The Pearson Institute Discussion Paper No. 2023-2

The Local Reaction to Unauthorized Mexican Migration to the US

Kara Ross Camarena

Ernesto Tiburcio

THE PEARSON INSTITUTE
FOR THE STUDY AND RESOLUTION OF GLOBAL CONFLICTS

The Local Reaction to Unauthorized Mexican Migration to the US*

Ernesto Tiburcio† Kara Ross Camarena‡

Current version: April 5, 2023
First version: October 2022

The latest version is available here. The appendix alone is available here.

Abstract

We study the political impacts of unauthorized Mexican migration to the United States. Our identification strategy relies on exogenous variation in Mexican municipalities and migrant networks from data on over 7 million likely unauthorized migrants who obtained consular IDs. We find evidence of conservative electoral and policy responses. Unauthorized migration significantly increases the vote share of the Republican Party in federal elections, decreases spending on education, and increases relative spending on policing and on the administration of justice. Among the mechanisms we explore, job loss in "migrant intensive" sectors and an increase in poverty best explain the political reaction. We also document subsequent out-migration and heightened in-group values among US natives. By contrast, unauthorized migration inflows have no discernible impact on wages, unemployment, or crime rates. We find suggestive evidence the main political impacts of unauthorized migration, and their drivers, are smaller in counties that have more progressive taxation or a more generous social safety net.

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

JEL Codes: D72, J61, J15

---

*We thank Dan Black, Steven Block, Ryan Enos, Kyle Emerick, Jeff Grogger, Eric Hansen, Cynthia Kinnan, Agustin Markarian, Margaret McMillan, Juan Pereira, Adam Storeygard, Marco Tabellini and Shinsuke Tanaka 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. All errors remain ours.

†Corresponding Author, Job Market Candidate, Tufts University. email: ernesto.tiburcio_manon@tufts.edu

‡Assistant Professor, Loyola University Chicago. email: kara@rosscamarena.com

1 Introduction

Political backlash of citizens against migrants from developing countries is well documented (Alesina and Tabellini, 2021; Rodrik, 2021; Rozo and Vargas, 2021; Mayda et al., 2022a).[1] While not new (Tabellini, 2020; Alsan et al., 2020), this backlash has gained attention because of negative media bias (Couttenier et al., 2021) and its connection to the rise of right-wing populism (Edo et al., 2019; Halla et al., 2017; Barone et al., 2016; Harmon, 2018; Otto and Steinhardt, 2014). Most contemporary evidence comes from Europe, focuses on refugees, and is limited to electoral responses and civic attitudes. There is less information about the contemporary context in the United States.

This study expands on the literature by estimating US citizens' reactions to unauthorized Mexicans, a group with theoretical and empirical import. Mexican migrants to the United States make up the world's largest diaspora in a single country. As of 2020, nearly 11.4 million Mexican-born persons lived in the US, and an estimated 4.5 million of them had irregular migration status (Passel and Cohn, 2018; Gonzalez-Barrera, 2021; United Nations Department of Economic and Social Affairs, 2021). Their interactions with their communities of residence are unlike those of regular migrants, transient migrants, or refugees; they actively participate in the US economy but have limited political effects, which puts them in a state of permanent legal vulnerability. Unauthorized Mexican migrants have been at the center of US political debates at the national and local levels.

There is a rich literature documenting the labor market impacts of this migration, which are small and limited to a few sectors (Peri and Sparber, 2009; Hanson, 2009; Monras, 2020; Blau and Mackie, 2017). However, there is little systematic evidence of their political and social impacts. We fill this gap by exploring the effect of unauthorized migration on voting behavior and public expenditure. Then we explore the economic, political, and social channels behind these responses. We document how the arrival of unauthorized migrants has prompted voting in favor of the Republican party. The political shift is driven by job

[1] However, see Hill et al. (2019), who document less reactionary behavior.

loss, increases in poverty, and value changes in the absences of a social safety net.

Selection bias is a central challenge to estimating political and social effects of unauthorized migrants. Mexican migrants, like any migrants, choose particular locations because of political and social context, making cross-section comparisons biased. Potential bias is compounded by the lack of appropriate data. There are no direct records of newly arrived unauthorized Mexican migrants to the US. The literature has relied on survey data like ACS (Borjas and Cassidy, 2019)—which proxies unauthorized migrants with education attainment—or apprehensions at the border (Hanson and Spilimbergo, 1999) to capture unauthorized migration. These samples, however, are unlikely to be representative. Unauthorized migrants are not necessarily unschooled. Further, they may avoid US surveys for fear of legal consequences. Those who are apprehended at the border, moreover, may differ systematically from those who are not. To address data concerns, several recent articles have used an administrative data set on consular identification cards issued by the Mexican government to nationals living in the United States (Allen et al., 2018; Caballero et al., 2018; Bhandari et al., 2021; Dinarte Diaz et al., 2022). While the data set does not explicitly sample unauthorized migrants, consular IDs are useful exclusively for unauthorized migrants—as we demonstrate in Section 3.1.3. An additional advantage of this data is that its geographic detail provides a way to address selection bias and predict current day migrant flows by exploiting past networks.

