migrants tend to boost support for right-wing parties (Otto and Steinhardt, 2014; Barone et al., 2016; Halla et al., 2017; Dinas et al., 2019; Edo et al., 2019; **?**). Similarly, studies from the United States identify a conservative reaction to European migrants historically (Tabellini, 2020) and to "low-skilled" migrants more recently (Mayda et al., 2022a,b). However there is evidence of heterogeneous impacts. In Europe sustained, voluntary contact with refugees shifts political attitudes to the left (Dustmann et al., 2019; Steinmayr, 2021). In the United States, more "high-skilled" migration does not prompt a conservative response (Mayda et al., 2022a,b). The characteristics of migrants and the context of their interaction with local residents shape the political response.

Does an inflow of unauthorized migrants provoke a reaction similar to an influx of refugees or poorly educated migrants? 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 party's vote share in federal elections and public spending at the local level. Unauthorized Mexican migrants constitute a salient group in the US politics. At 11 million people, the Mexican-born population in the US constitutes the largest diaspora in a single country in the world (United Nations Department of Economic and Social Affairs, 2021). Roughly 40% of migrants have no work authorization (Gonzalez-Barrera, 2021). US political discourse invokes these migrants regularly, and yet there is scant evidence of their impacts on politics or policies—probably because they are challenging to quantify.

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 likely unauthorized Mexican citizens living in the US between 2002 and 2020. While versions (often more limited) of this data have been used (Allen et al., 2018; Caballero et al., 2018; Di-

[1] See Alesina and Tabellini (2024) for a comprehensive literature review.

narte 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 American Community Survey (ACS) data (Borjas and Cassidy, 2019)—which approximates unauthorized migrants using low education attainment—or apprehensions at the border (Hanson and Spilimbergo, 1999). These samples are not representative, as not all unauthorized migrants are poorly educated, and those who are captured at the border may differ from those who enter successfully. 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. As we detail in Section 2.2, this data closely matches available estimates of the unauthorized Mexican population in the US.

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 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. 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 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 predicted 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).

Our main results establish a conservative response in voting and policy. Inflows of unauthorized migrants increase the vote share for the Republican Party in federal elections, reduce local public spending, and shift spending 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) boosts the Republican party 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 but there is higher turnout among White voters, our interpretation is that the electoral boost is from votes among independents and changes in the composition of the electorate. These results are larger but similar to political reactions to migration measured in other settings (Harmon, 2018; Dinas et al., 2019; Dustmann et al., 2019; Mayda et al., 2022a).

The impacts on public spending are consistent with the Republican agenda. A smaller government and a focus on law-and-order are two of the key tenets of conservatism in the US. 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%. The same flow increases relative spending on police and on the administration of justice by 0.04 and 0.02 percentage points, respectively. These impacts on relative spending suggest that the decrease in total expenditure does not simply reflect a reduction in tax revenues but also a conservative change in spending priorities. These results are in line with theories suggesting that lower spending in response to immigration can be due to 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).

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 GDP per capita and median household income. We also observe residential sorting. Migration inflows increase out-migration and lower the number non-poor and White residents. Finally, the inflows cause a rise in out-group bias—captured by the relative universalism index (Enke, 2020).

Taken together, these results provide evidence in favor of both cultural and economic reasons behind the backlash to immigration. 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 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 towards migrants. The fact that the response to the out-group is more intense in counties that experience more poverty or formal job loss suggests economic grievance is partially driving the response. 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 non-poor, White, 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 the Republican Party is between a third and a fifth smaller, there is job gain in construction, only a marginal increase in the poverty rate 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 redistribution.

This paper 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).

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). Two recent articles explore similar topics. Mayda et al. (2022a) study the impact of "high-skilled" and "low-skilled" immigration 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 "low-skilled" immigration and increase with "high-skilled" immigration.[2] We add to this work by focusing analysis on unauthorized migrants. This group is sizable, a focus of political contention, and theoretically distinct from authorized migrants, refugees, or "low-skilled" migrants. Among the differences, unauthorized migrants experience higher barriers to formal employment. Their wages are likely lower than those of comparable authorized migrants. In sectors with high levels of informal labor, there is greater scope for labor substitution relative to other migrant groups studied.

Our paper illuminates the mechanisms through which immigration influxes prompt conservative shifts. We highlight the role of economic and cultural/ideological 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), po-

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

tentially 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 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 use them strategically to gain office (Alsan and Yang, 2019; 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 inter-group 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 there are economic winners. Further, turnout of White voters increased, despite this group not being the most economically affected. Negative out-group bias and economic grievance are probably related, since the increase in the former is steeper in counties with higher changes in poverty. Like the effects in residential sorting, economic factors do not seem to explain out-migration—relatively wealthier residents are more likely to move as a result of inflows of migrants and thus lower median income and GDP per capita. The null results on crime weaken an explanation based on actual threat. 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 imply 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 formal 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. 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 rather 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. Therefore, the consular dataset is well-suited to study flows, rather 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.[3] 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 between 2011 and 2014, and 0.95 between 2015 and 2018. Figure A1 shows these numbers are consistent with other common estimates (Passel 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 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,[4] 88% of the total.

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

[4]To protect people living in areas with very few migrants, we only consider counties with more than 10

[Figure 1]

2.2 Validating Migration Flows Created with Consular Data

There is evidence that the consular dataset captures unauthorized migrants well. Because migrants with valid visas or work authorization have access to identification from US authorities, the working assumption among scholars using versions of this data is that it captures predominantly unauthorized migrants (Bhandari et al., 2021; Albert and Monras, 2022). Caballero et al. (2018) report a strong correlation between the (log) number of cards issued in each state between 2006 and 2010 and the (log) estimated number of Mexican-born residents obtained from the 2010 and 2011 ACS. We carry out a similar analysis using ACS 5 2006–10, 2010–14 and 2014–18 (Ruggles et al., 2022). Following the Allen et al. (2018), we consider a likely unauthorized newcomer Mexican migrant in the ACS 5 as those who were born in Mexico, with no US citizenship, with no college education, and who have been in the US for less than 4 years. Figure A2 plots the log of likely unauthorized migrants from our data and two repositories of ACS 5.[5] The correlation coefficients are over 0.8. Further, Table A1 compares key demographic variables of 441 counties covered by ACS 5 in IPUMS (Ruggles et al., 2022) and the consular data and finds no significant differences.

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 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 in 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 from 2002 to 2020. We exclude Alaska because counties changed over this period.

[5]Our estimate of recent Mexican migrants is the number of people born in Mexico multiplied by the county average share of migrants (from all countries) that arrived before 2000, for 2010, or before 2010, for 2014 and 2018.

migrants is weak and barely significant.[6]

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. Moreover, 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 migrants on the vote share for the Republican Party in Congressional and presidential elections. The electoral data comes from Dave Leip's Atlas of US Presidential Elections (Leip, 2022). We focus on elections in midterm and presidential years. For midterms, we

[6]Table A2 indicates that a marginal increase in the LOO instrument is associated with an expected increase of 0.553 in the proxy of unauthorized migrants—F statistic 192. In contrast, a marginal increase in the LOO instrument is associated with an expected increase of only 0.028 in the proxy of authorized migrants —F statistic 4.

analyze the elections of 2010, 2014, and 2018. For presidential year elections, we analyze 2012, 2016, and 2020. Since the Senate renews only partially every election cycle, we focus on House and presidential elections.

Second, we examine public good provision with county-level revenue and expenditures, and focus on spending in public education, policing, and the judiciary. 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.

Since *a priori* it is ambiguous whether migration affects absolute or relative expenditure, we explore both. The absolute expenditures are the log of total expenditures per capita, in 2010 thousand dollars. For this and all other per capita measures, we use US Census data for county population. The relative expenditures for education, police, and justice are the shares they represent of total direct expenditures. 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 sub-sample 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 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.[7]

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, CA, 7.5% in Ventura County, CA, and 5.8% in Milwaukee County, WI.

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 that 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 ($0.092\times(550–52)$), 39.8 ($0.075\times(550–21)$), and 26.6 ($0.058\times(550–93)$) 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

[7]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.

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.

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

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

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

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).[8]

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

[8] The potential for spatial correlation is why we leave out the CBSA, not only the county itself. As Figure B shows, 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). Further, as Table B2 shows, our results are robust to controlling for a spatial lag.

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 conduct provide evidence to allay these concerns. For pre-trends we analyze the association between the instruments and the lagged outcomes. For differential trends, we interact key pre-period characteristics with period indicators. We construct a simulated counterfactual instrument, the correction proposed by Borusyak and Hull (2023) to deal with possible non-random exposure.[9] Controlling by this simulated variable helps to examine if 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 around 95 municipalities. In Figure B2 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.

[9]We explain further in subsection 5 and B.1.

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 that 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]

4 Main Results

In this section, we examine the impact of recent unauthorized Mexican migration on voting behavior and policy delivery. The main finding is that flows of recent unauthorized migrants shift a county's vote share toward the right, potentially deciding races, and prompt local spending consistent with the fiscal conservatism and the law-and-order policies of the

Republican party.

4.1 Voting Behavior

Table 2 displays the estimated impacts of unauthorized migrant arrivals on Republican party vote share in House and presidential elections. We study midterm and presidential years separately because midterm elections coincide with the end of our periods, whereas presidential elections occur two years later. Besides different timing, presidential elections are partially driven by presidential candidates and 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 pp, equivalent to going from the top of the first to the top of the third quartile. While 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 OLS estimates for a baseline comparison. Panel B displays second-stage estimates from our preferred specification, the leave-one-CBSA-out shift-share (LOO) instrument. Panel C displays second-stage estimates from the push factors instrument. We use reduced form estimates when we analyze robustness and heterogeneity.

[Table 2]

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 (columns 1 and 2). A one percentage point increase in unauthorized migrants is associated with 6.51 point increase in the share of votes that go to Republicans. Presidential year relationships are smaller in magnitude, both for the House of Representatives (columns 3 and 4) and for the President (columns 5 and 6). Our weighted and unweighted estimates seldom differ statistically. We focus on population-weighted estimates throughout the paper 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.

Panels B and C indicate that the inflows of newcomers are 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) and a 0.52 point increase for the Republican presidential candidate (Panel B, column 5, std coeff: 0.17). In turn, a 0.1 pp increase, using the push factor instrument, causes a 0.83 point boost in Republican vote in House midterm elections (Panel C, Column 1, std coeff: 0.24). Generally, estimates from the instruments are statistically indistinguishable from each other.

These effects are consistent with electoral reactions in other settings (Barone et al., 2016; Edo et al., 2019; Tabellini, 2020). Mayda et al. (2022a) estimate that a one percentage point increase in the share of "low-skilled" migration raises the vote share for the Republican party, in all federal elections between 1990 and 2016, by 4.5 percentage points. Our estimates are larger than theirs likely because the populations we study are different. As explained below, the arrival of unauthorized migrants may have specific job market and ideological impacts, due to, for example, lower reservation wages and negative media coverage. Also, we analyze changes in flows, not stocks. Further, we focus on a historical period in which the GOP 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. Given available data, we unpack some of these factors indirectly. We examine the effect of inflows of newcomers on absolute

GOP votes, registered Republicans (for a subset of states), and turnout of White voters. We find evidence consistent with changes in the composition of the electorate, but not partisan preference or Republican turnout. As Table 3 indicates, although comparatively smaller than the boost in GOP vote share, we observe an increase in GOP votes (columns 1 and 2). Republican registration is unchanged (column 3), suggesting partisan realignment does not explain our results. The positive effect in the ratio of votes to registration (column 4) indicates that the GOP accumulates more votes, likely because of independents, than it accumulates registered voters. Finally, columns 5 and 7 suggest that the composition of the electorate changes too. Turnout among White voters drops in absolute terms but increases in proportion to the total white population. By contrast, Hispanic turnout decreases (column 8). The boost in the vote share of the GOP is likely explained by a combination of more support among independents and out-migration of white, otherwise non-Republican voters.

Table 3

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 party 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 suggest 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 are decisive. Between 2010 and 2018, 107 of the midterm races we study at the county-level were decided by less than 0.91 percentage points. 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 was comprised of voters from 14 counties, 12 of which were totally inside the district.[10] The combined inflow of newcomers in this group of 12 counties decreased by 0.03 percentage points relative to 2010. The predicted effect of the drop is around 0.31 percentage points (-0.036×8.5) or roughly 765 fewer votes for Republicans. The Democratic candidate, Gwen Graham, won that race by 0.8 percentage points, around 2,200 votes. Our estimates suggest that a third of the difference can be explained by a reduction in the arrival of newcomers.

4.2 Policy Change

To explore whether the observed effects on federal elections reflect local conservative response, we study the impact of flows of newcomers 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, in line with 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 to changes in local policy.[11]

Table 4 presents the effects of inflows of newcomers on absolute and relative expenditures. The former allows us to compare actual policy provision, but the estimates could be biased towards zero due to budgeting heterogeneity.[12] The latter allows us to compare changes in relative preferences taking the fiscal heterogeneity as given.

[Table 4]

The results present a pattern similar to that of the impact on voting. The OLS estimates

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

[11] 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).

[12] We exploit within period county variation, so this measurement error is probably classical.

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) goes down 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 capita 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 capita 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 intergovernmental transfers. In Table B1 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.

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 newcomers prompts a 0.5% reduction in spending per child on public education (column 3, std coeff: -0.10), and increases in police (0.4%) and judicial (1.6%) expenditures per capita. 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 platform of the GOP.

Columns 6, 7, and 8 of Table 4 display the results for relative spending. Police spending increases by 0.04 percentage points (column 7, std coeff: 0.15), and judicial expenditure increases by 0.02 percentage points (column 8, std coeff: 0.21). That is, local governments explicitly decide to strengthen law-and-order.[13] To put these effects in context, weighted by population, on average, between 2002 and 2017 relative police and judicial spending changed by 0.69 and -0.1 pp.

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 on law-and-order. Voters who shift toward Republican candidates for Congress probably also shift toward Republican 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 one percentage point 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.[14] They do not find significant effects on education and law-and-order. Again, this difference may be due to differences in the design–populations of study and type of independent variable.

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 administration of justice could indicate an increased perception of threat.

[13] We analyze eight public expenditure variables, so we carry out a Holm correction for multiple hypothesis testing. With a 0.05 significance level, we can reject the null hypothesis of four tests with a p-value of 0.009, less than Holm's benchmark of 0.01: police share, education, direct expenditures, and judicial share. In the remaining analysis, we consider these effects statistically significant.

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

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).

We examine this hypothesis in Table B2. First, we test for pre-trends by regressing the instrument on pre-period outcomes—lagged 12 years. We only observe a statistically significant association with the vote share of the GOP presidential candidate. This is not surprising as presidential preferences are highly persistent.

Second, to explore if the effects are being driven by differential trends rather than by our instrument, we control for the interaction of key pre-period characteristics with time/period indicators. 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). To control for long-term social and economic trends, we use the rate of high school completion among adults in 2000, and exposure to the China shock in 2006. 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 largely significant.

Our results remain significant to 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.[15] They are also robust to excluding outliers, not weighting by using predicted population and exploiting variation only within groups of politically similar counties (in the state) following Bazzi et al. (2023). Finally, in Table B4, 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.

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 shed light on 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 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 GDP per capita, median household income, unemployment, and poverty. In subsection 6.3, we examine demographic changes and universalist values to estimate if, as the electoral results suggest, migrants affect the demographic composition of the county, via residential sorting and internal migration, as well as the cultural preferences of residents. For demographic variables, we examine the effect on out-migration and adult population, both total and Hispanic, Black, and White separately. For values, we study the relative importance of universalist versus communal values. This variable, obtained from Enke (2020) replication files, aims

[15] 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 ($\rho \approx 0.86$).

to capture people's beliefs about the relative moral emphasis on the in-group as compared to all people. Those inclined toward universalism emphasize equal treatment, regardless of relationship, whereas those inclined toward communalism emphasize loyalty to members within their group. All the variables are summarized in Table A4.[16]

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 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, manufacturing and agriculture (which includes forestry, fishing, and hunting). The employment variables are measured in (log) per working-age population (ages 15–64) and the wages correspond to 2010 dollars.[17] The QCEW estimates are based on states' unemployment insurance data and surveys from employers. They explicitly exclude "*self-employed workers, most agricultural workers on small farms...some domestic workers*". Thus, this data captures largely formal employment.

Columns 1-5 of Table 5 present the results on formal employment. Inflows of unautho-

[16]There are two additional mechanisms we cannot explore due to data limitations. 1) The threat hypothesis; namely, that established residents are hostile to migrants due to a perceived threat (Hainmueller and Hopkins, 2014). We can, however, examine whether the threat itself has changed. In Appendix C.2 we show that in response to the arrival of newcomers, property crimes, violent crimes, and total crimes do not increase (Kaplan, 2020). While we cannot evaluate the police and prosecutors' response to migrants more specifically, nor politicians' willingness to use (mis)perceptions to gain office, there is no evidence that migrants cause more crime, and that more people are being arrested because of the presence of migrants. 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).

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

rized Mexican migrants have an imprecisely estimated zero effect on total employment. On average, employment per working-age people is unmoved by the arrival of migrants. This null effect, however, masks heterogeneity, as some sectors observe a decrease and others an increase. A 0.1 pp increase in the inflow of unauthorized migrants reduces 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 boost 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 agricultural jobs, but probably due the exclusion of most agricultural workers from the dataset, the estimates are unstable.

[Table 5]

These findings indicate a reallocation of jobs, away from construction and H&L to manufacturing. Construction and H&L 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. Hence, 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 H&L.

Columns 6–10 of Table 5 present the results on wages. We observe a small negative, yet imprecisely estimated effect on total wages: the average worker, either in the formal or informal sector, does not earn less as a result of the arrival of unauthorized newcomers. We do, nevertheless, observe an impact on construction and agriculture. As Panel B indicates, 0.1 pp increase in the flow of migrants drives construction wages down by \$2.4 weekly (column 7, std coeff: -0.06) and agricultural wages down by \$4.5 weekly (column 10, std coeff: -0.14). Agriculture is the industry that employs the highest number of unauthorized Mexican migrants as a share of total employment, so the steep decline in wages does reflect an increase in the supply of laborers (Passel and Cohn, 2015; Svajlenka, 2020).

Economic theory would suggest that wages and employment should move in opposite directions. Our interpretation, which reconciles the simultaneous decline in wages and formal employment in construction and H&L, is that inflows of unauthorized migrants increase the total labor supply but decrease the formal labor supply.