We use a confidential version of this data set with the crucial feature of being able to track individuals over time. Our data contains information on Mexican municipality of origin, US county of residence, gender, age, marital status, and educational attainment for 7.4 million people (14.5 million observations) from 2002 to 2020. Granular variation in time and geography, along with the stable migration patterns documented elsewhere (Durand et al., 2001; Munshi, 2003), allows us to predict Mexican migration as a fraction of county population. We develop two different shift-share approaches. In our preferred specification, we use an instrument for migration with a leave-one-out shift-share strategy. Our shares come from the proportion of migrants from each Mexican municipality living in each US county. We predict inflows from each Mexican municipality, net of those migrants who established residence in the core-based statistical area (CBSA) of each county; this is the leave-one-out component. A second, more demanding specification uses the same initial county-municipality shares, but predicts migration flows using time-varying municipality characteristics. Both of these approaches leverages the geographic variation in the consular data to predict a measure of migrant arrivals that is otherwise exogenous to the social and political changes in the US counties of interest.

Our identifying assumption is that the predicted number of migrants impacts the outcomes of interest only by its effect on observed migration. Given that we exploit within county and state-period variation, we assume that the US county-level characteristics that attracted Mexicans from particular municipalities in earlier decades do not affect the evolution of economic, political, and social characteristics of the county today. Despite reflecting historic migration patterns, we expect that our initial shares are unrelated to the evolution in our outcomes of interest (Goldsmith-Pinkham et al., 2020; Borusyak et al., 2022). We claim that the shifters used in both strategies—leaving a CBSA out or predicted migration from Mexican municipality characteristics—are exogenous to the trajectory of the outcomes of interest, resulting in exogenous instruments. We interrogate this assumption by looking for pre-trends and differential trends. To account for the possibility of non-random exposure to migration, we implement the correction proposed by Borusyak and Hull (2020). The results are robust to a number of our attempts to examine the identifying assumption. Moreover, the consistency between the results obtained with the two instruments reinforces the plausibility of our identifying assumption.

Our primary study is the effect of unauthorized migration on vote shares in House and presidential elections between 2010 and 2020. We find that unauthorized migration increases the vote share for Republicans across House and presidential elections. The effect is largest for midterm House elections. In our main specification, a mean inflow of newcomers (0.4

percentage points of the county population) increases the Republican vote share in midterm House elections by 3.9 percentage points, an impact large enough to alter some elections. Our results reinforce that US voters have a heightened response to unauthorized migrants, similar to Mayda et al. (2022a) and Baerg et al. (2018).[2]

We also explore whether migration also alters local policy. We estimate the impact on local public goods expenditure. Following Alesina et al. (1999), Hanson et al. (2007), Facchini and Mayda (2009), Card et al. (2012), Hainmueller and Hopkins (2014) and Alesina et al. (2022), we expect that larger flows of migrants will decrease the provision of public services due to coordination failures and out-group bias. Indeed, we find that migration reduces absolute expenditures in education and increases absolute and relative expenditures in policing and on the administration of justice. This finding is consistent with Derenoncourt (2022), who documents similar effects following the Great Migration. In our main specification, a mean inflow of newcomers (0.4 percent of the county population) decreases the expenditures in education by 3% and increases the share on policing and the administration of justice by 0.23 and 0.15 percentage points, respectively. Unauthorized migration alters public spending consistent with the conservative, law-and-order shift in voting.

Following on our conservative electoral and policy findings, we explore prominent explanations. We document labor market impacts in migrant-intensive industries and important losses at the bottom of the income distribution. In our main specification, a mean flow of newcomers (0.4 percent of the county population) decreased employment in construction by 2% and employment in hospitality and leisure by 1%. Some of the job loss in these industries is offset by job gains in manufacturing. Migration also increased the poverty rate by 4%. We find quantitative and qualitative changes in the electorate. As a response to migration, counties adopt less universalist (more communal) moral values—standardized coefficient of 0.16. That is, people are less supportive of ideas and policies that benefit everyone, consis-

[2] Mayda et al. (2022a) find a result of similar magnitude, also exploiting migrants' historic networks. A one percentage point increase in the share of "low-skilled" migrants increased the vote share for the Republican Party in all federal elections between 1990 and 2016 by 4.56 percentage points.

tent with right-leaning preferences (Enke et al., 2020). We show that counties with more migrants observe a decline in total population, 1%, and an increase in out-migration, 2%, with a mean flow. We find no consistent evidence of impacts on crime, GDP per capita, median household income, unemployment or total employment.

Finally, having found that prior political context does little to explain shifts in voting and the channels we investigate, we explore an alternative explanation. We examine how redistribution can mitigate the losses associated with unauthorized migrant arrivals. We compare the impacts of unauthorized newcomers in counties that at the beginning of our period of study had more progressive tax structure (ratio of income tax vs. sales tax revenue). We observe the effects tend to be more concentrated in counties with more regressive taxation and a less robust social safety net. Our interpretation is that these counties do not sufficiently compensate the economic losers.

We advance the understanding of migration and political effects in several ways. We provide causal estimates of the local electoral and fiscal responses to a subset of the largest diaspora in the world, and one of the most politicized groups of migrants in the US. Previous studies have measured the impacts of "low-skill" vs. "high-skill" migration (Mayda et al., 2022a). However, to our knowledge, none has specifically studied the country-wide impact of unauthorized migration. We document the different channels of these political responses. The mechanisms are relevant by themselves, as they portray a more holistic picture of the newcomers in their communities of arrivals. Our results help to identify losers from this particular migration and suggests policy that may help to compensate them. We take on the suggestion of Alesina and Tabellini (2021) and specifically study how migration shapes moral values. We demonstrate that value change is an important explanation for the political effects we observe. Last, the estimated impacts for the political effects are considerably larger than those of the mechanisms. This research suggests that the political reaction is not fully explained by the broad set of mechanisms explored.

In the remainder of the paper, we describe the background of Mexican migration and review existing theory for how migrants influence politics and policy (Section 2). We present our novel dataset and show how it is apt for our question of interest (Section 3). We explain our shift-share instruments and examine the key identifying assumptions (Section 4). We demonstrate that flows of migration drive an increase in vote share for the Republican party and shift public spending consistent with fiscal conservatism (Section 5). We perform several robustness checks (Section 6). In Section 7, we explore mechanisms that explain the rightward shift and highlight concentrated losses, demographic change, and a change in values among the electorate. Finally, in Section 8 we explore heterogeneous effects across fiscal structures and discuss implications.

2 Background

2.1 The Political Effects of Immigration

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

An extensive literature describes the demography of this population and analyzes the labor market impacts. Wassink and Massey (2022) describe the policy context and demographic consequences of the post-2000 immigration regime. While the number of new unauthorized Mexican migrants has declined in recent years, large flows still persist. Wage differentials between the US and Mexico remain a compelling explanation for the flows (Hanson and Spilimbergo, 1999). In the labor market, scholars have found null overall impacts on wages and on unemployment, but small negative impacts in certain sectors and regions (Hanson, 2009; Monras, 2020; Clemens et al., 2018; Blau and Mackie, 2017). Nevertheless, the political and social effects of these flows have not been widely analyzed.

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

2.2 Explaining Political Shifts

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

The second explanation relates to heterogeneity. By virtue of their otherness, migrants may trigger out-group responses. The hypothesis is that migrants' otherness prompts exclusionary attitudes, potentially offsetting more welcoming attitudes arising from increased contact (Enos, 2014). The reasons behind this may be economic, since natives would prefer lower redistribution to ethnically different people (Alesina et al., 1999; Alesina and Giuliano, 2009). They might be demographic, since natives want to preserve the current composition of their communities (Card et al., 2012). Last, they might be political, since natives may want to preserve their power in a polarized environment (Bazzi et al., 2019). Flows of unauthorized migrants would make it harder to deliver public goods, either because coordination is more difficult or because preferences for redistribution change. In the context of US democratic institutions, this may be reflected in voters' preferences for policies associated with the Republican party: lower taxes and lower government spending.

The third explanation has to do with attitudes and perceptions. Natives, driven by political entrepreneurs or the media (Couttenier et al., 2021), may assign negative characteristics to migrants (Hainmueller and Hopkins, 2014; Alesina et al., 2022; Facchini and Mayda, 2009). Negative perceptions may include the belief that migrants threaten natives, increase crime, or do not contribute economically to their place of residence. These attitudes lead citizens to vote for anti-migrant politicians and policies. Rozo and Vargas (2021) demonstrate how politicians can use these (mis)perceptions of migrants strategically to gain office, and Abrajano and Hajnal (2017) explore how the media can enhance these negative perceptions of immigrants. The logic of why natives acquire these negative associations is similar to a class of explanations for immigrant backlash in political and social psychology driven by intergroup threat (Riek et al., 2006). These threats may be economic, social, or cultural. Of particular importance is the finding in this literature that economic vulnerability further enhances the perception of threat (See Dustmann et al. (2019) for a discussion).