To further understand which demographic group might be losing or switching jobs, Table C7 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 PUMA, a geography covering several counties. Hence, we analyze the effect of inflows of newcomers at the level of a period-locality (county if available in IPUMS or PUMA if not available in IPUMS). Unlike the QCEW, ACS 5 records employed people rather than employment and, in principle, includes both formal and informal jobs.[18] The main takeaway from this exercise is that only populations who are most likely to compete with newcomers in specific sectors are negative affected: we observe fewer Hispanic people and White people with just high school employed in H&L. 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 and Hispanic individuals without a high school education.

These findings are 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 comparable to the

[18] While losing precision due to smaller sample sizes and different dependent variables (employed people instead of formal employment), Table C6 shows the effects are consistent: less people employed in construction and H&L and more on manufacturing. The first stage for this sample is in Table C5.

drop in manufacturing employment due to the China shock (Autor et al., 2013).[19] This suggests that there are some economic losers in counties that receive unauthorized migrant flows, and at first glance, these individuals are not compensated accordingly. Those with economic losses, especially White voters in the H&L sector, may account for some voters favoring conservative politicians. However, job loss likely explains only a part of the political reaction. If economic grievance was the main cause of the reaction, we would expect a higher turnout of Hispanic voters, a group especially affected, not just of White voters.

6.2 General Economic Effects

We now turn to examining if migration inflows affect general equilibrium indicators, like income levels. We then extend this analysis by looking at the income distribution. 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 6 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 newcomers, estimated with either instrument, raises the number of impoverished people by 0.7% (Panels B and C, column 1, std coeff: 0.02).

SAIPE's poverty estimates could include newcomers themselves, as they are calculated with data from ACS, tax returns, the previous Census, and SNAP (Supplemental Nutrition Assistance Program) beneficiaries.

Table 6

To approximate the effect of migration inflows on poverty among US citizens, the outcome of column 2 of Table 6 is the natural logarithm number of SNAP recipients—also obtained from SAIPE. SNAP is among the most responsive federal entitlement programs, and it

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

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.[20] A disadvantage is that participation is voluntary, so it does not fully capture poverty among citizens. A 0.1 pp rise in the inflow of newcomers increases the number of SNAP recipients in the county by 0.3% (Panel B, Column 2, std coeff: 0.01) with the LOO instrument—the effect is not statistically significant with the push factor instrument. This result suggests that the marginal increase in poverty is not all explained by newcomers themselves.

Column 3 of Table 6 estimates the impact on the county-level poverty rate. A 0.1 pp increase in the inflow of newcomers, estimated with either instrument, raises the share of impoverished people 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.[21] One potential explanation for this divergence is residential sorting and demographic change. As the next section shows, relatively wealthier residents leave their counties of residence in response to the arrival of newcomers. In line with the analysis of job loss, Table C8 explores changes in poverty by demographic groups using the ACS 5. The results are consistent. The only group with statistically significant impacts on poverty is Hispanic Americans with just high school education, one of the direct competitors of newcomers. The drop in turnout for this group observed in Table 3, however, suggests that economic grievance is not the only driver of the political reaction.

Column 4 of Table 6 displays the impact on the unemployment rate. In contrast to employment, the unemployment rate captures formal and informal work as 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

[20]USDA Food and Nutrition Service. SNAP: Guidance on Non-Citizen Eligibility

[21]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 poverty rate is 0.10.

insignificant effect on unemployment.

Columns 5 and 6 of Table 6 present the impact on (log) county GDP per capita and median household income. A 0.1 pp rise in the inflow of newcomers, estimated with the push factor instrument, decreases GDP per capita by 0.4% (Panel C, column 5, std coeff: 0.05) and median household income by 0.3% (Panel C, column 6, std coeff: 0.06). The effects are smaller (0.3% and 0.2% respectively), but imprecisely 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 GDP per capita and median household income partially reflects a reduction in formal economic activity, as opposed to a reduction in economic activity in general. (2) As the effects on relative vs. absolute poverty indicate, the reduction in formal economic activity is caused, at least partially, by the emigration of relatively wealthier residents.

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 switch residence. The resulting conservative response would be the result of the emigration of relatively left-leaning voters and/or a change in preferences for redistribution.

We study population changes using US Census data. We examine whether the adult (log) total population, White population, Hispanic population, and Black population the year after the end of our periods. Moreover, the census systematizes data from the ACS 5 on county-to-county demographic flows. Thus, we construct out-migration rates, using the data from 2007–11, 2011–15, and 2015–19. 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 for a subset of the counties we study.

Columns 1 to 4 of Table 7 display the effects on the adult population. We observe a decline in the total adult population, driven by White population, and an increase in both Black and Hispanic populations. A 0.1 pp rise in the flow 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 contrast, Hispanic and Black populations increase by 0.9% and 0.5%, respectively (columns 2 and 4, std coeff: 0.02 and 0.01).[22]

[Table 7]

Column 5 shows that 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). The arrival of newcomers generates residential sorting and demographic change, decreasing the population of the White majority and increasing the population of the two largest minorities. These results provide further evidence that relatively wealthier residents move out of their communities, bringing down median income and GDP per capita.

[22] Since ethnic and racial categories change over time and are not mutually exclusive, the effects on the different groups do not sum to the effect on total adult population.

The final column in Table 7 explores the impact on universalist (as compared to communal) values. We study individuals' universalist values to capture preferences for redistribution and openness to the out-group. Universalist values imply that one is concerned equally with the welfare of all individuals. By contrast, people with more communal values assign a greater 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 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 migrants trigger anti-out-group bias and preferences for less redistribution. Although this evidence is based on a smaller subset of counties, the impact is large. The change toward more communal values is consistent with theories that hinge on out-group bias. Ethnic heterogeneity breaks down trust, makes coordination more difficult, and reduces people's interest in universal redistribution (Alesina et al., 1999).[23]

To explore the relationship between economic shocks and social impacts, we estimate the differential effect of newcomers on universalism and out-migration according to whether the county had an above-median change in the poverty rate during our period of analysis (2011–2019).[24] If the conservative response is mainly driven by economic factors, counties with relatively better economic conditions should observe more temperate effects on out-group bias and residential sorting. Economic grievance seems to be connected to out-group bias, but not to residential sorting. As Table C4 shows, counties with above-median changes in poverty rates have stronger effects on universalist values, yet more muted effects on out-migration. Residents of counties where economic conditions declined appear to display more out-group bias in response to newcomers, but not necessarily to move out. Residential sorting

[23]Table C1 estimates the impact of newcomers on ideology and partisan affiliation using the Gallup Daily version aggregated at the Metropolitan Statistical Area (MSA) level and the Cooperative Election Study (CES). There is imprecise evidence that people become more conservative or identify more with the GOP. Since the variation comes from fewer observations, we have less statistical power.

[24]We average the two changes in the poverty rate (2011-15 and 2015-19) and sort counties based on whether they have an above or below median value. In Table C3, we divide counties by an index of economic grievance, rather than just by the change in the poverty rate. The results are similar.

appears to be driven by other factors, like compositional amenities or preference for ethnic homogeneity.

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 be underestimating 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.[25]

6.5 Robustness for mechanisms

Table B5 conducts robustness checks on the mechanisms. Among others, the results are largely robust to controlling for differential trends, a simulated instrument, a lagged instrument, and proxies of the stock of Mexican-born and Hispanic people.

[25] 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 5. Poverty effects ($1/3 \approx 0.07 \times 0.42/0.085$) are 7% per column 1 of Table 6. 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 7.

7 Taxation and the Social Safety Net

We have identified that the inflow of unauthorized Mexican migrants increases the vote share of the Republican party in federal elections, reduces total public expenditure and changes expenditure composition away from education toward policing and the judiciary. 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 if the impacts vary 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. Our hypothesis is that counties with more progressive tax structures or a larger safety net are better able to mitigate economic shocks and compensate those who lose economically; hence, the impact of migration inflows should be lower. We find suggestive evidence that this is the case.

We categorize counties based on four (pre-period) proxies of the system's ability to compensate economic losers: 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 poor population covered by the federal program Temporary Assistance to Needy Families (Shrivastava and Thompson, 2021), [26] and 4) the minimum wage, which, although not an explicit measure of redistribution, is associated with higher incomes at the bottom of the distribution and lower poverty rates (Cengiz et al., 2019; Dustmann et al., 2021). Proxies 2-4 vary at the state-level.

Figure 2 displays the differential effects of unauthorized Mexican migration on the main political and socioeconomic variables for above and below 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 socioeco-

[26] Like Medicaid, states have the ability to set the operational rules (i.e. generosity of the program).

nomic impacts. Although the differences are not always statistically significant, the direction is consistent and the magnitude is meaningful.

[Figure 2]

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 boost for the GOP in midterm elections is reduced by between half and a third in these counties. The formal job losses in construction, increases in poverty, and reduction in universalism are either partially or fully reversed. In contrast, these counties do not see smaller effects on out-migration. In fact, the impact is exacerbated. This exception supports the hypothesis that residential sorting does not respond to economic factors, but rather to other motivations like compositional amenities. Section D.1 shows the results in regression format. Otherwise, the political and socioeconomic impacts are not systematically heterogeneous.[27]

8 Conclusion

We estimate the impact of recent unauthorized Mexican migration on the political, economic, and social conditions of US counties using two different shift-share strategies. In response to the arrival of newcomers, county vote share for the Republican party increase in House and presidential elections. Local government agencies reduce total expenditure, divest in education, and increase relative spending in policing and the administration of justice. We contend that three socioeconomic channels partially explain this conservative reaction. (1) This migration creates formal job loss in "migrant-intensive" sectors, especially among likely competing demographic groups. Despite creating formal job gain in manufacturing and, thus,

[27] To explore whether the demographic composition of the counties matters, we test if there is a differential impact in counties with higher shares of Hispanic, White, or Black populations. To explore if 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.

no aggregate decline in overall employment levels, the arrival of newcomers causes an increase in the number of poor people—probably resulting from transient job loss. However, there is no evidence that people most economically affected are driving the conservative response. (2) Established residents, arguably motivated in part by economic grievance, display more out-group bias. (3) Newcomers generate population loss, especially among White residents, explained by out-migration. This residential sorting seems to be unrelated to economic factors and is in line with a re-composition of the electorate.

Both our 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, removing outliers, and exploiting changes within groups of politically similar counties (as opposed to within the whole state). We do not find a statistically significant association between migration and lagged outcomes, supporting the parallel trends assumption.

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, scholars had yet to estimate the impacts of unauthorized migrants, arguably the most politically salient group. This effort is first to study the political impacts of unauthorized migration throughout a whole country. Study this group directly is important because unauthorized migrants have distinctive labor market and political characteristics. For example, they cannot vote, but 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 study and link both. 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 registered with the party.

We help to disentangle the roles and interactions of economic and cultural factors in explaining the right-wing reaction to immigration (Alesina and Tabellini, 2024). Our findings are similar to forces described in the literature on 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 causing limited formal job loss only and a modest increase in the number of poor people. This grievance explains a decrease in universalism values but 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 answer to Alesina and Tabellini (2024)'s suggestion of exploring the role of migration on moral values.

A county's taxation and redistribution 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 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 the back-of-the-envelope calculations, we observe the effects of migration on the political outcomes are larger than the effects on the proposed mechanisms. The conservative reaction to unauthorized Mexican newcomers cannot be fully explained by the channels we have explored. Future research should document the role of additional channels, like (mis)perceptions and politicians.

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Tables

Table 1: First stage

	(1) LOO, county out	(2) LOO, CBSA out	(3) LOO, state out	(4) Push factors
Newcomers, percent	1.116***	1.160***	1.315***	1.338***
population	(0.038)	(0.040)	(0.067)	(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 country itself out. Column 2 displays results for a LOO shift-share regressor that leaves the CBSA out. Column 3 displays results for a LOO shift-share regressor that leaves the state out. Column 4 displays results for a shift-share regressor that predicts 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 estimations control for county and state-year fixed effects and are weighted by predicted population. Stars indicate *p<0.1,**p<0.05,***p<0.01

Table 2: Effects of arrival of unauthorized Mexican migrants on GOP vote shares (2010-20)

	House, Midterm		House, Pres year		President	
	(1)	(2)	(3)	(4)	(5)	(6)
	Weight	Un-weight	Weight	Un-weight	Weight	Un-weight
	A. OLS					
Newcomers, pct. pop.	6.51***	5.25***	2.82***	3.27***	3.48***	3.37***
	(0.87)	(0.70)	(1.06)	(0.65)	(0.62)	(0.43)
	B. 2SLS LOO					
Newcomers, pct. pop.	8.50***	9.21***	3.49***	4.94***	4.81***	5.85***
	(1.03)	(1.05)	(1.21)	(0.94)	(0.69)	(0.68)
Std. Coefficient	0.26	0.23	0.10	0.12	0.17	0.17
$\hat{\beta} * P(75) - P(25)$	0.91	0.99	0.37	0.53	0.52	0.63
	C. 2SLS Push Factors					
Newcomers, pct. pop.	7.78***	8.81***	3.29***	5.08***	5.10***	6.36***
	(1.14)	(1.19)	(1.27)	(1.03)	(0.70)	(0.72)
Std Coefficient	0.24	0.22	0.10	0.12	0.18	0.18
$\hat{\beta} * P(75) - P(25)$	0.83	0.94	0.35	0.54	0.55	0.68
Observations	7995	7995	8015	8015	8019	8019
Dep. Var., Mean	48.16	61.70	47.24	63.79	46.05	61.70
Dep. Var., Sd	19.44	18.14	19.92	19.14	16.51	15.52

Dependent variables are share of Republican vote. Source: Dave Leip's United States Election Data. Newcomers are the number of new consular IDs per county per 4–year period as a proportion of predicted population as described in Section 3. Panel A displays the results for OLS estimator. Panel B displays results for a LOO shift-share regressor that leaves the CBSA out, our preferred specification. Panel C displays results for a shift-share regressor that predicts yearly migration by municipality using push factors like homicide rates, economic activity and variation in temperature. Standard errors clustered at the CBSA level. All estimations control for county and state-year fixed effects. Stars indicate *p<0.1,**p<0.05,***p<0.01

Table 3: Effect of arrival of unauthorized Mexican migrants on GOP vote and turnout (midterm elections, 2010-18)

	Log			Ratio	Log		Share	
	(1) House votes	(2) House votes	(3) Registered	(4) Votes/ Regist voters	(5) White votes	(6) Hispanic votes	(7) White votes	(8) Hispanic votes
				A. OLS				
Newcomers, pct. pop.	0.15***	0.13***	0.01	0.09**	-0.01	-0.08***	0.01***	-0.00
	(0.03)	(0.04)	(0.02)	(0.04)	(0.01)	(0.03)	(0.00)	(0.00)
				B. 2SLS LOO				
Newcomers, pct. pop.	0.20***	0.15**	0.01	0.11**	-0.03**	-0.14***	0.02***	-0.01**
	(0.04)	(0.07)	(0.02)	(0.05)	(0.01)	(0.02)	(0.00)	(0.00)
Std. Coefficient	0.08	0.06	0.01	0.23	-0.01	-0.03	0.09	-0.04
$\hat{\beta} * P(75) - P(25)$	0.021	0.016	0.001	0.012	-0.003	-0.015	0.002	-0.001
				C. 2SLS Push Factors				
Newcomers, pct. pop.	0.21***	0.19***	0.03*	0.10**	-0.03**	-0.12***	0.01***	-0.01***
	(0.04)	(0.07)	(0.02)	(0.04)	(0.01)	(0.03)	(0.00)	(0.00)
Std. Coefficient	0.09	0.08	0.01	0.21	-0.01	-0.03	0.08	-0.05
$\hat{\beta} * P(75) - P(25)$	0.023	0.020	0.004	0.011	-0.004	-0.013	0.002	-0.001
Observations	7868	3321	3367	3367	8019	8019	8019	8019
Dep. Var., Mean	10.82	11.03	11.34	0.76	11.01	8.43	0.29	0.23
Dep. Var., Sd	1.39	1.33	1.36	0.29	1.42	2.46	0.11	0.14
Ind. Var., Mean	0.46	0.47	0.47	0.47	0.46	0.46	0.46	0.46
Ind. Var., Sd	0.59	0.58	0.58	0.58	0.59	0.59	0.59	0.59
Inst. Loo, Mean	0.41	0.43	0.43	0.43	0.40	0.40	0.40	0.40
Inst. Loo, Sd	0.55	0.57	0.57	0.57	0.55	0.55	0.55	0.55

Dependent variable in columns 1 and 2 are the log of Republican votes in House midterm elections. The sample in column 2 is the subset of counties for which the Dave Leip's United States Election Data has data on active registered Republicans. Dependent variable in column 3 is the log of active registered Republicans in a county in a midterm election. Dependent variable in column 4 is the ratio of Republican votes to active registered Republicans in a midterm election. Dependent variables in column 5 and 6 are the log of European/White and Hispanic voters in a midterm election, respectively. Dependent variable in column 7 and 8 is the votes of European/White and Hispanic voters as share of their respective populations in the county during the election year. Source: Dave Leip's United States Election Data, L2 Voter Data 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. Instruments are as described in Section 3. All regressions have period and county 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

Table 4: Public spending effects of arrival of unauthorized Mexican migrants (2012 and 2017)

		Expend (log pc 2010 USD)				Share of Dir Expend		
	(1) Revenue	(2) Direct exp	(3) Educ	(4) Police	(5) Judicial	(6) Educ	(7) Police	(8) Judicial
			A. OLS					
Newcomers, pct. pop.	-0.02*	-0.02*	-0.03**	0.02	0.08	0.32	0.20	0.12
	(0.01)	(0.01)	(0.01)	(0.02)	(0.06)	(0.42)	(0.12)	(0.09)
			B. 2SLS LOO					
Newcomers, pct. pop.	-0.03**	-0.04***	-0.05***	0.04**	0.15**	0.22	0.41***	0.26***
	(0.01)	(0.02)	(0.02)	(0.02)	(0.07)	(0.61)	(0.14)	(0.10)
Std. Coefficient	-0.05	-0.07	-0.10	0.06	0.12	0.01	0.15	0.21
$\hat{\beta} * P(75) - P(25)$	-0.003	-0.004	-0.005	0.004	0.016	0.024	0.044	0.028
			C. 2SLS Push Factors					
Newcomers, pct. pop.	-0.03**	-0.04**	-0.03**	0.04*	0.11	0.92	0.38**	0.21**
	(0.01)	(0.02)	(0.01)	(0.02)	(0.07)	(0.56)	(0.16)	(0.10)
Std. Coefficient	-0.05	-0.07	-0.07	0.05	0.08	0.05	0.14	0.17
$\hat{\beta} * P(75) - P(25)$	-0.003	-0.005	-0.004	0.004	0.011	0.099	0.041	0.022
Observations	5338	5338	5328	5334	5266	5338	5338	5338
Dep. Var., Mean	1.57	1.53	1.96	-1.44	-2.95	40.74	5.45	1.40
Dep. Var., Sd	0.38	0.38	0.34	0.49	0.87	11.85	1.86	0.85
Ind. Var., Mean	0.55	0.55	0.55	0.55	0.55	0.55	0.55	0.55
Ind. Var., Sd	0.68	0.68	0.68	0.68	0.68	0.68	0.68	0.68
Inst. Loo, Mean	0.49	0.49	0.49	0.49	0.49	0.49	0.49	0.49
Inst. Loo, Sd	0.63	0.63	0.63	0.63	0.63	0.63	0.63	0.63

Dependent variables in columns 1–5 are in log 2010 dollars per capita, except education (column 3) which is per child (population under 19). Dependent variables in columns 6–8 are shares of total direct expenditures. Revenue includes taxes, intergovernmental revenue, current charges, and miscellaneous general revenue. Direct Expenditure includes spending on public education, policing, health, as well as other categories as described in section 3. Education expenditures include all public education expenditures of the county. Police expenditures include city police spending in a county as well as sheriff department spending and local incarceration at county jails. Judicial expenditure includes all county expenditures on the administration of justice including prosecutors, public defense, judges, court administration, and expenses related to the civil court system. Source: Annual Survey of State and Local Government Finances 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. Instruments are as described in Section 3. All regressions have period and county 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

Table 5: Effect of arrival of unauthorized Mexican migrants on employment and weekly wages (2010-19)

	Employment, (log per working age pop)					Weekly Wages (2010 USD)				
	(1) Total	(2) Constr	(3) Manufact	(4) H & L	(5) Agric	(6) Total	(7) Constr	(8) Manufact	(9) H & L	(10) Agric
A. OLS										
Newcomers, pct pop.	0.01	-0.03*	0.07***	-0.01**	0.03	3.88	-14.41	12.20	-4.79	-18.18
	(0.01)	(0.02)	(0.02)	(0.01)	(0.06)	(14.22)	(9.57)	(23.96)	(4.45)	(12.17)
B. 2SLS LOO										
Newcomers, pct pop.	-0.00	-0.05***	0.08***	-0.02**	-0.09	-1.79	-22.60*	15.98	-8.08	-42.81**
	(0.01)	(0.02)	(0.02)	(0.01)	(0.08)	(18.67)	(12.83)	(33.37)	(6.44)	(19.39)
Std. Coefficient	-0.00	-0.07	0.06	-0.03	-0.04	-0.00	-0.06	0.03	-0.04	-0.14
$\hat{\beta} * P(75) - P(25)$	-0.000	-0.006	0.008	-0.002	-0.010	-0.192	-2.422	1.712	-0.866	-4.587
C. 2SLS Push Factors										
Newcomers, pct pop.	-0.00	-0.04*	0.09***	-0.03***	-0.05	-13.61	-26.20***	-11.11	-9.90*	-41.51**
	(0.01)	(0.02)	(0.02)	(0.01)	(0.09)	(12.70)	(9.72)	(21.03)	(5.90)	(18.91)
Std. Coefficient	-0.00	-0.06	0.07	-0.04	-0.02	-0.03	-0.07	-0.02	-0.05	-0.13
$\hat{\beta} * P(75) - P(25)$	-0.000	-0.004	0.010	-0.003	-0.005	-1.459	-2.807	-1.190	-1.061	-4.448
Observations	8000	7385	7373	7903	4117	8003	7388	7376	7906	4117
Dep. Var., Mean	-0.67	-3.62	-3.07	-2.75	-6.56	872.40	992.86	1120.65	365.50	611.14
Dep. Var., Sd	0.34	0.44	0.73	0.43	1.49	260.60	223.12	354.91	119.55	199.50
Ind. Var., Mean	0.46	0.46	0.46	0.46	0.52	0.46	0.46	0.46	0.46	0.52
Ind. Var., Sd	0.59	0.59	0.59	0.59	0.63	0.59	0.59	0.59	0.59	0.63
Inst. Loo, Mean	0.40	0.41	0.41	0.40	0.47	0.40	0.41	0.41	0.40	0.47
Inst. Loo, Sd	0.55	0.55	0.55	0.55	0.59	0.55	0.55	0.55	0.55	0.59

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 annual average weekly wages in 2010 USD. Sources: Quarterly Census of Employment and Wages 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. 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

Table 6: Socioeconomic effects of arrival of unauthorized Mexican migrants (2010-19)

	People in poverty (log)		County Economy (rate)		County Economy (log)	
	(1) People in poverty	(2) SNAP recipients	(3) Poverty rate	(4) Unemployment rate	(5) GDP per capita	(6) Median household income
A. OLS						
Newcomers, pct. pop.	0.04***	0.01	1.17***	-0.03	-0.01	-0.02
	(0.01)	(0.02)	(0.22)	(0.15)	(0.01)	(0.01)
B. 2SLS LOO						
Newcomers, pct. pop.	0.07***	0.03*	1.59***	0.18	-0.03	-0.02*
	(0.01)	(0.02)	(0.26)	(0.18)	(0.02)	(0.01)
Std. Coefficient	0.02	0.01	0.18	0.04	-0.04	-0.05
$\hat{\beta} * P(75) - P(25)$	0.007	0.003	0.170	0.019	-0.003	-0.002
C. 2SLS Push Factors						
Newcomers, pct. pop.	0.067***	0.008	1.557***	0.056	-0.039**	-0.028**
	(0.013)	(0.019)	(0.269)	(0.189)	(0.015)	(0.012)
Std. Coefficient	0.02	0.00	0.17	0.01	-0.05	-0.06
$\hat{\beta} * P(75) - P(25)$	0.007	0.001	0.167	0.006	-0.004	-0.003
Observations	8019	8019	8019	8019	7857	8019
Dep. Var., Mean	10.89	10.81	14.36	6.02	3.88	10.88
Dep. Var., Sd	1.63	1.65	5.31	2.83	0.44	0.26
Ind. Var., Mean	0.46	0.46	0.46	0.46	0.47	0.46
Ind. Var., Sd	0.59	0.59	0.59	0.59	0.59	0.59
Inst., Mean	0.40	0.40	0.40	0.40	0.41	0.40
Inst., Sd	0.55	0.55	0.55	0.55	0.55	0.55

Dependent variables in columns 1–2 are the log of poor people and SNAP beneficiaries. Dependent variables in columns 3–4 are poverty rate and unemployment rate. Dependent variables in columns 5–6 are the log of GDP per capita (in 2012 USD) and median household income (in 2010 USD). Sources: SAIPE Program; US Department of Commerce: Bureau of Economic Analysis. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population as described in Section 3. Source: SRE. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects, and are weighted by predicted population. Stars indicate *p<0.1,**p<0.05,***p<0.01

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

	Adult Pop (log)				Per capita	Relative importance
	(1)	(2)	(3)	(4)	(5)	(6)
	Total	Hispanic	White	Black	Out migration	Universalist values
A. OLS						
Newcomers, pct. pop.	-0.03***	0.06***	-0.02***	0.02	1.26**	-0.09**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.58)	(0.04)
B. 2SLS LOO						
Newcomers, pct. pop.	-0.03***	0.08***	-0.03***	0.04**	1.59***	-0.13***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.61)	(0.04)
Std. Coefficient	-0.01	0.02	-0.01	0.01	0.06	-0.16
$\hat{\beta} * P(75) - P(25)$	-0.004	0.009	-0.003	0.005	0.170	-0.014
C. 2SLS Push Factors						
Newcomers, pct. pop.	-0.03***	0.09***	-0.02**	0.06***	1.34**	-0.16***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.60)	(0.04)
Std. Coefficient	-0.01	0.02	-0.01	0.01	0.05	-0.19
$\hat{\beta} * P(75) - P(25)$	-0.003	0.010	-0.002	0.006	0.144	-0.017
Observations	8019	8019	8019	8009	8017	5712
Dep. Var., Mean	12.62	10.22	12.36	10.01	55.16	0.15
Dep. Var., Sd	1.59	2.43	1.52	2.34	17.00	0.50
Ind. Var., Mean	0.46	0.46	0.46	0.46	0.46	0.47
Ind. Var., Sd	0.59	0.59	0.59	0.59	0.59	0.60
Inst. Loo, Mean	0.40	0.40	0.40	0.40	0.40	0.41
Inst. Loo, Sd	0.55	0.55	0.55	0.55	0.55	0.56

The dependent variable in columns 1-4 are the log of county adult: total population, Hispanic population, White population and Black population. The dependent variable in column 5 is out-migration, calculated as the number of out-migrants (in thousands) divided by county population. The dependent variable in column 6 is the average relative importance of universalist values, taken from Enke (2020). Sources: Enke, (2020); US Census Bureau: Population Division and Small Area; US Census Bureau: 2011, 2015, and 2019 ACS 5. 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

Figures

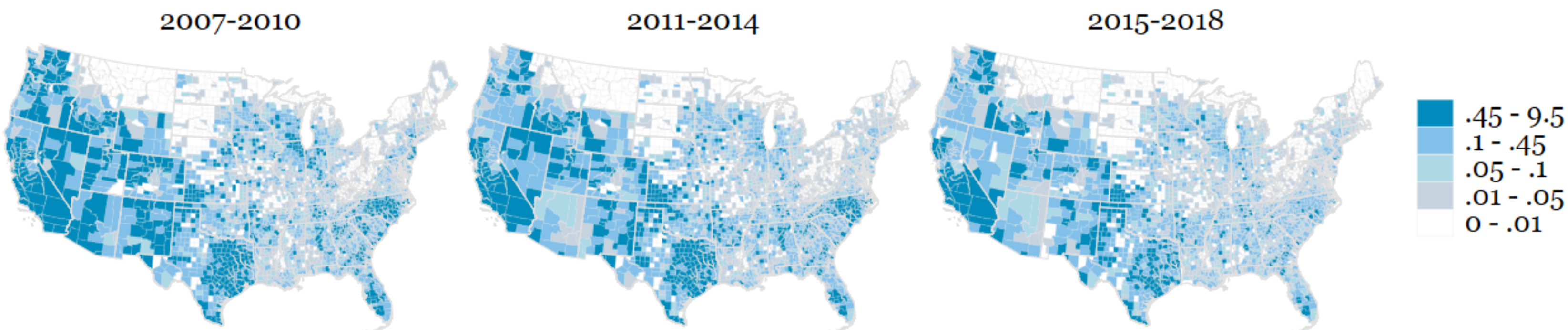

Figure 1: Observed recent unauthorized Mexican migrants as percent of county population.

Sources: SRE and US Census Bureau, Population Division

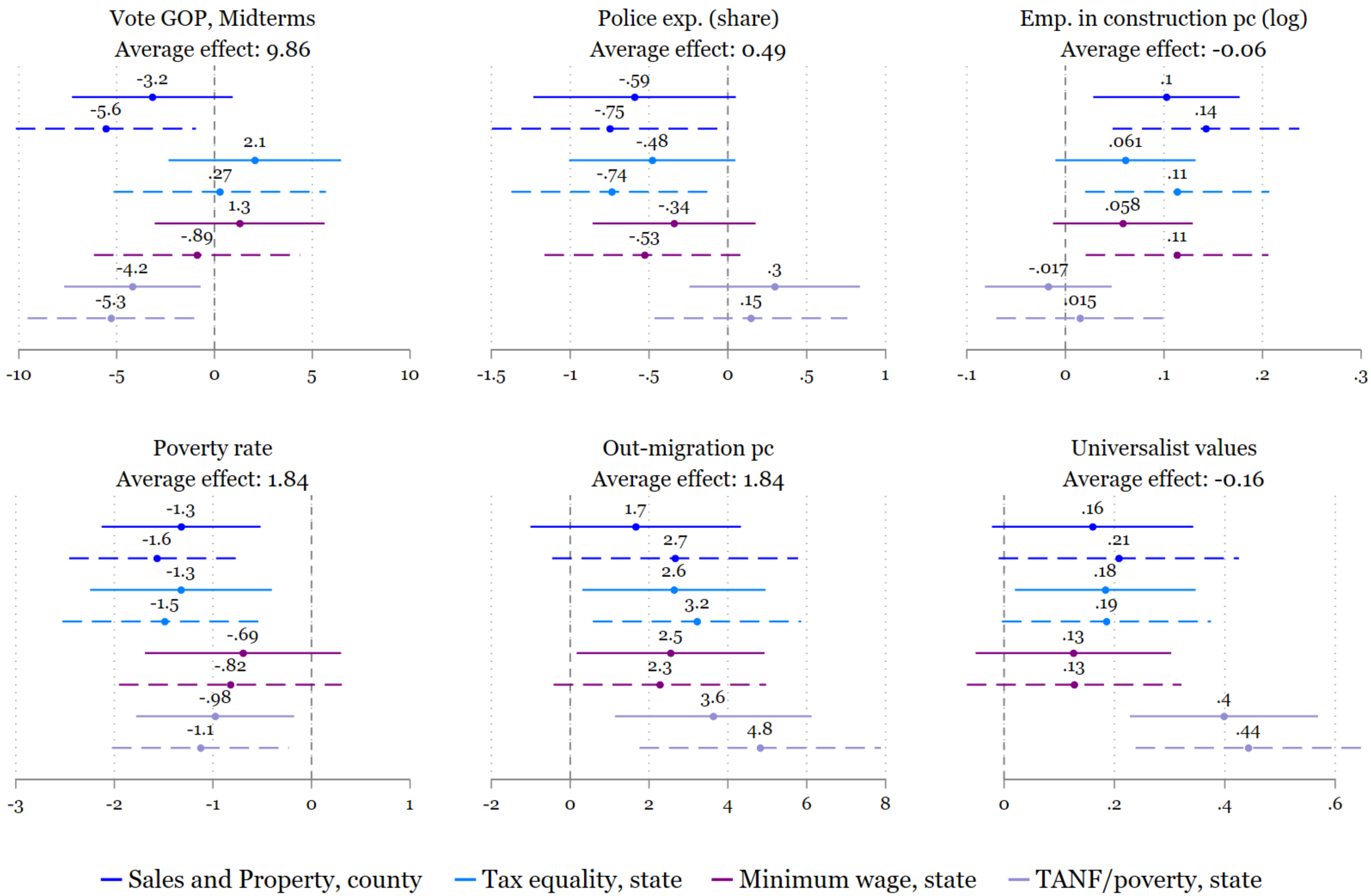

Figure 2: Heterogeneous effects by tax progressivity/strength of the safety net.

Displayed are the 90% 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 US 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).

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 (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).

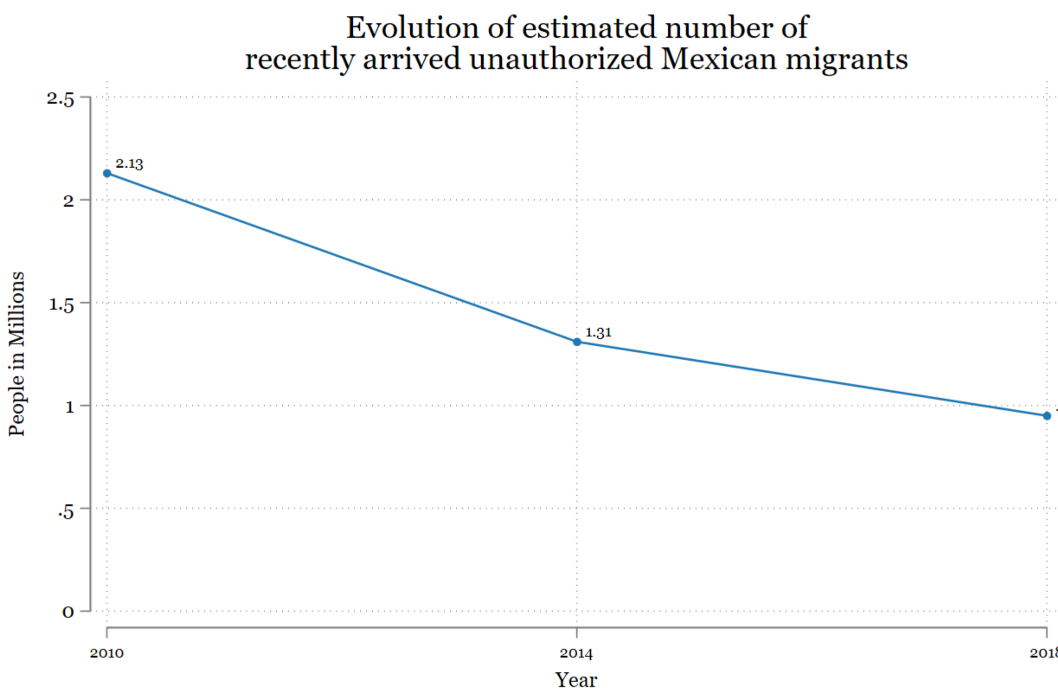

Figure A1: The figure plots the evolution of inflows of recent unauthorized migrants. Passel and Cohn (2018) estimates the stock of all unauthorized migrants to have decline from 11.4 million in 2010 to 10.7 million in 2016, and the subset who are Mexican from 6.2 million in 2010 to 5.4 million in 2016.