3 Data

3.1 Consular Data

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

3.1.1 Data Context

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

The Mexican government, therefore, has data on the municipality and date of birth, marital status, educational attainment, sector of employment, and US county and state of residence of cardholders. The National Institute of Mexicans Abroad (IME) intermittently publishes aggregated versions of this data.[3] However, the aggregated dataset does not show specific people, nor does it allow construction of Mexican municipality-US county pairs. The Mexican Ministry of Foreign Affairs (SRE) has shared with us a confidential, detailed version of the dataset. It contains all the demographic information, except name, of every Mexican national who got an ID between 2002 and 2020. The SRE created an identification number that allows us to track people over time. This number has no relevant meaning nor is it linked in any form to other demographic information. The data consists of 16.7 million observations corresponding to 8.8 million individuals.

3.1.2 Constructing a Measure of Newcomers

There are two main challenges to estimating the number of newcomers from our data. It is necessary to (1) differentiate between renewals and first-timers and (2) to make assumptions about the likelihood that an average newcomer applies for an ID and remains in the same county. We have taken the following approach. First, we count the number of new cards per person in 4-year intervals, 2007–10, 2011–14, and 2015–18. [4] This frequency is convenient because it allows us to observe the compounded number of migrants during an election year. Moreover, there is evidence that cardholders tend to be newly arrived migrants, that the majority of newly arrived Mexican migrants obtain a card over a 5-year period, and that cardholders tend to remain at least in the same state over those five years (Allen et al., 2018; Caballero et al., 2018; Massey et al., 2015). Second, for each one of these periods, we classify as newcomers the people who got an ID for the first time in a new core-based statistical area (CBSA), a geography that encompasses several counties. The strategy, as opposed to counting solely the people who got an ID for the first time, considers migrants moving from one CBSA to another as newcomers. Third, we count only the observations with complete and consistent information regarding place of birth and county of residence. We estimate

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

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

that there were 2.13 million newcomers in 2010, 1.3 in 2014, and 0.95 in 2018. As Appendix B shows, these figures are consistent with Passel and Cohn (2018). Unauthorized Mexican migration has decreased consistently since its peak in the early 2000s.

To calculate the fraction that the migrants represent in every county, we divide the total number by the county population in the final year of the period—e.g., 2010, 2014, and 2018—using population estimates from the US Census. Figure 1 shows the national distribution of the fraction of unauthorized Mexican migrants for each period. The average is 0.69% for the first period, 0.4% for the second, and 0.28% for the third. During the time of our study, there were at least 10 migrants in 2,674 US counties,[5] around 88% of all US counties.

Recent unauthorized Mexican migrants as share of county population

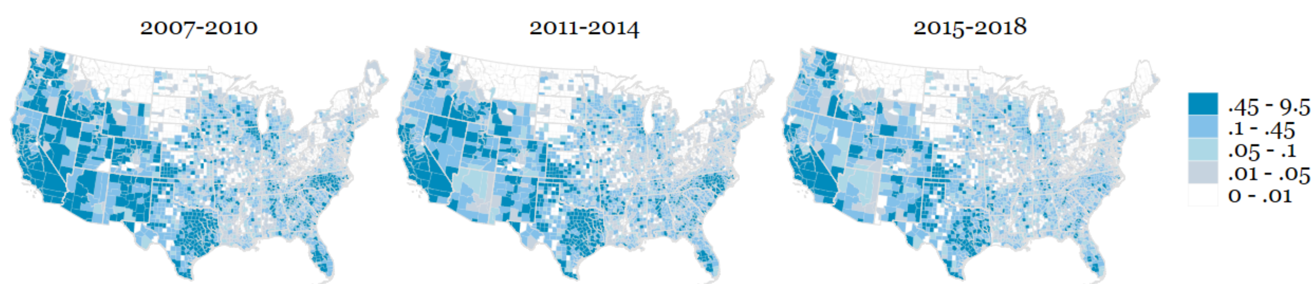

Figure 1: Map of observed number of newcomers. Sources: SRE and US Census Bureau, Population Division