A.2 Comparison of likely unauthorized Mexican migrants' characteristics

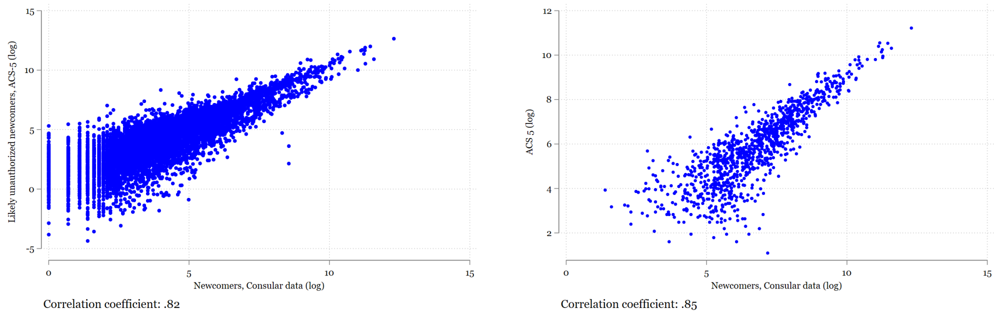

Figure A2: The left panel uses data for the 2,674 counties covered by ACS 5, in the Social Explorer, and our data. The right panel uses data for the 441 counties covered by ACS 5, in IPUMS, and our data. The association is weaker in areas with few migrants, probably due to low precision from the Social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

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 unauthorized Mexican migrants

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

The ACS 5 includes people born in Mexico, ages 16-64, without US citizenship and college degree, who arrived in the US less than five years before. The consular sample includes unique new observations per period (2010, 2014 and 2018) per CBSA. Sources: SRE and US Census Bureau: 2010, 2014, and 2018 ACS 5

A.3 Authorized vs. unauthorized migrants

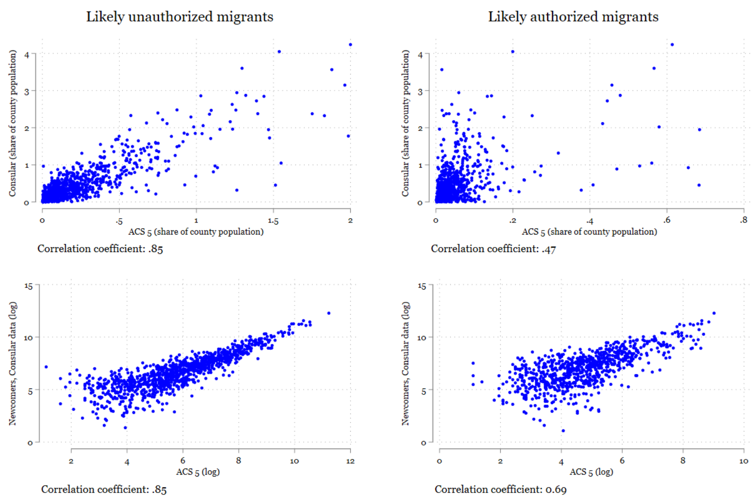

Figure A3: Comparison between estimates of authorized, unauthorized migrants from ACS 5 and consular data (2010, 2014 and 2018)

Table A2: Correlation between instrument and ACS 5 estimates of unauthorized and authorized Mexican migrants

	(1) Unauthorized	(2) Authorized
LOO Instrument	0.553***	0.028*
	(0.040)	(0.014)
Observations	974	692
F statistic	192	4
Mean of Dep. Var	0.265	0.056
Mean of Ind. Var	0.518	0.518

Both ACS 5 samples include people born in Mexico, ages 16 to 64 years, without US citizenship, who arrived in the US less than five years before. The likely unauthorized don't have a college degree. The likely authorized have a college degree. The consular sample includes unique new observations per period (2010, 14 and 18) 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 and S Census Bureau: 2010, 2014, and 2018 ACS 5

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 discourage migrants from applying for a consular ID. With the ID it would be obvious to local authorities that the cardholder is a foreign national, perhaps prompting authorities to submit fingerprints. We carry out six event study designs to explore whether consular IDs are responsive to the rollout.[28] The rollout was progressive, but not entirely random. Thus, the event studies differ by the period of analysis and the use of controls identified in previous studies (Alsan and Yang, 2019; East et al., 2022). Figure A4 displays the evolution of take-up rates across specifications. While sensitive, the results are consistently not statistically significant.

As of 2018, 12 states and DC allowed unauthorized migrants to get a diver's license, as compared with only 3 before 2012. [29] 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.

[28] 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.

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

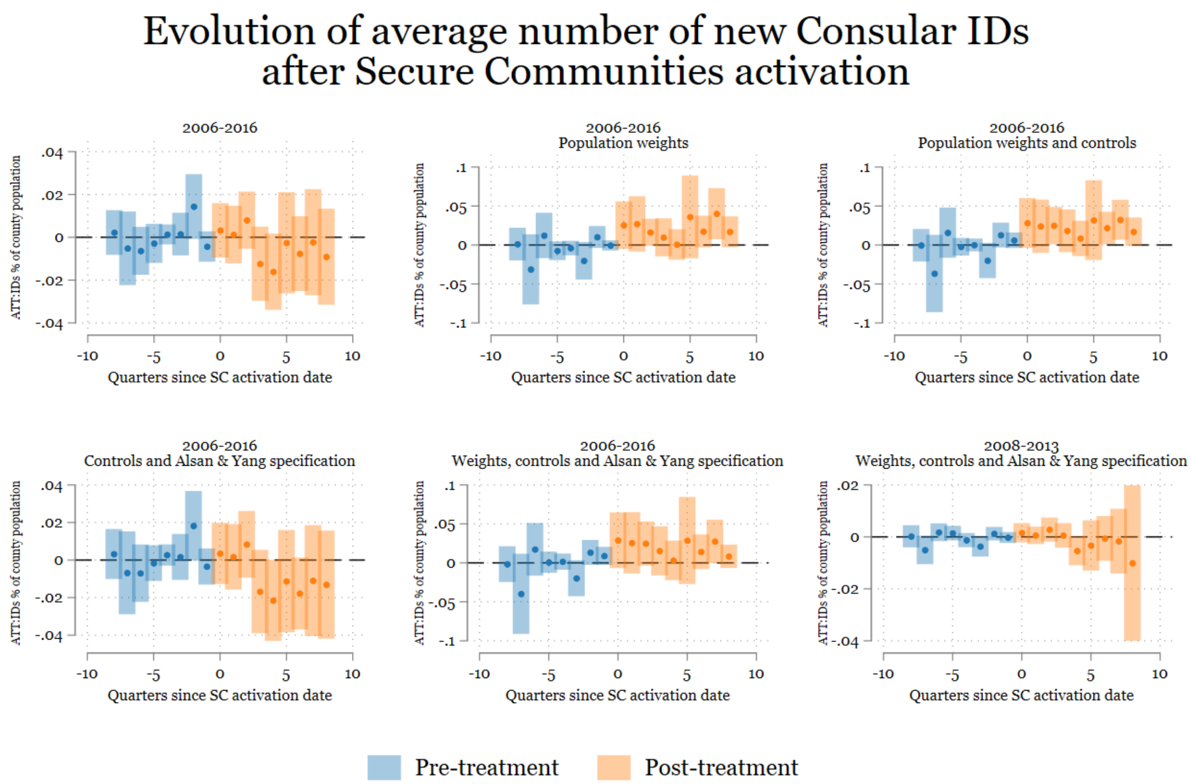

Figure A4: SC was implemented from October 2008 to September 2013, so analysis ranges from the first quarter in 2006 to the fourth quarter in 2016. Always-control counties (98 out of 2678) in the first estimation (no controls) are those that adopted the program lastly. The second estimation builds by using population weights. The third controls for distance to the Mexican border and share of Hispanic population. The fourth follows Alsan and Yang (2019) and also excludes border counties and the states of MA, NY, and IL. 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.

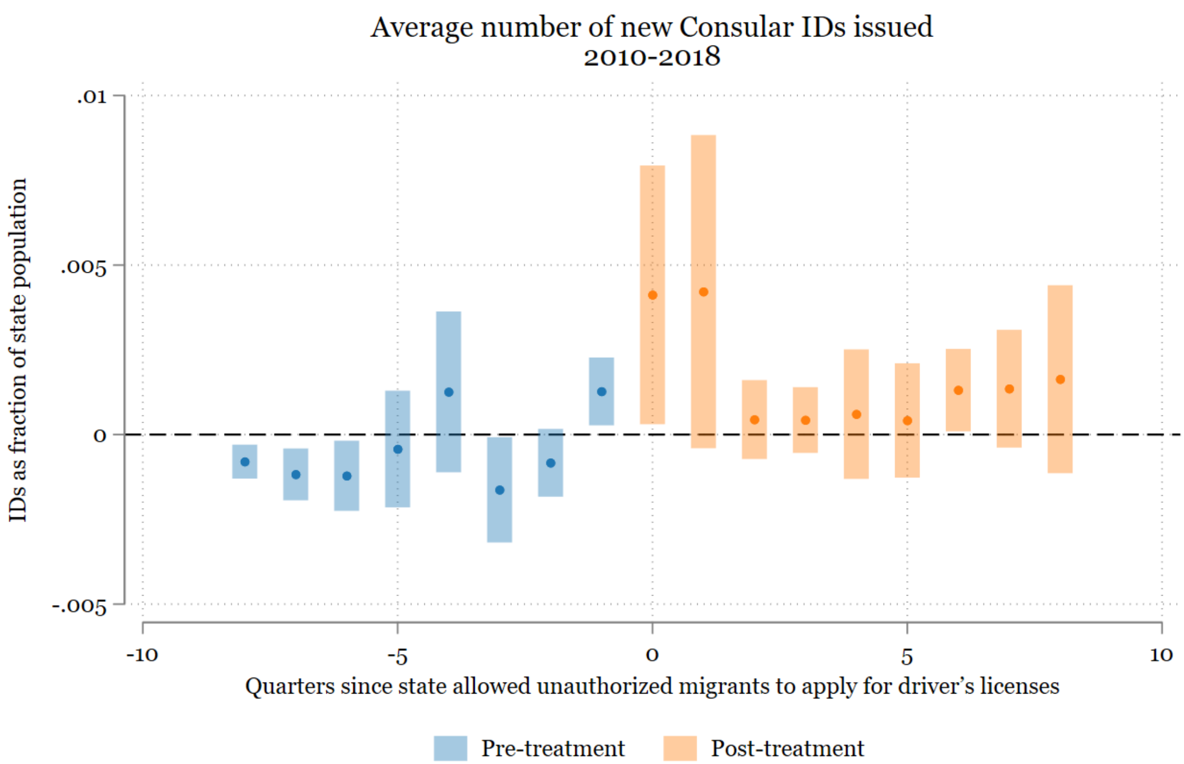

Figure A5: Change in demand of consular IDs after driver's license regulations

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	9.404	8022	2674	0
Instrument, leave county out	.421	.572	0	3.822	8022	2674	0
Instrument, leave CBSA out	.404	.551	0	3.822	8022	2674	0
Instrument, push factors	.44	.595	0	3.651	8022	2674	0
Population, '000	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
Vote share GOP President	45.97	16.59	4.09	95.43	8022	2673	2
Total revenue, pc log	1.57	.39	-.02	4.18	5340	2634	2.5
Total (dir exp), pc log	1.54	.39	-.07	4.17	5340	2670	2.5
Edu (dir exp), pc 0-19 log	1.97	.34	.47	4.69	5330	2665	2.5
Edu (dir exp), share	40.69	11.89	0	89.47	5340	2670	2.5
Police (dir exp), pc log	-1.43	.5	-6.87	1.54	5338	2670	2.5
Police (dir exp), share	5.45	1.855	0	71.746	5340	2670	2.5
Judicial (dir exp), pc log	-2.95	.88	-10.12	-.31	5296	2662	2.5
Judicial (dir exp), share	1.401	.851	0	12.486	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 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 US Election Data; 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	8006	2671	1
Construction emp, pc log	-3.62	.45	-6.65	.42	7461	2579	1
Manufacturing emp, pc log	-3.07	.74	-8.03	-.33	7430	2539	1
Leisure emp, pc log	-2.75	.43	-7.26	.23	7916	2658	1
Agric emp, pc log	-6.57	1.53	-11.18	-1.14	4516	1892	1
Weekly average wages, 2010 USD	874	262	313	2401	8009	2672	1
Weekly wages, construction	993	223	227	2363	7464	2580	1
Weekly wages, manufacturing	1121	356	135	3760	7433	2540	1
Weekly wages, leisure	366	120	81	1048	7919	2659	1
Weekly wages, agric	611	198	136	1894	4516	1892	1
Real GDP, pc log	3.88	.44	2.1	8.32	7860	2629	1
Real Median HH income, log	10.88	.26	9.97	11.79	8022	2674	1
Unemployment rate	6.03	2.83	1.4	29.3	8022	2674	1
Number of poor people, log	10.89	1.63	4.04	14.4	8022	2674	1
Number of people in SNAP, log	10.81	1.65	2.77	13.99	8022	2674	1
Out-migration per 1000 people	55.24	17.06	8.63	300.25	8020	2674	1
Adult population (log)	12.63	1.59	5.66	15.85	8022	2674	1
Adult Hispanic pop (log)	10.23	2.43	2.48	15.04	8022	2674	1
Adult White pop (log)	12.36	1.52	5.59	15.49	8022	2674	1
Adult Black pop (log)	10.01	2.34	0	13.74	8013	2674	1
Relative importance univ values	.152	.497	-3.803	3.482	5802	2096	.5
All crime, pc log	-3.48	.94	-11.11	-.92	7872	2657	1.5
Violent crime, pc log	-5.87	1.03	-15.45	-3.42	7820	2652	1.5
Property crime, pc log	-3.88	.92	-11.15	-.95	7858	2656	1.5

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. Population variables come from the U.S. Census Bureau, Population Division. Unemployment rate comes from the LAUS Program. County GDP per capita comes from the Regional Economic Accounts of the Department of Commerce. Out-migration comes from the US Census based on ACS 5. The index of moral universalism comes from Enke et al. (2020). Crime variables come from the Jacob Kaplan's Concatenated Files.

Sources

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

Kaplan, J. (2020). Jacob Kaplan's Concatenated Files: Uniform Crime Reporting (UCR) Program Data: Arrests by Age, Sex, and Race, 1974-2018.

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.

U.S. Department of Commerce. Bureau of Economic Analysis. Regional Economic Accounts (2022). GDP by County, Metro, and Other Areas.

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. One, 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. Two, we use the dataset of underdevelopment estimates. Among others, it has 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, 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 Map of variation employed

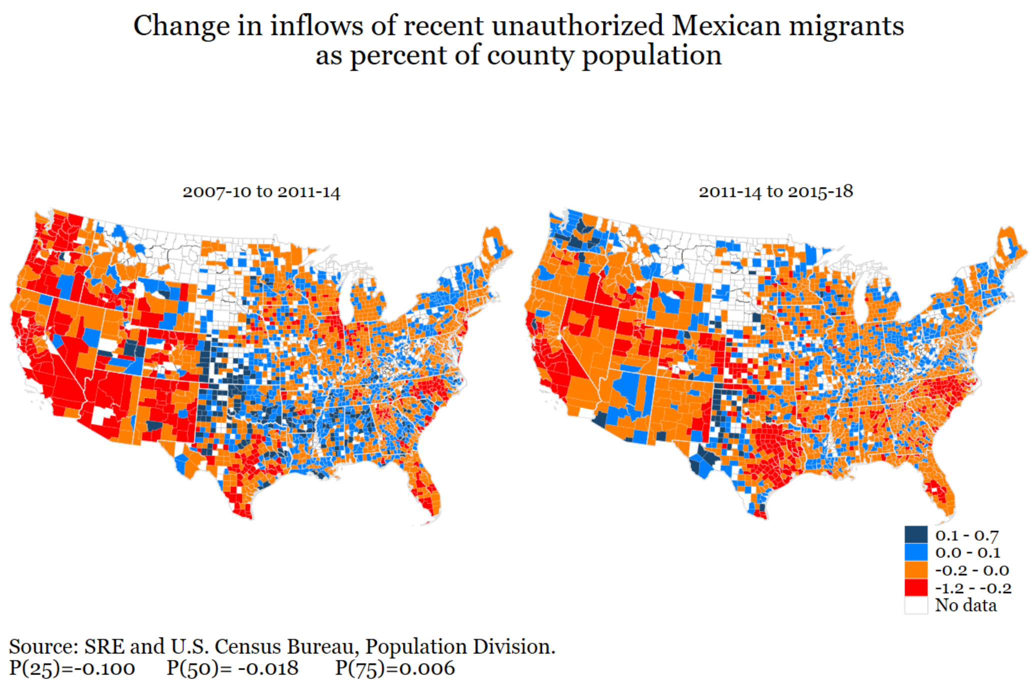

Figure A6: Identifying variation: within state-period county changes

B Robustness checks

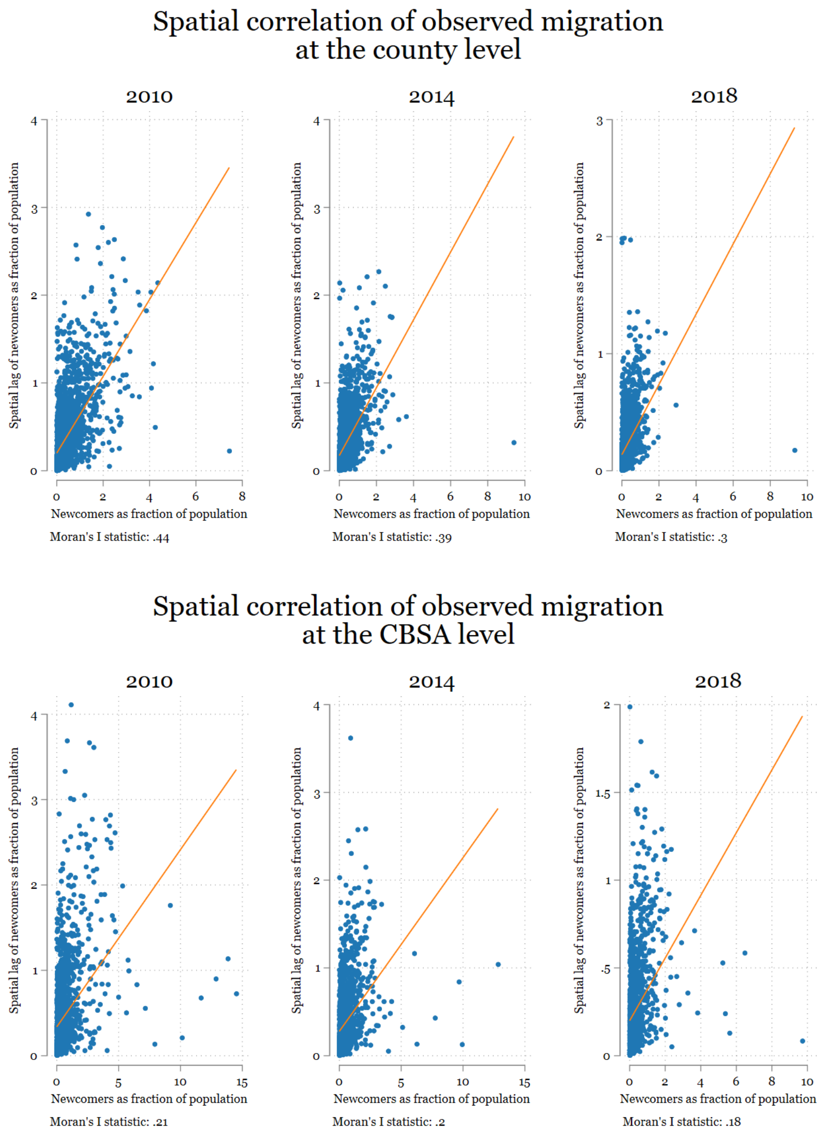

Figure B1: Spatial auto-correlation in number of observed migrants

Table B1: 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	-2.10***	-0.02	-4.25***	-0.80**	0.08	-0.17	0.22
	(0.01)	(0.03)	(0.01)	(0.02)	(0.55)	(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	-2.03***	-0.03	-1.63	-0.59	0.55	-0.16	-0.27
	(0.01)	(0.03)	(0.02)	(0.02)	(0.54)	(0.03)	(1.66)	(0.44)	(0.43)	(0.13)	(0.76)
Std. Coefficient	-0.05	-0.05	-0.04	0.02	-0.23	-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	-0.218	-0.003	-0.175	-0.063	0.059	-0.018	-0.029
C. 2SLS Push Factors											
Newcomers, pct. pop.	-0.03**	-0.07**	-0.06***	-0.02	-2.17***	-0.01	-3.53*	-1.24***	-0.01	-0.17	0.49
	(0.01)	(0.03)	(0.01)	(0.02)	(0.52)	(0.03)	(1.81)	(0.43)	(0.46)	(0.13)	(0.74)
Std. Coefficient	-0.05	-0.09	-0.08	-0.03	-0.25	-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	-0.233	-0.001	-0.378	-0.132	-0.001	-0.018	0.052
Observations	5338	5338	5338	5338	896	5338	5338	5338	5338	5338	5338
Dep. Var., Mean	1.57	1.03	0.50	0.19	-1.60	0.50	59.46	36.31	27.58	1.31	36.11
Dep. Var., Sd	0.38	0.47	0.49	0.54	1.63	0.45	12.44	11.95	12.18	3.69	11.15
Ind. Var., Mean	0.55	0.55	0.55	0.55	0.18	0.55	0.55	0.55	0.55	0.55	0.55
Ind. Var., Sd	0.68	0.68	0.68	0.68	0.23	0.68	0.68	0.68	0.68	0.68	0.68

Source: 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. Regressions control for county and state-period fixed effects, and are weighted by predicted population. Standard errors clustered at the CBSA level. *p<0.1,**p<0.05,***p<0.01

B.1 Main robustness checks

Table B2: Robustness checks

	Midterms	Pres year		Log pc		Share of Expend	
	(1) House	(2) House	(3) Pres	(4) D. Exp	(5) Educ	(6) Police	(7) Judicial
A. Reduced form, baseline							
Instrument	9.86***	4.04***	5.57***	-0.05***	-0.06***	0.49***	0.31***
	(1.22)	(1.37)	(0.77)	(0.02)	(0.02)	(0.17)	(0.12)
B. Lagged outcome (LO)							
Instrument	0.99	2.91	4.10***	-0.00	-0.02	-0.03	-0.02
	(2.01)	(2.04)	(0.56)	(0.02)	(0.01)	(0.20)	(0.11)
C. Mex non-citizen, share							
Instrument	9.86***	8.10***	8.72***	-0.04*	-0.06**	0.42**	0.32**
	(1.36)	(2.31)	(1.08)	(0.03)	(0.03)	(0.19)	(0.15)
D. Hispanics, share							
Instrument	10.34***	4.91***	6.93***	-0.04**	-0.06***	0.48***	0.29**
	(1.33)	(1.77)	(0.75)	(0.02)	(0.02)	(0.18)	(0.12)
E. Adult HS completion							
Instrument	11.20***	5.65***	7.15***	-0.05***	-0.06***	0.50***	0.31***
	(1.25)	(1.31)	(0.57)	(0.02)	(0.02)	(0.17)	(0.12)
F. China shock							
Instrument	8.41***	2.71*	3.90***	-0.05***	-0.07***	0.53***	0.33***
	(1.35)	(1.47)	(0.76)	(0.02)	(0.02)	(0.16)	(0.12)
G. Simulated instrument							
Instrument	9.86***	7.95*	7.59***	-0.05	-0.13***	0.43*	0.49***
	(2.52)	(4.51)	(1.45)	(0.05)	(0.04)	(0.26)	(0.15)
H. Spatial lag							
Instrument	7.97***	4.22*	4.33***	-0.05**	-0.05	0.41**	0.25*
	(1.49)	(2.35)	(1.13)	(0.02)	(0.03)	(0.16)	(0.13)
I. Stock Mex foreign							
Instrument	10.04***	4.67***	5.67***	-0.05***	-0.06***	0.50***	0.31***
	(1.25)	(1.40)	(0.79)	(0.02)	(0.02)	(0.16)	(0.11)
J. Stock Hispanics							
Instrument	9.08***	2.50*	5.23***	-0.05**	-0.06**	0.57***	0.32**
	(1.18)	(1.39)	(0.78)	(0.02)	(0.02)	(0.20)	(0.14)
K. No-outliers							
Instrument	11.01***	5.05***	6.53***	-0.06**	-0.08***	0.65***	0.33*
	(1.46)	(1.56)	(0.88)	(0.03)	(0.02)	(0.21)	(0.17)
L. No pop weights							
Instrument	10.73***	5.75***	6.82***	-0.03	-0.06***	0.55***	0.26***
	(1.27)	(1.08)	(0.76)	(0.02)	(0.02)	(0.19)	(0.09)
*M. County-group * period FE*							
Instrument	8.87**	4.72**	3.32**	-0.08***	-0.06	0.49	0.14
	(3.52)	(1.97)	(1.31)	(0.03)	(0.04)	(0.32)	(0.15)

Dependent variables in columns 1–3 are the vote share for Republicans in different federal elections. Dependent variables in columns 4–5 are the log of per capita (per child population in column 4) expenditure. Columns 6–7 are the share of direct expenditure. All estimations are reduced form. Sources: Dave Leip's US Election Data; Annual Survey of State and Local Government Finances; Peter K. Schott's Data; County Business Patterns; ACS 5 (Ruggles et al., 2022 and Social Explorer); Acemoglu et al. (2016), and US Census Bureau: Population Division. Panel 1 is reduced form estimation. Panel 2 estimates effect on lagged dependent variables. Panel 3 controls for pre-2007 features interacted with period dummies. Panel 4 includes simulated instrument following Borusyak and Hull (2023). To obtain the simulated instrument, we average 2,000 instruments created by interacting 2,000 randomly permuted shifters with the original shares. Panel 5 controls for the spatial lag of the instrument (values of neighboring counties). Panel 6 controls for the stock of Mexican non-citizens and Hispanics at the beginning of period. Panel 7 excludes outliers (percentiles 1 and 99 of the distribution of changes in inflows), does not use predicted population weights, and instead of state-period fixed effects uses politically similar counties-period fixed effects. Standard errors clustered at CBSA level. Estimations control for county and state-period fixed effects, except for row M. Estimations weighted by predicted population, except row L. Stars indicate *p<0.1,**p<0.05,***p<0.01

Table B3: Robustness checks, push factors instrument

	Midterms	Pres year		Log pc		Share of Expend	
	(1) House	(2) House	(3) Pres	(4) D. Exp	(5) Educ	(6) Police	(7) Judicial
A. Reduced form, baseline							
Instrument	10.40***	4.40***	6.81***	-0.05***	-0.04**	0.47**	0.26**
	(1.47)	(1.70)	(0.93)	(0.02)	(0.02)	(0.20)	(0.12)
B. Lagged outcome (LO)							
Instrument	1.22	5.06**	4.56***	0.00	-0.04	-0.07	0.09
	(2.40)	(2.35)	(0.78)	(0.02)	(0.02)	(0.18)	(0.10)
C. Mex non-citizen, share							
Instrument	9.46***	8.85***	10.75***	-0.05**	-0.04	0.38*	0.24
	(1.70)	(2.00)	(1.14)	(0.02)	(0.02)	(0.21)	(0.15)
D. Hispanics, share							
Instrument	10.77***	5.37***	8.50***	-0.05**	-0.04**	0.46**	0.23*
	(1.69)	(1.85)	(0.90)	(0.02)	(0.02)	(0.21)	(0.12)
E. Adult HS completion							
Instrument	12.15***	6.45***	8.81***	-0.05***	-0.05**	0.49**	0.26**
	(1.45)	(1.62)	(0.68)	(0.02)	(0.02)	(0.20)	(0.12)
F. China shock							
Instrument	8.35***	2.48	4.38***	-0.05***	-0.05***	0.54***	0.29**
	(1.65)	(1.75)	(0.92)	(0.02)	(0.02)	(0.19)	(0.12)
G. Simulated instrument							
Instrument	7.98***	10.68**	12.54***	-0.07*	-0.10***	0.33	0.39***
	(2.98)	(4.15)	(1.97)	(0.04)	(0.04)	(0.29)	(0.15)
H. Spatial lag							
Instrument	7.72***	4.75*	5.39***	-0.05**	-0.02	0.38*	0.17
	(1.66)	(2.49)	(1.23)	(0.02)	(0.03)	(0.20)	(0.14)
I. Stock Mex foreign							
Instrument	10.80***	5.48***	7.08***	-0.05***	-0.05**	0.51***	0.27**
	(1.45)	(1.65)	(0.94)	(0.02)	(0.02)	(0.18)	(0.11)
J. Stock Hispanics							
Instrument	9.55***	3.10*	6.43***	-0.05**	-0.04*	0.50**	0.25*
	(1.34)	(1.62)	(0.90)	(0.02)	(0.02)	(0.23)	(0.13)
K. No-outliers							
Instrument	12.55***	8.03***	8.84***	-0.08***	-0.07***	0.77***	0.33*
	(1.58)	(1.84)	(1.05)	(0.03)	(0.02)	(0.23)	(0.19)
L. No pop weights							
Instrument	11.63***	6.70***	8.40***	-0.04	-0.06**	0.83***	0.39***
	(1.70)	(1.33)	(0.95)	(0.03)	(0.03)	(0.29)	(0.10)
*M. County-group * period FE*							
Instrument	8.38*	5.83**	4.64***	-0.07*	-0.04	0.38	0.18
	(5.00)	(2.67)	(1.40)	(0.04)	(0.04)	(0.42)	(0.14)

Columns 1–3 are the vote share for Republicans in different federal elections. Dependent variables in columns 4–5 are the log of per capita (per child population in column 4) expenditure. Columns 6–7 are the share of direct expenditure. All estimations are reduced form. Sources: Dave Leip's US Election Data; Annual Survey of State and Local Government Finances; Peter K. Schott's Data; County Business Patterns; ACS 5 (Ruggles et al., 2022 and Social Explorer); Acemoglu et al. (2016), and US Census Bureau: Population Division. Panel 1 is reduced form estimation. Panel 2 estimates effect on lagged dependent variables. Panel 3 controls for pre-2007 features interacted with period dummies. Panel 4 includes simulated instrument following Borusyak and Hull (2023). Panel 5 controls for the spatial lag of the instrument (values of neighboring counties). Panel 6 controls for the stock of Mexican non-citizens and Hispanics at the beginning of period. Panel 7 excludes outliers (percentiles 1 and 99 of changes in inflows), does not use population weights, and instead of state-period fixed effects uses politically similar counties-period fixed effects. Standard errors clustered at CBSA level. Estimations control for county and state-period fixed effects and are weighted by predicted population, except for rows M and L. *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. The last group includes Cook, Orange, Harris, Maricopa, and LA counties. Our results remain robust.

Table B4: Alternative standard error calculation

	Midterms	Pres year		Log pc		Share of Expend	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	House	House	Pres	D. Exp	Educ	Police	Judicial
Baseline							
Instrument	9.856***	4.045***	5.575***	-0.049***	-0.059***	0.487***	0.307***
	(1.215)	(1.373)	(0.769)	(0.018)	(0.021)	(0.165)	(0.116)
Clustered at state-level							
Instrument	9.863***	4.049**	5.582***	-0.049***	-0.059***	0.488**	0.308**
	(1.424)	(2.014)	(1.335)	(0.016)	(0.017)	(0.193)	(0.134)
Eicker Huber White							
Instrument	9.856***	4.045***	5.575***	-0.049***	-0.059***	0.487***	0.307**
	(1.199)	(1.396)	(0.689)	(0.018)	(0.019)	(0.159)	(0.126)
PCA 1							
Instrument	9.840***	4.020***	5.550***	-0.050***	-0.059***	0.485***	0.307**
	(1.252)	(1.318)	(0.703)	(0.015)	(0.021)	(0.130)	(0.129)
Kmeans, 200 (pca)							
Instrument	9.840***	4.020*	5.550***	-0.050***	-0.059***	0.485***	0.307**
	(2.022)	(2.388)	(1.870)	(0.018)	(0.019)	(0.157)	(0.132)
Kmeans, 1000 (pca)							
Instrument	9.840***	4.020**	5.550***	-0.050***	-0.059***	0.485***	0.307**
	(1.444)	(1.652)	(1.124)	(0.018)	(0.019)	(0.161)	(0.127)
Kmeans, 1000 (all shares)							
Instrument	9.840***	4.020**	5.550***	-0.050***	-0.059***	0.485***	0.307**
	(1.580)	(1.731)	(1.294)	(0.018)	(0.019)	(0.161)	(0.126)
Hierarchical, 800 (all shares)							
Instrument	9.840***	4.020	5.550***	-0.050***	-0.059***	0.485***	0.307**
	(2.181)	(2.488)	(1.924)	(0.018)	(0.020)	(0.152)	(0.135)

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 by the distribution of the first component of all shares —obtained after carrying out a principal component analysis. Counties are assigned to one of 500 groups. Rows 5-6 cluster at the level of 200 or 1000 groups obtained by classifying counties according to their first 10 components using kmeans. Row 7 clusters at the level of one of 1000 groups obtained by classifying counties according to their shares using kmeans. Row 8 clusters at the level of 800 groups obtained by classifying counties according to their shares using hierarchical clusters. Estimations control for county and state-period fixed effects and are weighted by predicted population. *p<0.1,**p<0.05,***p<0.01

B.3 Rotemberg weights

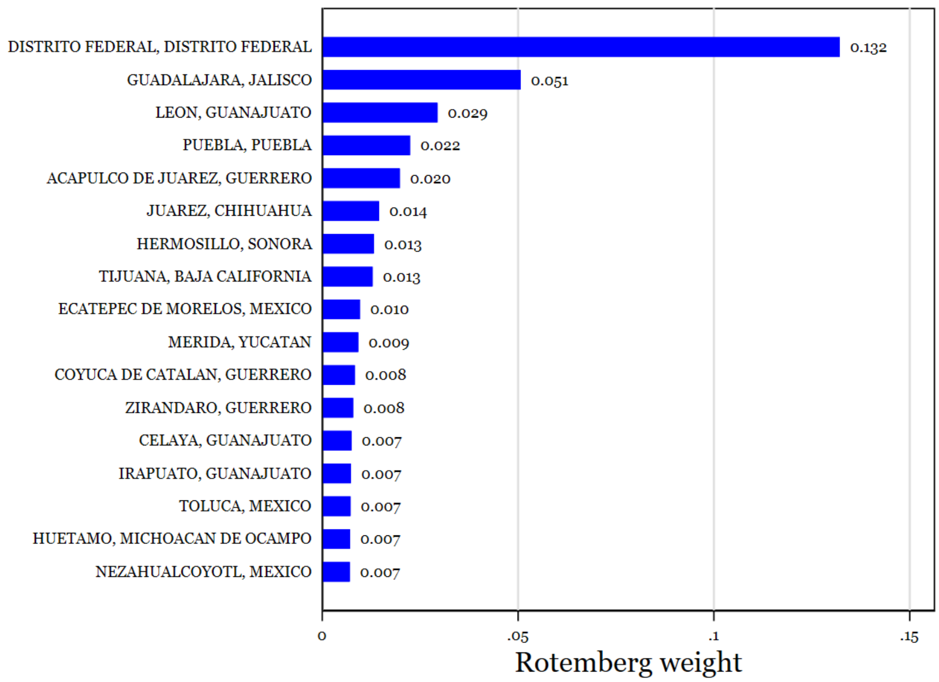

Figure B2: Rotemberg weight using push factor instrument. Made with Goldsmith-Pinkham et al. (2020) code.

Table B5: Robustness checks for mechanisms

	Emp (log)		Wages		County Economy (log)		Log		Rate	Pop (Log)		Values
	(1) Cons	(2) H & L	(3) Cons	(4) Ag	(5) GDP pc	(6) Med HH inc	(7) Poor	(8) SNAP	(9) Out-mig	(10) Tot	(11) White	(12) Univ
A. Reduced form, baseline												
Instrument	-0.06***	-0.02**	-26.20*	-49.98**	-0.03	-0.03*	0.08***	0.03*	1.84**	-0.04***	-0.03***	-0.16***
	(0.02)	(0.01)	(14.80)	(22.43)	(0.02)	(0.02)	(0.01)	(0.02)	(0.72)	(0.01)	(0.01)	(0.05)
B. Lagged outcome (LO)												
Instrument	0.03	-0.01	-1.74	45.35**	-0.00	0.02**	-0.09***	-0.14***		0.01	0.03	
	(0.02)	(0.02)	(9.10)	(20.07)	(0.02)	(0.01)	(0.03)	(0.05)		(0.02)	(0.02)	
C. Mex non-citizen, share												
Instrument	-0.14***	0.01	-56.58***	-34.75	-0.04*	-0.04**	0.06***	0.05*	2.23*	-0.07***	-0.04***	-0.21***
	(0.03)	(0.01)	(15.17)	(25.18)	(0.02)	(0.02)	(0.02)	(0.03)	(1.14)	(0.01)	(0.01)	(0.07)
D. Hispanics, sh												
Instrument	-0.08***	-0.01	-39.50***	-41.66*	-0.03	-0.02*	0.07***	0.04**	1.94**	-0.05***	-0.03***	-0.18***
	(0.02)	(0.01)	(13.41)	(21.79)	(0.02)	(0.01)	(0.02)	(0.02)	(0.77)	(0.01)	(0.01)	(0.05)
E. Adult HS completion												
Instrument	-0.07***	-0.02*	-34.12**	-44.33**	-0.03*	-0.03*	0.07***	0.03*	1.59**	-0.05***	-0.04***	-0.19***
	(0.02)	(0.01)	(14.08)	(22.13)	(0.02)	(0.02)	(0.01)	(0.02)	(0.70)	(0.01)	(0.01)	(0.05)
F. China shock												
Instrument	-0.06***	-0.02	-21.67	-54.72**	-0.02	-0.02	0.07***	0.05**	2.26***	-0.04***	-0.03***	-0.12**
	(0.02)	(0.01)	(15.01)	(23.29)	(0.02)	(0.02)	(0.01)	(0.02)	(0.76)	(0.01)	(0.01)	(0.05)
G. Simulated instrument												
Instrument	-0.17***	0.02	-24.58	-27.16	0.00	-0.01	0.05*	0.12***	4.33**	-0.07***	-0.05***	-0.28***
	(0.04)	(0.03)	(25.62)	(36.33)	(0.03)	(0.02)	(0.03)	(0.03)	(1.72)	(0.01)	(0.02)	(0.10)
H. Spatial lag												
Instrument	-0.06**	0.00	-16.47	-44.54*	-0.02	-0.02*	0.07***	0.04*	1.34*	-0.03*	-0.02	-0.14**
	(0.03)	(0.01)	(16.00)	(24.75)	(0.02)	(0.01)	(0.02)	(0.02)	(0.80)	(0.02)	(0.01)	(0.06)
I. Stock Mex foreign												
Instrument	-0.06***	-0.02**	-24.69*	-50.85**	-0.03	-0.02	0.07***	0.03	1.66**	-0.04***	-0.03***	-0.13**
	(0.02)	(0.01)	(14.79)	(23.12)	(0.02)	(0.02)	(0.01)	(0.02)	(0.72)	(0.01)	(0.01)	(0.05)
J. Stock Hispanics												
Instrument	-0.06***	-0.04**	-31.90**	-65.44***	-0.05**	-0.04***	0.09***	0.06**	1.01	-0.04***	-0.03**	-0.17***
	(0.02)	(0.02)	(14.59)	(25.04)	(0.02)	(0.02)	(0.02)	(0.02)	(0.76)	(0.01)	(0.01)	(0.05)
K. No-outliers												
Instrument	-0.08***	-0.04***	-34.82**	-75.27**	-0.04	-0.04**	0.10***	0.05*	1.97**	-0.06***	-0.04***	-0.21***
	(0.03)	(0.01)	(16.29)	(30.17)	(0.03)	(0.02)	(0.02)	(0.03)	(1.00)	(0.01)	(0.01)	(0.07)
L. No pop weights												
Instrument	-0.09***	-0.01	-37.66***	-9.73	-0.02	-0.04***	0.05***	0.04*	0.29	-0.08***	-0.07***	-0.25***
	(0.03)	(0.01)	(11.12)	(13.70)	(0.02)	(0.01)	(0.01)	(0.02)	(1.93)	(0.01)	(0.01)	(0.09)
*M. County-group * period FE*												
Instrument	-0.01	-0.02	8.34	-10.17	0.01	0.01	0.04**	0.00	2.32	-0.02*	-0.03***	-0.06
	(0.06)	(0.02)	(28.80)	(22.13)	(0.04)	(0.02)	(0.02)	(0.03)	(1.63)	(0.01)	(0.01)	(0.12)