3.1.3 Evaluating the Consular ID Data

There is evidence that the consular dataset captures unauthorized migrants well. Caballero et al. (2018) compare the log number of cards issued in each state between 2006 and 2010 with the log estimated number of Mexican-born residents obtained from the 2010 and 2011 American Community Surveys (ACS). Their R^2 is over 0.97. We carry out a similar analysis using ACS 5 2006–10, 2010–14, and 2014–18 (Ruggles et al., 2022). Following Allen et al. (2018), we consider a likely unauthorized newcomer Mexican migrant in the ACS 5 those

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

people born in Mexico, with no US citizenship, with no college education, and who have been in the US for less than 4 years. Figure 2 plots the log of likely unauthorized migrants from our data and two repositories of ACS-5. The left panel uses data for the 2,674 counties covered by AC5, in the Social Explorer, and our data [6]. The right panel uses data for the 441 counties covered by ACS 5, in IPUMS Ruggles et al. (2022), and in our data. Our correlation coefficients are 0.85 and 0.82, respectively. The association is considerably weaker in areas with few migrants, probably due to low precision from the Social Explorer data. Further, we compare key demographic variables of 441 counties covered by ACS 5 (IPUMS) and the consular data and find no significant differences (see Appendix C). We conclude from these efforts that our data is capturing the unauthorized Mexican population well.

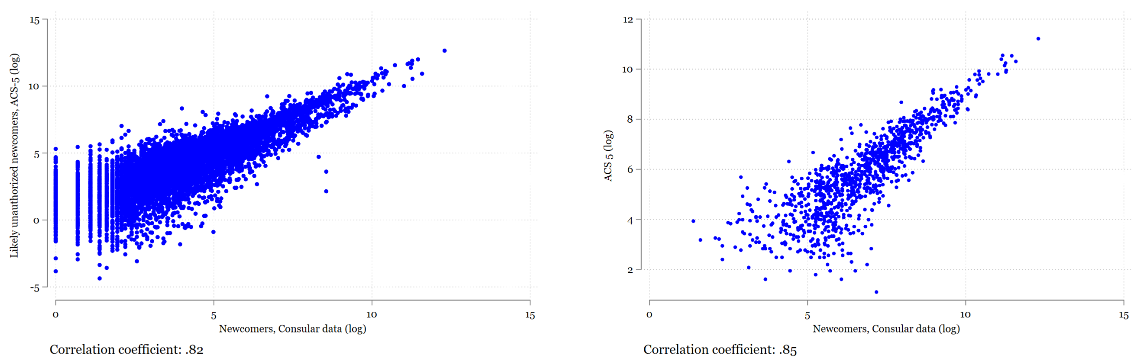

Figure 2: Correlation between ACS 5 and Consular Data

Another potential concern is that, even if the consular data is representative of unauthorized migrants, it may also be representative of authorized migrants. If that were the case, it would be hard to test whether the effects that we observe are due to authorized or unauthorized migration. We explore this possibility. Using the detailed data from the 441 counties covered by from ACS 5 (IPUMS), we regress the estimate of unauthorized Mexican migration described before and an analogous estimate of authorized migration—constructed following Allen et al. (2018)—on our preferred instrument. We find a weak relationship

[6] 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.

(see Appendix E). In short, our instrument results in a strong first stage for unauthorized migrants but a weak, and not significant, first stage for authorized migrants. This exercise gives us confidence that we are not conflating unauthorized and authorized migrants in our study.

Selection bias at the county level is the main threat to the validity of this data as a metric of unauthorized migration flows across counties and years. The potential problem is that unauthorized migrants in counties that change their policy environments could have a stronger (or weaker) incentive to request an ID. Our assumption is that migrants get consular IDs to access basic services, like banking or housing, and to send remittances to Mexico, almost regardless of the policy environment in their county of residence. We test this assumption by observing the evolution of IDs after some states made driver's licenses and non-driver IDs available to unauthorized migrants. As of 2018, 12 states and the District of Columbia allowed unauthorized migrants to get a diver's license (NCSL Immigrant Policy Project, 2021), as compared with only 3 before 2012. Following Callaway and Sant'Anna (2021), we implement an event-study to test whether states that modified their regulations observed an uptick in consular cards issued. Figure 3 shows the evolution of consular ID take-up by quarter from 2013 to 2016. A jump lasting three quarters, a time frame much shorter than what we are studying, starting before the policy went into effect, is evident. The "pre-treatment effect" is probably explained by the announcement of the program, whereas the short-lived "post-treatment effect" suggests that individuals may have waited a little longer to get their consular ID, until after the states' policies were implemented. Therefore, the evidence suggests that within our units of analysis—4 years—policy changes do not consistently alter selecting into our dataset. Furthermore, our specification controls flexibly for state by period fixed effects, so within-period shocks will not bias our results.

We carry out an analogous exercise after the activation date of Secure Communities, a program where local police submit individuals to federal authorities for deportation review. We follow East et al. (2022) and Alsan and Yang (2019), and we fail to observe significant

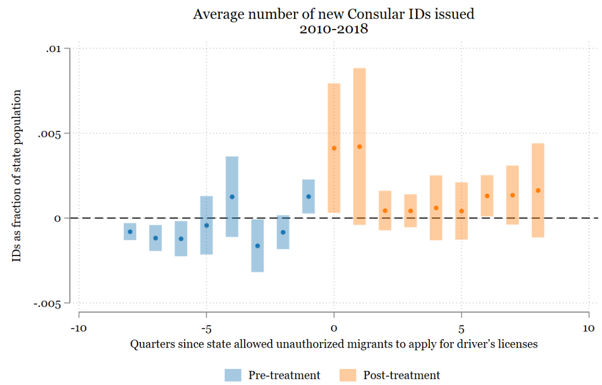

Figure 3: Effect of driver's license regulation on demand for Consular IDs

changes (see Appendix D). Collectively, the results from the driver's license and Secure Communities studies suggest that demand for consular IDs is rather inelastic to the local policy environment in the medium term. Getting a consular ID is not only important to carry out regular tasks in everyday life, but also a common, almost habitual task that unauthorized migrants do.

3.2 Dependent Variables

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

3.2.1 Voting Behavior

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

2018. For presidential year elections, we analyze 2012, 2016, and 2020.

We estimate midterm and presidential years separately because they may provide leverage on different quantities. Midterm election coincide with the end of our periods, whereas presidential year elections occur two years later. Midterm and presidential years could reflect different timing of the effect. Presidential elections are often influenced by international and national issues, up and down the ticket. In these years, turnout from the center is higher, and so presidential years reflect the behavior of the whole electorate, while midterm House elections are more distinctly partisan. Finally, somewhat novel in the presidential elections we study, the Democratic candidate won the majority of the vote. Meanwhile in two of the three midterm elections, Republicans made gains in the House. While we had no *a priori* expectation about how unauthorized migrants might affect these elections differently, it is clear that they are contextually different for both theoretical and empirical reasons.

3.2.2 Public Expenditures

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

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

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

4 Empirical Strategy

A simple comparison between counties with more and fewer unauthorized migrants would provide biased estimates. The number of migrants that counties receive is not random. For example, migrants may select into places that are more economically promising or more friendly toward migrants. To address this bias, we use two shift-share strategies that differ with respect to the estimation of the shifters. Our main strategy uses, in the spirit of Tabellini (2020), a leave-one-out approach, in which we predict migration flows from Mexican municipalities, excluding the flows to our county of interest. Our second strategy predicts migration flows from every Mexican municipality using time-varying push factors. Both of these strategies interact such migration flows with pre-period shares.

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 the predicted unauthorized Mexican migrants as share of predicted population. ψ_c are county fixed effects and η_{st} are state-period fixed effects.

Equation 2 is the first stage of this estimating equation, also 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, and 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, the way we described in Section 3.1.2. We count the unique new consular IDs in every US county during each of the three 4-year periods 2007–10, 2011–14, and 2015–18.[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 that got a consular ID in every county C in this five-year period—following the same rejection rule regarding the CBSA duplication. We decompose this total number of migrants by county according to their municipality of origin M in Mexico. Migrants from our sample come from 2,449 municipalities, over 99% of the total. Then, we add up the migrants from each municipality living in all US counties during that period. Finally, we calculate the share of those migrants from municipality M that lived in each US county C. Thus, our initial shares are the proportion of migrants from municipality M who live in county C. For example, we counted 585 people from Alvarado, Veracruz in the US from 2002–2006. Among them, 9.2% lived in Los Angeles County, CA, 7.5% in Ventura County, CA, and 5.8% in Milwaukee County, WI.

For the leave-one-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 that entered the US during that period, net of those that eventually settled in that county's CBSA. This is

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

the leave-one-out component. There are a few counties that do not belong to any CBSA. For those, we only leave out the county itself. The product of the initial share and the new flow, leaving out the CBSA, is our leave-one-CBSA-out shift-share. For example, we count 550 people from Alvarado in the US between 2007 and 2010; 52 settled in Los Angeles' CBSA, 21 in Ventura's and 93 in Milwaukee's. Thus, the predicted migration in each county is 46 (0.092×(550–52)), 39.8 (0.075×(550–21)), and 26.6 (0.058×(550–93)).