Dependent variables in columns 1–2 are the the log of average annual employment divided by working age population. Dependent variables in columns 3–4 are the annual average weekly wages in 2010 USD. Dependent variables in columns 5–6 are the log of GDP per capita and median household income. Dependent variables in columns 7–8 are the log of poor people and of SNAP recipients. The dependent variable in column 9 is out-migration, calculated as the number of out-migrants (in thousands) divided by county population. Dependent variables in columns 10–11 are the log of total adults population and white adult population. Column 12 is the average county value, as reported by Enke (2020). All estimations are reduced form. Sources: ACS 5 (Ruggles et al., 2022 and Social Explorer); Department of Commerce; SAIPE; Peter K. Schott's Data; County Business Patterns; Acemoglu et al. (2016); Enke (2020); US Census Bureau: Population Division, and QCEW. Panel 1 is reduced form estimation. Panel 2 estimates effect on lagged dependent variables. We do not have lagged values for out-migration and the index of relative universalism. Panel 3 controls for pre-2007 features interacted with period dummies. Panel 4 includes a simulated instrument following Borusyak and Hull (2023). Panel 5 controls for the spatial lag of the instrument.Panel 6 controls for the stock of Mexican non-citizens and Hispanics at the beginning of period. Panel 7 excludes outliers (percentiles 1 and 99 of the distribution of changes in inflows), does not use predicted population weights and uses politically similar counties-period fixed effects. Standard errors clustered at CBSA level. Estimations control for county and state-period fixed effects, except for row M, and are weighted by predicted population, except row L. $^{*}p<0.1$,$^{**}p<0.05$,$^{***}p<0.01$

Table B6: Robustness checks for mechanisms, push factors instrument

	Emp (log)		Wages		County Economy (log)		Log		Rate	Pop (Log)		Values
	(1) Cons	(2) H & L	(3) Cons	(4) Ag	(5) GDP pc	(6) Med HH inc	(7) Poor	(8) SNAP	(9) Out-mig	(10) Tot	(11) White	(12) Univ
					A. Reduced form, baseline							
Instrument	-0.06*	-0.04***	-35.02***	-55.29**	-0.05***	-0.04**	0.09***	0.01	1.79**	-0.04***	-0.03**	-0.22***
	(0.03)	(0.01)	(12.80)	(25.55)	(0.02)	(0.02)	(0.02)	(0.03)	(0.81)	(0.01)	(0.01)	(0.06)
					B. Lagged outcome (LO)							
Instrument	0.05**	-0.03	-4.81	45.97*	-0.01	0.03**	-0.11***	-0.14**		0.03	0.05**	
	(0.02)	(0.03)	(9.52)	(27.23)	(0.02)	(0.01)	(0.03)	(0.06)		(0.02)	(0.02)	
					C. Mex non-citizen, share							
Instrument	-0.14***	-0.01	-76.89***	-36.57	-0.07***	-0.06***	0.08***	0.01	1.98*	-0.07***	-0.04***	-0.31***
	(0.03)	(0.01)	(16.97)	(29.54)	(0.03)	(0.02)	(0.02)	(0.03)	(1.13)	(0.01)	(0.01)	(0.08)
					D. Hispanics, share							
Instrument	-0.08***	-0.02	-52.48***	-44.61*	-0.05**	-0.04**	0.08***	0.02	1.87**	-0.05***	-0.03**	-0.25***
	(0.03)	(0.01)	(11.75)	(24.36)	(0.02)	(0.02)	(0.02)	(0.02)	(0.84)	(0.01)	(0.01)	(0.06)
					E. Adult HS completion							
Instrument	-0.07**	-0.03**	-44.97***	-47.56*	-0.06***	-0.04**	0.08***	0.01	1.47*	-0.05***	-0.04***	-0.27***
	(0.03)	(0.01)	(12.20)	(24.88)	(0.02)	(0.02)	(0.02)	(0.02)	(0.78)	(0.01)	(0.01)	(0.06)
					F. China shock							
Instrument	-0.05*	-0.03**	-29.85**	-64.81**	-0.04**	-0.03*	0.09***	0.03	2.24***	-0.03**	-0.03**	-0.17***
	(0.03)	(0.01)	(13.10)	(27.06)	(0.02)	(0.02)	(0.02)	(0.03)	(0.87)	(0.01)	(0.01)	(0.06)
					G. Simulated instrument							
Instrument	-0.20***	0.00	-54.55**	-19.64	-0.06	-0.03	0.06*	0.06	5.05*	-0.07***	-0.05*	-0.60***
	(0.04)	(0.03)	(24.66)	(38.98)	(0.04)	(0.02)	(0.04)	(0.04)	(2.68)	(0.02)	(0.02)	(0.13)
					H. Spatial lag							
Instrument	-0.05*	-0.00	-23.63	-49.44	-0.04*	-0.04**	0.09***	0.01	1.01	-0.02	-0.01	-0.21***
	(0.03)	(0.01)	(15.79)	(30.75)	(0.02)	(0.02)	(0.02)	(0.02)	(0.99)	(0.02)	(0.02)	(0.07)
					I. Stock Mex foreign							
Instrument	-0.06**	-0.04***	-33.42**	-58.29**	-0.04**	-0.03**	0.08***	0.00	1.51*	-0.04***	-0.03**	-0.18***
	(0.03)	(0.01)	(13.06)	(27.43)	(0.02)	(0.02)	(0.02)	(0.03)	(0.80)	(0.01)	(0.01)	(0.06)
					J. Stock Hispanics							
Instrument	-0.05*	-0.05***	-39.08***	-64.57**	-0.07***	-0.05***	0.10***	0.03	1.09	-0.04***	-0.03**	-0.23***
	(0.03)	(0.02)	(13.15)	(27.34)	(0.02)	(0.02)	(0.02)	(0.03)	(0.90)	(0.01)	(0.01)	(0.06)
					K. No-outliers							
Instrument	-0.10***	-0.05***	-53.48***	-87.03**	-0.08**	-0.07***	0.14***	0.03	1.47	-0.07***	-0.05***	-0.28***
	(0.03)	(0.02)	(15.99)	(38.30)	(0.03)	(0.02)	(0.02)	(0.03)	(1.23)	(0.01)	(0.01)	(0.08)
					L. No pop weights							
Instrument	-0.11***	-0.02	-42.33***	-17.92	-0.02	-0.05***	0.07***	0.03	0.61	-0.09***	-0.08***	-0.31***
	(0.04)	(0.02)	(14.67)	(14.90)	(0.03)	(0.01)	(0.01)	(0.02)	(3.18)	(0.01)	(0.01)	(0.11)
					*M. County-group * period FE*							
Instrument	-0.02	-0.02	1.94	-16.87	0.01	0.01	0.02	-0.06	3.42*	-0.03*	-0.04**	-0.09
	(0.07)	(0.02)	(38.71)	(27.81)	(0.05)	(0.03)	(0.03)	(0.04)	(2.04)	(0.02)	(0.02)	(0.17)

Dependent variables in columns 1–2 are the the log of average annual employment divided by working age population. Dependent variables in columns 3–4 are the annual average weekly wages in 2010 USD. Dependent variables in columns 5–6 are the log of GDP per capita and median household income. Dependent variables in columns 7–8 are the log of poor people and of SNAP recipients. The dependent variable in column 9 is out-migration, calculated as the number of out-migrants (in thousands) divided by county population. Dependent variables in columns 10–11 are the log of total adults population and white adult population. Column 12 is the average county value, as reported by Enke (2020). All estimations are reduced form. Sources: ACS 5 (Ruggles et al., 2022 and Social Explorer); Department of Commerce; SAIPE; Peter K. Schott's Data; County Business Patterns; Acemoglu et al. (2016); Enke (2020); US Census Bureau: Population Division, and QCEW. Panel 1 is reduced form estimation. Panel 2 estimates effect on lagged dependent variables. Panel 3 controls for pre-2007 features interacted with period dummies. Panel 4 includes a simulated instrument following Borusyak and Hull (2023). Panel 5 controls for the spatial lag of the instrument (values of neighboring counties). Panel 6 controls for the stock of Mexican non-citizens and Hispanics at the beginning of period. Panel 7 excludes outliers (percentiles 1 and 99 of the distribution of changes in inflows), does not use predicted population weights and instead of state-period fixed effects uses politically similar counties-period fixed effects. Standard errors clustered at CBSA level. Estimations control for county and state-period fixed effects, except for row M. Estimations weighted by predicted population, except row L. Stars indicate *p<0.1,**p<0.05,***p<0.01

Table B7: Robustness checks for null results

	Share D. Exp	Midterms	Employment (log)		Weekly Wages (2010 USD)			Rate	Crime (log pc)		
	(1) Edu	(2) Turnout	(3) Total	(4) Agric	(5) Total	(6) Manufac	(7) Hosp and leis	(8) Unemp	(9) All	(10) Violent	(11) Property
A. Reduced form, baseline											
Instrument	0.27	-0.81	-0.00	-2.08	18.51	-9.37	-49.98**	0.20	-0.03	-0.01	-0.01
	(0.71)	(0.96)	(0.01)	(21.63)	(38.75)	(7.42)	(22.43)	(0.21)	(0.03)	(0.03)	(0.03)
B. Lagged outcome (LO)											
Instrument	-1.38**	1.01	0.02	-20.80**	-7.98	-17.55***	45.35**	-0.34	0.07*	0.10	0.07
	(0.64)	(1.14)	(0.02)	(10.14)	(15.00)	(5.97)	(20.07)	(0.27)	(0.04)	(0.07)	(0.04)
C. Mex non-citizen, share											
Instrument	-0.40	-1.61	-0.01	-46.21**	-5.02	-21.46***	-34.75	0.07	-0.04	-0.01	-0.04
	(1.08)	(1.45)	(0.01)	(19.20)	(40.56)	(6.60)	(25.18)	(0.23)	(0.03)	(0.04)	(0.04)
D. Hispanics, share											
Instrument	-0.03	-1.23	0.00	-12.31	14.53	-7.85	-41.66*	0.05	-0.01	-0.02	0.02
	(0.78)	(1.20)	(0.01)	(18.77)	(35.52)	(7.10)	(21.79)	(0.19)	(0.03)	(0.03)	(0.03)
E. Adult HS completion											
Instrument	0.25	-1.15	-0.00	-7.71	16.88	-9.80	-44.33**	-0.03	-0.03	-0.01	-0.01
	(0.72)	(0.99)	(0.01)	(20.35)	(37.45)	(7.06)	(22.13)	(0.17)	(0.03)	(0.03)	(0.03)
F. China shock											
Instrument	-0.21	-0.34	-0.00	5.69	26.71	-4.85	-54.72**	0.35	-0.01	-0.03	0.02
	(0.73)	(1.02)	(0.01)	(22.21)	(41.45)	(7.36)	(23.29)	(0.22)	(0.03)	(0.04)	(0.03)
G. Simulated instrument											
Instrument	-1.76	-1.99	-0.02	19.39	143.03	7.65	-27.16	1.02**	0.13***	-0.01	0.18***
	(1.54)	(2.46)	(0.02)	(45.10)	(95.11)	(11.51)	(36.33)	(0.42)	(0.05)	(0.07)	(0.06)
H. Spatial lag											
Instrument	0.17	-1.32	0.00	-5.17	25.99	-6.59	-44.54*	0.36	-0.02	-0.01	-0.01
	(1.02)	(1.53)	(0.01)	(17.96)	(39.98)	(7.51)	(24.75)	(0.23)	(0.03)	(0.04)	(0.03)
I. Stock Mex foreign											
Instrument	0.19	-0.53	-0.00	-0.53	18.94	-9.19	-50.85**	0.22	-0.02	0.00	-0.00
	(0.69)	(0.94)	(0.01)	(20.85)	(38.99)	(7.43)	(23.12)	(0.21)	(0.03)	(0.03)	(0.03)
J. Stock Hispanics											
Instrument	0.84	-1.75**	-0.01	-15.57	-5.40	-16.56**	-65.44***	0.17	-0.04	-0.03	-0.02
	(0.78)	(0.85)	(0.01)	(20.88)	(37.38)	(8.10)	(25.04)	(0.22)	(0.03)	(0.04)	(0.03)
K. No-outliers											
Instrument	0.13	0.65	-0.01	-15.65	-0.03	-18.46***	-75.27**	0.60**	-0.03	0.02	-0.02
	(0.87)	(0.78)	(0.01)	(24.87)	(46.97)	(7.04)	(30.17)	(0.23)	(0.04)	(0.04)	(0.04)
L. No pop weights											
Instrument	-1.28	-1.58***	0.01	-15.21**	21.79	-3.91	-9.73	-0.26*	0.02	0.04	0.02
	(0.96)	(0.57)	(0.01)	(6.29)	(17.32)	(3.13)	(13.70)	(0.16)	(0.05)	(0.06)	(0.05)
*M. County-group * period FE*											
Instrument	-1.08	-1.43*	-0.00	-4.30	39.68	3.02	38.80*	-0.16	-0.06	-0.03	-0.04
	(1.30)	(0.81)	(0.02)	(13.05)	(30.61)	(6.00)	(20.54)	(0.17)	(0.07)	(0.09)	(0.08)

Dependent variables in column 1 is education expenditure as total direct expenditures. Dependent variable in column 2 is turnout in House midterm elections, as share of registered voters. Dependent variables in columns 3–4 are the log of average annual employment divided by working age population. Dependent variables in columns 5–8 are the annual average weekly wages in 2010 USD. Dependent variables in columns 9–11 are two year averages of the log of per capita total crime, violent crime index and property crime index. Sources: Annual Survey of State and Local Government Finances; Dave Leip's US Election Data; QCEW; Jacob Kaplan's Concatenated Files: Uniform Crime Reporting Program Data: Offenses Known and Clearances by Arrest (Return A), 1960-2020; ACS 5 (Ruggles et al., 2022 and Social Explorer); Department of Commerce; SAIPE; Peter K. Schott's Data; County Business Patterns; Acemoglu et al. (2016); Enke (2020), and US Census Bureau: Population Division. Panel 1 is reduced form estimation. Panel 2 estimates effect on lagged dependent variables. Panel 3 controls for pre-2007 features interacted with period dummies. Panel 4 includes a simulated instrument following Borusyak and Hull (2023). Panel 5 controls for the spatial lag of the instrument (values of neighboring counties). Panel 6 controls for the stock of Mexican non-citizens and Hispanics at the beginning of period. Panel 7 excludes outliers, does not use predicted population weights and instead of state-period fixed effects uses politically similar counties-period fixed effects. Standard errors clustered at CBSA level. Estimations control for county and state-period fixed effects, except for row M. Estimations weighted by predicted population, except row L. Stars indicate *p<0.1,**p<0.05,***p<0.01