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

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

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

For the push factor strategy, we predict the observed number of migrants from Mexican municipality m during each 4-year period t using four types of time-varying variables: historic and contemporaneous climate and precipitation, from University of Delaware; infant, child, and maternal deaths, as well as homicide rates, from the Mexican Statistical Agency (INEGI); poverty and several other social development indicators, from the National Council for the Evaluation of Social Development Policy (CONEVAL), and indicators of economic activity, like number of economic units or total production, from the Economic Census. 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 H descibes 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, thus, 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 formally

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

where all the terms are just as in equation 3. Following the example above, we predict 781.3 people from Alvarado in the US between 2007 and 2010. 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).

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

4.1 Identifying Assumptions

To provide causal estimates, at least one of the components of shift-share designs must be exogenous (Borusyak et al., 2022). Given that we have panel data, and exploit only within-county variation, the exogeneity of the shift-share in our setting relates to changes, rather than levels. We argue that, while our initial shares (fraction of people from Mexican municipality M living in US county C) reflect historic linkages between US cities/counties and Mexican municipalities/states (Durand, 2016), they are likely exogenous to our variables of interest. We also claim that the shifters are exogenous; our assumption is that by excluding the CBSA of the county of interest or by using Mexican municipality push factors, the constructed shock is uncorrelated with any unobserved factors in the residuals. For the leave-one-out instrument, a key component of this assumption is that the number of migrants are not spatially correlated among CBSAs (Borusyak and Hull, 2020).[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; Cunningham, 2021). We assume that the observed effects in the variables of interest are solely due to the instrument via the endogenous variable. The argument is that, conditional on county and state by period fixed effects, predicted migration affects the evolution of electoral and policy outcomes only through observed migration.

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

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

[8] The potential for spatial correlation is the reason we leave out the entire CBSA, not only the county itself. In Appendix I we show that, while there is some spatial auto-correlation in the number of newcomers among counties (Moran's I of between .44 and .3), the correlation among CBSAs is significantly lower (Moran's I of between .21 and .18). Moreover, as Table 4 indicates our results are robust to including a spatial lag.

We conduct the following checks to provide evidence against both these concerns in our main specification. First, we test for pre-trends by analyzing the association between the instrument and the lagged outcomes. We find a statistically significant correlation for only one lagged outcome, providing support for the parallel-trends assumption. Second, we test for differential trends by interacting key pre-period characteristics with period indicators. Our baseline results are, for the most part, robust to these controls; key regressors do not seem to have evolved along predicted unauthorized migration. Third, we implement the correction proposed by Borusyak and Hull (2020) to deal with possible non-random exposure. This is, we control for a constructed counterfactual instrument.[9]. Controlling by this simulated variable is also useful to test whether the results are solely driven by the initial shares. Our main results are largely unaffected. All these results are displayed in Table 4. Finally, we analyze concentration of migrant networks by county. Predicted Mexican migrant composition in counties is not excessively concentrated. The top 50 sending municipalities account for a little over 30% of predicted migrants. On average, each of these top 50 municipalities provide only around 2.5% of the total predicted migrants per county, but have migrants living in over 700 counties. The average county has predicted migrants from around 650 municipalities, out of which the top 20 per county provide on average 85% of the total.

4.2 First Stage

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

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

For comparison purposes, Table 1 presents four instruments. Column 1 shows the results for the least conservative instrument. This is almost an identical instrument to the one described before; however, it leaves out only the county of interest rather than the CBSA. This results in the strongest first stage with an F-stat of 880. The instrument in Column 3 leaves the whole state out. Column 4 displays the results of the push factor shift-share instrument. Despite exploiting variation in Mexican municipalities, the first stage is very strong. Throughout the rest of the paper, we use the leave-one-CBSA-out as our preferred specification, but also present the push factors instrument. Results are similar with the other two LOO instruments.

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***	1.386***
population	(0.038)	(0.040)	(0.067)	(0.058)
Observations	8019	8019	8019	8019
F statistic	880	822	386	569
Mean of Dep. Var	0.463	0.463	0.463	0.463
Mean of Ind. Var	0.421	0.404	0.318	0.415

Column 1 displays the results for a leave-one-out (LOO) shift-share regressor that leaves the country itself out. Column 2 displays results for a LOO shift-share regressor that leaves the CBSA out. Column 3 displays results for a LOO shift-share regressor that leaves the state out. Column 4 displays results for a shift-share regressor that predicts yearly migration by municipality using push factors like poverty, 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. *p<0.05,**p<0.01,***p<0.001