Table B8: Robustness checks for null results, push factors instrument

	Share D. Exp	Midterms	Employment (log)		Weekly Wages (2010 USD)			Rate	Crime (log pc)		
	(1) Edu	(2) Turnout	(3) Total	(4) Agric	(5) Total	(6) Manufac	(7) Hosp and leis	(8) Unemp	(9) All	(10) Violent	(11) Property
A. Reduced form, baseline											
Instrument	1.15*	-0.71	-0.00	-18.19	-14.83	-13.23*	-55.29**	0.08	-0.06*	-0.01	-0.05
	(0.65)	(0.77)	(0.01)	(16.74)	(27.94)	(8.02)	(25.55)	(0.25)	(0.03)	(0.04)	(0.04)
B. Lagged outcome (LO)											
Instrument	-1.81***	3.01*	0.02	-24.88**	-20.21	-26.75***	45.97*	-0.71**	0.12**	0.20**	0.11**
	(0.60)	(1.70)	(0.02)	(9.88)	(19.90)	(5.55)	(27.23)	(0.32)	(0.05)	(0.09)	(0.05)
C. Mex non-citizen, share											
Instrument	0.95	-1.74	-0.02	-84.57***	-73.82	-31.73***	-36.57	-0.21	-0.11***	-0.00	-0.12***
	(0.91)	(1.46)	(0.01)	(24.80)	(50.70)	(8.55)	(29.54)	(0.29)	(0.04)	(0.05)	(0.04)
D. Hispanics, share											
Instrument	0.89	-1.23	-0.00	-33.13**	-24.40	-11.98	-44.61*	-0.14	-0.04	-0.03	-0.02
	(0.71)	(1.11)	(0.01)	(14.75)	(27.09)	(8.00)	(24.36)	(0.25)	(0.03)	(0.04)	(0.04)
E. Adult HS completion											
Instrument	1.14*	-1.14	-0.00	-25.40*	-17.00	-13.75*	-47.56*	-0.23	-0.06**	-0.02	-0.05
	(0.66)	(0.85)	(0.01)	(15.31)	(27.35)	(7.54)	(24.88)	(0.25)	(0.03)	(0.04)	(0.03)
F. China shock											
Instrument	0.59	-0.23	-0.01	-8.91	-7.18	-7.61	-64.81**	0.27	-0.04	-0.04	-0.01
	(0.68)	(0.85)	(0.01)	(17.35)	(31.40)	(7.88)	(27.06)	(0.26)	(0.03)	(0.04)	(0.04)
G. Simulated instrument											
Instrument	1.05	-1.97	-0.03	-43.12	62.25	5.39	-19.64	0.88	0.09	-0.01	0.15
	(1.72)	(2.30)	(0.02)	(37.18)	(57.48)	(14.71)	(38.98)	(0.56)	(0.08)	(0.08)	(0.09)
H. Spatial lag											
Instrument	1.36	-1.40	0.00	-28.83**	-16.33	-10.49	-49.44	0.26	-0.06*	-0.01	-0.05
	(1.16)	(1.66)	(0.01)	(13.24)	(27.09)	(9.28)	(30.75)	(0.27)	(0.03)	(0.05)	(0.04)
I. Stock Mex foreign											
Instrument	0.93	-0.24	-0.00	-16.79	-16.33	-13.27*	-58.29**	0.09	-0.04	0.01	-0.04
	(0.65)	(0.72)	(0.01)	(14.87)	(29.31)	(8.01)	(27.43)	(0.25)	(0.03)	(0.04)	(0.04)
J. Stock Hispanics											
Instrument	1.64**	-1.39**	-0.01	-29.34	-35.14	-18.76**	-64.57**	0.04	-0.07**	-0.03	-0.06
	(0.77)	(0.60)	(0.02)	(20.26)	(30.22)	(9.19)	(27.34)	(0.27)	(0.03)	(0.04)	(0.04)
K. No-outliers											
Instrument	1.01	0.51	-0.02	-58.04***	-70.07*	-29.84***	-87.03**	0.57**	-0.06	0.04	-0.07
	(0.88)	(0.83)	(0.02)	(21.42)	(42.53)	(6.48)	(38.30)	(0.27)	(0.04)	(0.06)	(0.05)
L. No pop weights											
Instrument	-1.08	-1.83***	0.02	-30.10***	3.34	-5.09	-17.92	-0.41**	0.04	0.06	0.04
	(1.13)	(0.71)	(0.01)	(7.91)	(19.40)	(4.02)	(14.90)	(0.19)	(0.05)	(0.07)	(0.05)
*M. County-group * period FE*											
Instrument	-0.80	-1.41	0.00	-20.30	37.26	4.64	29.26	-0.36*	-0.05	0.01	-0.03
	(1.53)	(0.99)	(0.02)	(13.24)	(29.78)	(8.12)	(22.72)	(0.21)	(0.09)	(0.11)	(0.09)

Dependent variables in column 1 is education expenditure as total direct expenditures. Dependent variable in column 2 is turnout in House midterm elections, as share of registered voters. Dependent variables in columns 3–4 are the log of average annual employment divided by working age population. Dependent variables in columns 5–8 are the annual average weekly wages in 2010 USD. Dependent variables in columns 9–11 are two year averages of the log of per capita total crime, violent crime index and property crime index. Sources: Annual Survey of State and Local Government Finances; Dave Leip's US Election Data; QCEW; Jacob Kaplan's Concatenated Files: Uniform Crime Reporting Program Data: Offenses Known and Clearances by Arrest (Return A), 1960-2020; ACS 5 (Ruggles et al., 2022 and Social Explorer); Department of Commerce; SAIPE; Peter K. Schott's Data; County Business Patterns; Acemoglu et al. (2016); Enke (2020), and US Census Bureau: Population Division. Panel 1 is reduced form estimation. Panel 2 estimates effect on lagged dependent variables. Panel 3 controls for pre-2007 features interacted with period dummies. Panel 4 includes a simulated instrument following Borusyak and Hull (2023). Panel 5 controls for the spatial lag of the instrument (values of neighboring counties). Panel 6 controls for the stock of Mexican non-citizens and Hispanics at the beginning of period. Panel 7 excludes outliers, does not use predicted population weights and instead of state-period fixed effects uses politically similar counties-period fixed effects. Standard errors clustered at CBSA level. Estimations control for county and state-period fixed effects, except for row M. Estimations weighted by predicted population, except row L. Stars indicate *p<0.1,**p<0.05,***p<0.01

C Additional analysis

C.1 Effects for ideology

Table C1: Ideology effects of arrival of unauthorized Mexican migrants (2010-19)

	Partisan Identity	Ideology	
	(1)	(2)	(3)
	Republican	Conservative	Conservative/very conserv
A. OLS			
Newcomers, pct. pop.	0.29	0.15	0.00
	(0.26)	(0.19)	(0.00)
B. 2SLS LOO			
Newcomers, pct. pop.	0.114	0.117	0.007**
	(0.298)	(0.224)	(0.003)
Std. Coefficient	0.021	0.030	0.068
$\hat{\beta} * P(75) - P(25)$	0.012	0.013	0.001
C. 2SLS Push Factors			
Newcomers, pct. pop.	0.060	0.073	0.004
	(0.294)	(0.220)	(0.003)
Std. Coefficient	0.01	0.02	0.04
$\hat{\beta} * P(75) - P(25)$	0.01	0.01	0.00
Observations	5223	5113	1883
Dep. Var., Mean	6.32	5.17	0.35
Dep. Var., Sd	3.12	2.23	0.06
Ind. Var., Mean	0.47	0.47	0.52
Ind. Var., Sd	0.59	0.59	0.61

Column 1 is partisan identity, ranging from 1 (Strong Democrat) to 7 (Strong Republican). Column 2 is ideology, ranges from 1 (Very Liberal) to 5 (Very Conservative). These two variables come from CES. Column 3 is the share of the Metropolitan Statistical Area (MSA) who identifies as Conservative or Very Conservative, obtained from the Gallup Daily Poll —all the counties within one MSA take the same value. Sources: CES; Gallup Daily Poll; US Census Bureau: Population Division and Small Area. 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. Estimations control for county and state-period fixed effects, and are weighted by predicted population. *p<0.1,**p<0.05,***p<0.01

The Cooperative Election Study (CES) is a survey of political ideas and behaviors. The cumulative data-set contains 557,455 observations across 3,079 counties. We use an ideology measure that quantifies political leanings in five categories from very liberal to very conservative. For partisan identity, we use a seven-category measure that ranges from strong Democrat to strong Republican. We adjust using the weights provided by CES, and exclude counties with less than five observations per year, dropping all the "Not Sure" and "Don't

Know" responses.

The Gallup Daily Tracker was a poll carried out between 2008 and 2018 daily to 1,000 people around the US. One of the questions asked is "How would you describe your political views?", with categories going from Very Conservative to Very Liberal. The most detailed version is at the ZIP code level but we only have access to the MSA level. We have data for 2008-2016, so we use the 2008, 2012, and 2016 values.

C.2 Effects on crime

The perception of immigrants as a criminal threat is widely theorized. This section explores whether there is evidence of increased threat as a result of inflows of unauthorized Mexican migration. Our crime information comes from the Jacob Kaplan's Concatenated Files, retrieved from the National Archive of Criminal Justice Data (Kaplan, 2020). This unofficial dataset condenses the information of yearly "Offenses Known and Clearances by Arrest (Return A)" by crime reported by the Uniform Crime Reporting Program Data. We focus on total crime, violent crime index, and property crime index. Since these crime data are noisy, we aggregate counts for 2010–11, 2014–15, and 2018–19. We construct our measures by dividing the counts by county population and then taking the natural logarithm. As Columns 5-7 of Table C2 indicate, there is no evidence of a causal link between inflows of unauthorized migration and crime —if anything, the association seems to be negative.

Another potential explanation for the shifting of votes in favor of the law-and-order party or police and judiciary spending is the demand for deportation of the unauthorized migrants, following a logic of backlash (Barone et al., 2016). To examine this account, we use the intensive margin of participation in Secure Communities (SC). Before participation was mandatory, in 2013, the program became politicized. States tried to opt-out. If the shift is driven by a backlash, we would expect larger efforts to deport the unauthorized migrant population and more extensive use of the Secure Communities program.

We compile aggregated statistics from SC from October 2008 to September 2013. We

Table C2: Effect of unauthorized Mexican migration on crime (2010-19) and immigration enforcement (2008-13)

	Count by foreign population (log)			Rate	Crime (log pc)		
	(1) Submissions	(2) Matches	(3) Removals	(4) Success	(5) All	(6) Violent	(7) Property
			A. OLS				
Newcomers, pct. pop.	0.47***	0.98***	0.99***	0.52***	-0.01	-0.02	0.01
	(0.10)	(0.11)	(0.13)	(0.05)	(0.02)	(0.03)	(0.03)
			B. 2SLS Loo				
Newcomers, pct. pop.	0.71***	1.31***	1.26***	0.62***	-0.02	-0.01	-0.01
	(0.11)	(0.13)	(0.16)	(0.05)	(0.02)	(0.03)	(0.03)
Std. Coefficient	0.26	0.38	0.33	0.37	-0.01	-0.01	-0.01
$\hat{\beta} * P(75) - P(25)$	0.08	0.14	0.14	0.07	-0.00	-0.00	-0.00
			C. 2SLS push factors				
Newcomers, pct. pop.	0.95***	1.64***	1.59***	0.71***	-0.04*	-0.01	-0.04
	(0.10)	(0.13)	(0.15)	(0.07)	(0.02)	(0.03)	(0.03)
Std. Coefficient	0.35	0.47	0.42	0.42	-0.03	-0.01	-0.02
$\hat{\beta} * P(75) - P(25)$	0.10	0.18	0.17	0.08	-0.00	-0.00	-0.00
Observations	7964	7584	6068	7587	7847	7789	7830
Dep. Var., Mean	7.62	4.17	2.23	1.13	-3.49	-5.87	-3.89
Dep. Var., Sd	1.61	2.07	2.30	1.00	0.94	1.02	0.92
Ind. Var., Mean	0.46	0.47	0.48	0.47	0.46	0.46	0.46
Ind. Var., Sd	0.60	0.60	0.61	0.60	0.60	0.60	0.60
Inst., Mean	0.45	0.45	0.47	0.45	0.44	0.44	0.44
Inst., Sd	0.60	0.60	0.61	0.60	0.60	0.60	0.60

Columns 1-3 are submissions, matches and removals from Secure Communities (SC), calculated proportional to the time SC was in place in the county between 2008 and 2013 and proportional to the foreign population in the county in 2010. Column 4 is success rate –matched/submissions. Columns 5-7 are two year averages of the log of per capita total crime, violent crime index and property crime index. Sources: ICE: Secure Communities Monthly Statistics through September 30, 2013; Jacob Kaplan's Concatenated Files: Uniform Crime Reporting Program Data: Offenses Known and Clearances by Arrest (Return A), 1960-2020, and US Census Bureau: Population Division. Newcomers are the number of new consular IDs per county per period as a proportion of predicted population as described in Section 3. Source: SRE. Instrument is as described in Section 3. Standard errors are robust for the first four columns and clustered at the CBSA level for the last three. The first four estimations control for state fixed effects and the last three control for county and state-period fixed effects. Estimations weighted by predicted population. *p<0.1,**p<0.05,***p<0.01

focus on four outcomes. We use fingerprint submissions to capture local inquiries to ICE. Fingerprint matches are the subset of inquiries by local authorities for which ICE determines the individual is deportable. Removals are the subset of matches for which deportation actually occurs. We also calculate the match success rate, which is the ratio of matches to submissions.

Columns 1 through 4 in Table C2 display the results of the analysis on SC. The second stage estimates suggest that in response to a 0.1 percentage point inflow of migrants, police departments increase the number of fingerprint submissions per foreign born population by 8% (Panel B, Column 1, std coeff: 0.26). Furthermore, counties increased the number of matches from ICE by 14% (Panel B, Column 2, std coeff: 0.38), and subsequent removals (deportations) increased by 14%, as well (Panel B, Column 3, std coeff: 0.33). These findings suggest that as more unauthorized migrants arrive in a county, police and sheriff's departments use Secure Communities more often and with greater accuracy. Indeed the success rate improves dramatically. Authorities both use the program more and use it better.

Since these estimates are based on a cross-section, we are hesitant to draw firm conclusions from the analysis. Nevertheless, the evidence does suggest a local approach to using Secure Communities that is actively anti-immigrant.

C.3 Effects on values and out-migration by economic grievance

Table C3: Effect of newcomers on values and out-migration (2010-19), by index of economic grievance

	Above median economic grievance	
	(1) Universalist values	(2) Out migration
A. Reduced form Loo		
Newcomers, pct. pop.	-0.10*	1.96**
	(0.06)	(0.84)
Above median index eco. grievance × Newcomers, pct. pop.	-0.06	-4.07
	(0.17)	(2.85)
B. Reduced form push factor		
Newcomers, pct. pop.	-0.16**	1.85**
	(0.07)	(0.92)
Above median index eco. grievance × Newcomers, pct. pop.	-0.07	-4.15
	(0.22)	(3.14)
Observations	5400	7146
Dep. Var. Below, Mean	0.19	54.09
Dep. Var. Below, Sd	0.47	16.17
Dep. Var. Above, Mean	0.08	56.44
Dep. Var. Above, Sd	0.53	17.43
Inst. Loo, Below, Mean	0.62	0.62
Inst. Loo, Below Sd	0.66	0.66
Inst. Loo. Above, Mean	0.14	0.14
Inst. Loo. Above, Sd	0.26	0.26

Column 1 is the average relative importance of universalist values, taken from Enke (2020). Column 2 is out-migration, calculated as the number of out-migrants (in thousands) divided by county population. We interact the instruments and fixed effects with an indicator of whether the county had an above-median value of an index of economic grievance. To create the index, we calculate the relative change in the poverty rate, employment in construction per capita, and employment in hospitality and leisure (H&L) per capita during the three periods. We standardized the variables and subtract the relative change in construction and H&L from the relative change in the poverty rate —a high relative change in poverty breeds economic grievance but a high change in employment does the opposite. Sources: Enke, (2020); US Census Bureau: Population Division and Small Area; ACS 5; QCEW; SAIPE. Regressions are reduced form. Instrument is as described in Section 3. Source: SRE. Standard errors clustered at the CBSA level. Estimations control for county and state-period fixed effects and weighted by predicted population. Stars *p<0.1,**p<0.05,***p<0.01

Table C4: Effect of newcomers on values and out-migration (2010-19), by change in poverty levels

	(1) Universalist values	(2) Out migration
A. Reduced form Loo		
Newcomers, pct. pop.	-0.12*	1.48*
	(0.06)	(0.85)
Above median poverty change × Newcomers, pct. pop.	-0.16	-0.26
	(0.21)	(2.20)
B. Reduced form push factor		
Newcomers, pct. pop.	-0.19***	1.41
	(0.07)	(0.98)
Above median poverty change × Newcomers, pct. pop.	-0.12	-1.08
	(0.25)	(2.51)
Observations	5706	8014
Dep. Var. Below, Mean	0.19	54.45
Dep. Var. Below, Sd	0.47	16.80
Dep. Var. Above, Mean	0.08	56.62
Dep. Var. Above, Sd	0.53	17.43
Inst. Loo, Below, Mean	0.62	0.62
Inst. Loo, Below Sd	0.66	0.66
Inst. Loo. Above, Mean	0.13	0.13
Inst. Loo. Above, Sd	0.23	0.23

Column 1 is the average relative importance of universalist values, taken from Enke (2020). Column 2 is out-migration, calculated as the number of out-migrants (in thousands) divided by county population. We interact the instruments and fixed effects with an indicator of whether the county had an above-median change in the poverty rate during the periods. Sources: Enke, (2020); US Census Bureau: Population Division and Small Area; ACS 5; SAIPE. Regressions are reduced form. Instrument is as described in Section 3. Source: SRE. Standard errors clustered at the CBSA level. Estimations control for county and state-period fixed effects and weighted by predicted population. *p<0.1,**p<0.05,***p<0.01

C.4 Effect on employment and poverty by demographic group

Table C5: First stage for localities (Counties and PUMA)

	(1) LOO	(2) Push Factors
Newcomers, percent population	1.174^{***} (0.062)	1.350^{***} (0.086)

Sources: US Census Bureau: 2011, 2015, and 2019 ACS 5. 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 locality level. All estimations control for locality and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.1,**p<0.05,***p<0.011

Table C6: Effect of arrival of unauthorized Mexican migrants on employment among working age (2010-19)

	Per 1,000 working age people		
	(1)	(2)	(3)
	Construction	Manufacturing	Hospitality and Leisure
A. OLS			
Newcomers, pct pop.	-0.623	1.837**	-1.492**
	(0.560)	(0.901)	(0.609)
B. 2SLS LOO			
Newcomers, pct pop.	-0.173	1.672*	-1.723**
	(0.592)	(0.883)	(0.705)
Std. Coefficient	-0.009	0.025	-0.054
$\hat{\beta} * P(75) - P(25)$	-0.018	0.179	-0.185
C. 2SLS Push Factors			
Newcomers, pct pop.	-0.249	3.079**	-2.473***
	(0.801)	(1.325)	(0.847)
Std. Coefficient	-0.012	0.045	-0.077
$\hat{\beta} * P(75) - P(25)$	-0.027	0.330	-0.265
Observations	1253	1253	1253
Dep. Var., Mean	44.70	68.64	69.13
Dep. Var., Sd	11.55	32.90	19.09
Ind. Var., Mean	0.56	0.56	0.56
Ind. Var., Sd	0.62	0.62	0.62

Dependent variables are the 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 not available. Sources: US Census Bureau: 2011, 2015, and 2019 ACS 5. 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. Standard errors clustered at the locality level. All estimations control for locality and state-period fixed effects and are weighted by predicted population. *p<0.1,**p<0.05,***p<0.01

Table C7: Effect of arrival of unauthorized Mexican migrants on employment among working age population by demographic group (2010-19)

	Construction				Manufacturing				Hosp and leis			
	(1) White> HS	(2) White<= HS	(3) His> HS	(4) His<= HS	(5) White> HS	(6) White<= HS	(7) His> HS	(8) His<= HS	(9) White> HS	(10) White<= HS	(11) His> HS	(12) His<= HS
A. OLS												
Newcomers, pct pop.	0.002*	0.000	-0.005***	0.002	0.005***	0.001	0.003	0.007**	0.001	-0.006**	0.001	-0.006*
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.003)	(0.003)	(0.003)	(0.001)	(0.003)	(0.003)	(0.003)
B. 2SLS LOO												
Newcomers, pct pop.	0.003**	0.001	-0.003	0.006	0.004***	-0.000	0.005	0.008*	0.001	-0.006**	-0.004	-0.007
	(0.001)	(0.002)	(0.002)	(0.004)	(0.001)	(0.002)	(0.004)	(0.005)	(0.001)	(0.003)	(0.004)	(0.004)
Std. Coefficient	0.123	0.025	-0.062	0.045	0.053	-0.000	0.044	0.050	0.019	-0.104	-0.060	-0.061
$\hat{\beta} * P(75) - P(25)$	0.000	0.000	-0.000	0.001	0.000	-0.000	0.001	0.001	0.000	-0.001	-0.000	-0.001
C. 2SLS Push Factors												
Newcomers, pct pop.	0.001	0.002	-0.004*	0.006	0.004**	-0.000	0.005	0.010*	0.002	-0.005	-0.008*	-0.011**
	(0.001)	(0.003)	(0.002)	(0.005)	(0.002)	(0.003)	(0.004)	(0.005)	(0.002)	(0.004)	(0.004)	(0.005)
Std. Coefficient	0.050	0.062	-0.090	0.044	0.051	-0.001	0.038	0.060	0.044	-0.093	-0.107	-0.097
$\hat{\beta} * P(75) - P(25)$	0.000	0.000	-0.000	0.001	0.000	-0.000	0.000	0.001	0.000	-0.001	-0.001	-0.001
Observations	1253	1253	1253	1253	1253	1253	1253	1253	1253	1253	1253	1253
Dep. Var., Mean	0.04	0.10	0.04	0.13	0.08	0.09	0.09	0.12	0.07	0.12	0.09	0.16
Dep. Var., Sd	0.01	0.02	0.02	0.07	0.04	0.05	0.06	0.07	0.02	0.03	0.04	0.06
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

Dependent variables are the people 16-64 from the relevant demographic group who are employed in a particular sector as share of the total labor force (16-64) of that group in that locality. <= HS (up to high school) and HS > (more than high school) are mutually exclusive groups. White people are those who self-identify as white non-Hispanic and are US citizens. Hispanics include only US citizens. The geographic unit of observation is a county, when available in IPUMS, and a (consistent) PUMA, for counties that not available. Sources: US Census Bureau: 2011, 2015, and 2019 ACS 5. 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. Standard errors clustered at the locality level. All estimations control for locality and state-period fixed effects and are weighted by predicted population. *p<0.1,**p<0.05,***p<0.01

Table C8: Effect of arrival of unauthorized Mexican migrants on adult poverty, by demographic group (2010-19)

	Old unaut Mex mig	Hispanic		Old non-Mex Migrant		White	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	> HS	<= HS	> HS	<= HS	> HS	<= HS
A. OLS							
Newcomers, pct pop.	0.048	0.070	0.116***	0.042	0.019	0.067*	-0.005
	(0.049)	(0.043)	(0.038)	(0.055)	(0.058)	(0.040)	(0.037)
B. 2SLS LOO							
Newcomers, pct pop.	0.052	0.062	0.149***	0.023	0.007	0.066	0.020
	(0.064)	(0.051)	(0.045)	(0.064)	(0.076)	(0.045)	(0.039)
Std. Coefficient	0.013	0.019	0.042	0.007	0.002	0.032	0.010
$\hat{\beta} * P(75) - P(25)$	0.006	0.007	0.016	0.002	0.001	0.007	0.002
C. 2SLS Push Factors							
Newcomers, pct pop.	0.005	0.025	0.154***	0.024	0.069	0.037	-0.035
	(0.068)	(0.055)	(0.044)	(0.061)	(0.077)	(0.049)	(0.064)
Std. Coefficient	0.001	0.007	0.043	0.007	0.019	0.018	-0.018
$\hat{\beta} * P(75) - P(25)$	0.001	0.003	0.017	0.003	0.007	0.004	-0.004
Observations	1072	1246	1249	1221	1206	1253	1253
Dep. Var., Mean	7.83	8.55	9.24	7.73	7.89	9.28	8.91
Dep. Var., Sd	2.22	1.81	1.94	1.81	1.94	1.12	1.01
Ind. Var., Mean	0.59	0.56	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	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. <= HS (up to high school) and More than HS (more than high school high school) are mutually exclusive groups. The sample for column 1 are likely unauthorized Mexican migrants who have been in the US for more than 5 years. The sample for columns 4 and 5 are people born outside the US, not in Mexico, 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 includes only US citizens. The geographic unit of observation is a county, when available in IPUMS, and a (consistent) PUMA, for counties that not available. Sources: US Census Bureau: 2011, 2015, and 2019 ACS 5. 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. Standard errors clustered at the locality level. Estimations control for locality and state-period fixed effects, and are weighted by predicted population. *p<0.1,**p<0.05,***p<0.01

D Heterogeneity analysis

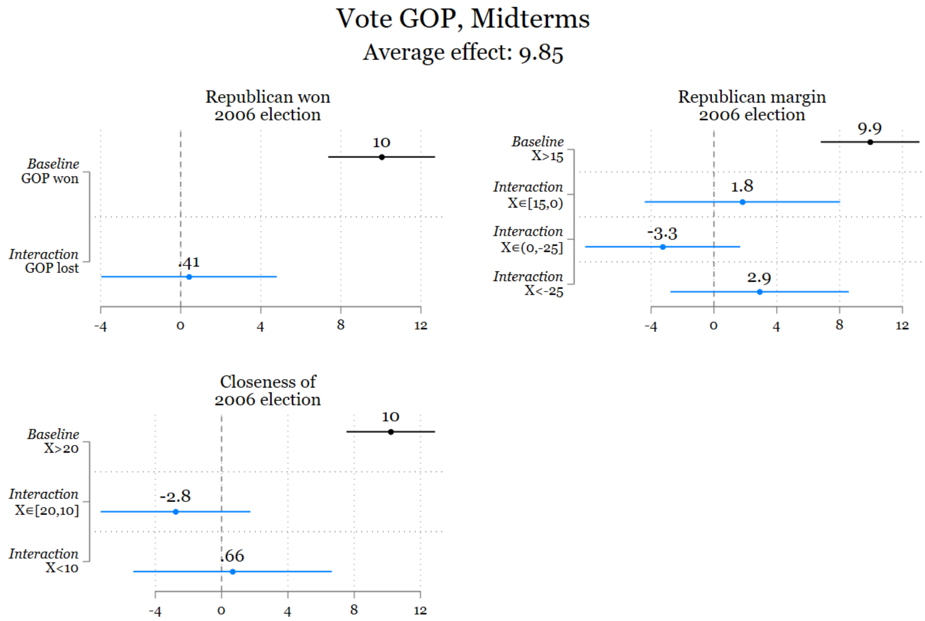

Figure D1: Effects of recent newcomers on the vote share for the Republican party in midterm elections by pre-period political behavior.

The first graph presents the differential effect of predicted newcomers based on whether the GOP won the county's vote in the 2006 House election. The baseline category (black line) consists of counties where the GOP won, and the blue line displays the interactive effect in counties where the GOP lost. The second picture presents the differential effect based on the margin of the GOP in the 2006 House election. The baseline category (black line) comprises counties where the GOP won by over 15 percentage points. The first group consists of counties where the GOP won by less than 15 pp. The second group consists of counties where the Democratic party won by less than 25 pp. The third group comprises counties where the Democratic party 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 won by more than 20 pp. The first group consists of counties where either party 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 90% confidence interval.

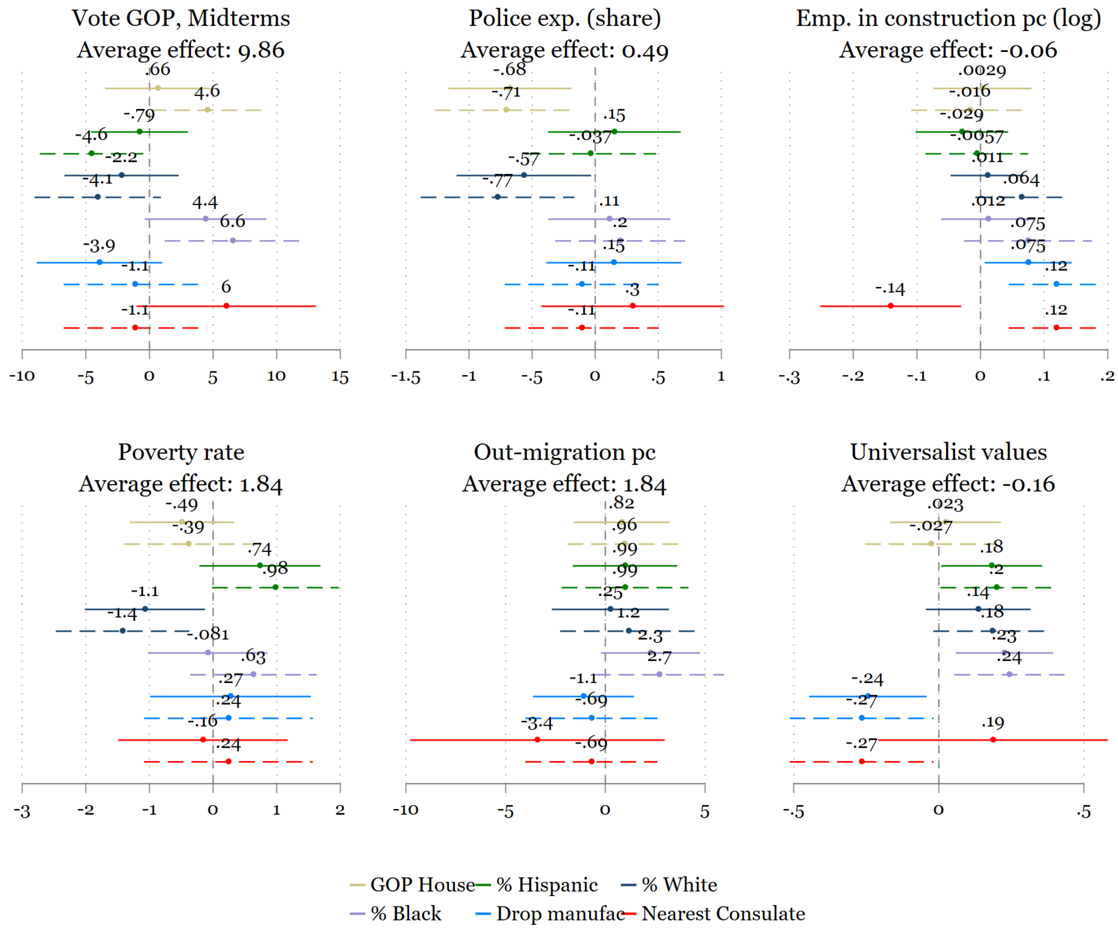

Figure D2: Heterogeneity

Displayed are the 90% coefficient intervals of the interaction between the instruments and dummy indicating above median values of: vote share for the GOP in the House midterm elections of 2006, share of adult 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; US Census Bureau: 2007-2011, 2011-2015, and 2015-2019 ACS 5; QCEW; SAIPE; US Department of Commerce: Bureau of Economic Analysis; Annual Survey of State and Local Government Finances.

D.1 Heterogeneous effects by tax progressivity/safety nets

Table D1: Main effects and mechanisms by ratio of revenue generates by sales and property tax to own revenue

	Vote share	Share of Expend	Emp (log)	Log	Rate	Values
	(1) GOP House	(2) Education expenditure	(3) Construction employment	(4) Poverty rate	(5) Out migration	(6) Universalist values
	A. Reduced Form LOO					
Instrument	11.82***	0.88***	-0.11***	2.39***	0.94	-0.24***
	(1.67)	(0.30)	(0.04)	(0.29)	(1.15)	(0.08)
Sales and Property, county=1 × Instrument	-3.18	-0.59	0.10**	-1.32***	1.66	0.16
	(2.50)	(0.39)	(0.05)	(0.49)	(1.62)	(0.11)
	B. Reduced Form Push factors					
Instrument	13.64***	0.93***	-0.12***	2.73***	0.26	-0.34***
	(1.77)	(0.34)	(0.04)	(0.32)	(1.34)	(0.09)
Sales and Property, county=1 × Instrument	-5.55**	-0.75*	0.14**	-1.57***	2.66	0.21
	(2.80)	(0.45)	(0.06)	(0.54)	(1.89)	(0.13)
Observations	7977	5330	7364	8001	7999	5690
Dep. Var., Mean	49.00	5.45	-3.61	14.24	55.44	0.14
Dep. Var., Sd	18.77	1.85	0.44	5.22	17.09	0.49

Column 1 is the share of GOP vote in midterm House elections. Column 2 is the share of total direct expenditures in the police. Column 3 is the log of per working age employment rate in construction. Column 4 is poverty rate. Column 5 is the the number of out-migrations by 1,000 people. Column 6 is the prevalence of universalist values following Enke (2020). We interact the corresponding instruments and fixed effects 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. Above suggests a more progressive fiscal policy. This criteria varies at the county-level. Sources: Enke, (2020); US Census Bureau: Population Division and Small Area; ACS 5; QCEW; SAIPE; 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. 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: Main effects and mechanisms by state tax inequality

	Vote share	Share of Expend	Emp (log)	Log	Rate	Values
	(1)	(2)	(3)	(4)	(5)	(6)
	GOP House	Education expenditure	Construction employment	Poverty rate	Out migration	Universalist values
A. Reduced Form LOO						
Instrument	9.10***	0.69***	-0.08***	2.33***	0.87	-0.23***
	(1.35)	(0.21)	(0.02)	(0.23)	(0.86)	(0.07)
Tax equality index, state=1 × Instrument	2.06	-0.48	0.06	-1.32**	2.63*	0.18*
	(2.68)	(0.32)	(0.04)	(0.56)	(1.41)	(0.10)
B. Reduced Form Push factors						
Instrument	10.29***	0.78***	-0.10***	2.66***	0.53	-0.29***
	(1.56)	(0.23)	(0.03)	(0.24)	(1.03)	(0.08)
Tax equality index, state=1 × Instrument	0.27	-0.74*	0.11**	-1.49**	3.22**	0.19
	(3.30)	(0.39)	(0.06)	(0.63)	(1.61)	(0.12)
Observations	7995	5334	7385	8019	8017	5712
Dep. Var., Mean	48.16	5.45	-3.62	14.36	55.16	0.15
Dep. Var., Sd	19.44	1.85	0.44	5.31	17.00	0.50

Column 1 is the share of GOP vote in midterm House elections. Column 2 is the share of total direct expenditures in the police. Column 3 is the log of per working age employment rate in construction. Column 4 is poverty rate. Column 5 is the the number of out-migrations by 1,000 people. Column 6 is the prevalence of universalist values following Enke (2020). We interact the corresponding instruments and fixed effects with an indicator of whether the county is in a state that, according to the Institute for Taxation and Economic Policy (Davis et al., 2009), in 2007 was above or below the median of an idex of tax equality. 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; ACS 5; QCEW; SAIPE; 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. 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 D3: Main effects and mechanisms by state tax inequality

	Vote share	Share of Expend	Emp (log)	Log	Rate	Values
	(1) GOP House	(2) Education expenditure	(3) Construction employment	(4) Poverty rate	(5) Out migration	(6) Universalist values
		A. Reduced Form LOO				
Instrument	9.38***	0.63***	-0.08***	2.10***	0.91	-0.20***
	(1.39)	(0.22)	(0.02)	(0.24)	(0.84)	(0.06)
TANF/poverty rate,	1.29	-0.34	0.06	-0.69	2.55*	0.13
state=1 × Instrument	(2.64)	(0.31)	(0.04)	(0.60)	(1.45)	(0.11)
		B. Reduced Form Push factors				
Instrument	10.75***	0.69***	-0.10***	2.40***	0.90	-0.27***
	(1.60)	(0.24)	(0.03)	(0.24)	(1.00)	(0.07)
TANF/poverty rate,	-0.89	-0.53	0.11**	-0.82	2.27	0.13
state=1 × Instrument	(3.20)	(0.39)	(0.06)	(0.69)	(1.64)	(0.12)
Observations	7995	5334	7385	8019	8017	5712
Dep. Var., Mean	48.16	5.45	-3.62	14.36	55.16	0.15
Dep. Var., Sd	19.44	1.85	0.44	5.31	17.00	0.50

Column 1 is the share of GOP vote in midterm House elections. Column 2 is the share of total direct expenditures in the police. Column 3 is the log of per working age employment rate in construction. Column 4 is poverty rate. Column 5 is the the number of out-migrations by 1,000 people. Column 6 is the prevalence of universalist values following Enke (2020). We interact the corresponding instruments and fixed effects with an indicator of whether the county is in a state that, according to the Institute for Taxation and Economic Policy (Davis et al., 2009), in 2007 was above or below the median of an idex of tax equality. 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; ACS 5; QCEW; SAIPE; 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. 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 D4: Main effects and mechanisms by state Tanf-poverty ratio

	Vote share	Share of Expend	Emp (log)	Log	Rate	Values
	(1)	(2)	(3)	(4)	(5)	(6)
	GOP House	Education expenditure	Construction employment	Poverty rate	Out migration	Universalist values
		A. Reduced Form LOO				
Instrument	12.53***	0.29	-0.05*	2.47***	-0.47	-0.41***
	(1.35)	(0.25)	(0.03)	(0.32)	(1.28)	(0.08)
Minimum wage,	-4.19**	0.30	-0.02	-0.98**	3.63**	0.40***
state=1 × Instrument	(2.12)	(0.33)	(0.04)	(0.49)	(1.51)	(0.10)
		B. Reduced Form Push factors				
Instrument	13.80***	0.39	-0.07**	2.80***	-1.30	-0.51***
	(1.79)	(0.24)	(0.03)	(0.32)	(1.62)	(0.10)
Minimum wage,	-5.29**	0.15	0.01	-1.13**	4.82***	0.44***
state=1 × Instrument	(2.59)	(0.37)	(0.05)	(0.55)	(1.86)	(0.12)
Observations	7995	5334	7385	8019	8017	5712
Dep. Var., Mean	48.16	5.45	-3.62	14.36	55.16	0.15
Dep. Var., Sd	19.44	1.85	0.44	5.31	17.00	0.50

Column 1 is the share of GOP vote in midterm House elections. Column 2 is the share of total direct expenditures in the police. Column 3 is the log of per working age employment rate in construction. Column 4 is poverty rate. Column 5 is the the number of out-migrations by 1,000 people. Column 6 is the prevalence of universalist values following Enke (2020). We interact the corresponding instruments and fixed effects with an indicator of whether the county is in a state that in 2005-06 was above or below the median Tanf to poverty ratio (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; ACS 5; QCEW; SAIPE; 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. 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

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