5 Main Results

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

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

5.1 Voting Behavior

Across our specifications, we find that unauthorized migrant flows drive greater vote share for the political right. Table 2 displays estimated impacts of unauthorized migrant arrivals on Republican party vote share in House and presidential elections. The baseline OLS estimates, in Panel A, show that there is a statistically significant, positive relationship between unauthorized migration and Republican vote share. The coefficients present a pattern that is consistent with the causal estimates, too. The House midterm relationship is the largest (Columns 1 and 2). A 1 percent point increase in unauthorized migrants is associated with a 6.51 point increase in the share of votes that go to Republicans. Presidential year relationships are smaller in magnitude, both for the House of Representatives (Columns 3 and 4) and for the President (Columns 5 and 6). Finally, our weighted and unweighted estimates seldom differ statistically. We focus on population weighted estimates throughout the remainder of the article because these estimates are often more precise and robust than the unweighted estimates.

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

	House, Midterm		House, Pres year		President	
	(1) Weight	(2) Un-weight	(3) Weight	(4) Un-weight	(5) Weight	(6) Un-weight
A. OLS						
Newcomers, pct. pop.	6.51***	5.25***	2.82**	3.25***	3.19***	3.17***
	(0.87)	(0.70)	(1.06)	(0.65)	(0.65)	(0.45)
B. 2SLS, Loo-cbsa						
Newcomers, pct. pop.	8.49***	9.18***	3.49**	4.93***	4.42***	5.51***
	(1.03)	(1.05)	(1.21)	(0.94)	(0.71)	(0.70)
Std. Coefficient	0.26	0.23	0.10	0.12	0.16	0.17
$\hat{\beta} * \bar{x}$	3.93	2.52	1.61	1.35	2.12	1.57
C. 2SLS, push factors						
Newcomers, pct. pop.	7.86***	8.49***	3.08*	4.60***	4.31***	5.58***
	(1.07)	(1.14)	(1.30)	(1.00)	(0.69)	(0.72)
Std Coefficient	0.24	0.21	0.09	0.11	0.16	0.17
$\hat{\beta} * \bar{x}$	3.64	2.33	1.43	1.26	2.06	1.60
Observations	7995	7995	8015	8015	7236	7236
Dep. Var., Mean	48.16	61.70	47.24	63.79	45.83	61.54
Dep. Var., Sd	19.44	18.14	19.92	19.14	16.39	15.52

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

Panel B presents the LATEs of our preferred specification, the leave-one-CBSA-out shift-share (LOO CBSA) instrument which follows Tabellini (2020). These inflows are larger in magnitude than OLS estimates. In the House midterm elections, a mean flow of unauthorized migrants causes a 3.93 point increase in vote share for Republican candidates (Column 1, std coeff: 0.26). In presidential years, a mean flow of unauthorized migrants causes a 1.61 point increase in vote share for Republican House candidates (Column 3, std coeff: 0.10) and a 2.12 point increase for the Republican presidential candidate (Column 5, std coeff: 0.16). Hence, when unauthorized migrants choose their location because of past networks, US natives respond by voting more conservatively.

The 2SLS estimates in Panel C are from the shift-share instrument that leverages shocks in Mexican municipalities. This LATE captures the impacts of migrants who arrive because of shocks in Mexico and the migrant network. Generally, the estimates from this instrument are statistically identical to those from the LOO CBSA instrument. A mean flow of migrants (moved by shocks and migrant networks) causes a 3.64 point increase in Republican vote share (Column 1, std coeff: 0.24). With this instrument, the weighted impacts in presidential years are also precise (Columns 3 and 5).

These effects are consistent with historical findings and those from other countries receiving migrants. Mayda et al. (2022a) estimate that a 1 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. We hypothesize that our estimates are almost twice as large as theirs because our dataset captures the politically salient population which prompts natives' reactions ("low-skilled" vs unauthorized). Our period of study is one in which anti-immigrant policy is arguable more salient.

Collectively, these results suggest that migrant flows driven by the existing migrant network prompt more conservative voting. Importantly, the estimated effect from both instruments is consistently higher than that of OLS. Our hypothesis is that migrants tend to migrate to economically promising and politically welcoming areas, leading to downward bias in OLS. We explore this possibility in raw data and find suggestive evidence that migrants select into more economically promising locations (See Appendix K).

For each analysis in the remainder of the paper, we present three sets of estimates. In Panel A, we show OLS estimates for a baseline comparison. Panel B displays second stage estimates from the leave-one-out shift-share instrument and Panel C displays second stage estimates from the push factors instrument. For both specifications, our instrument is closely correlated in our first stage, so we use the reduced form estimates when we investigate robustness.

5.2 Election Outcomes

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

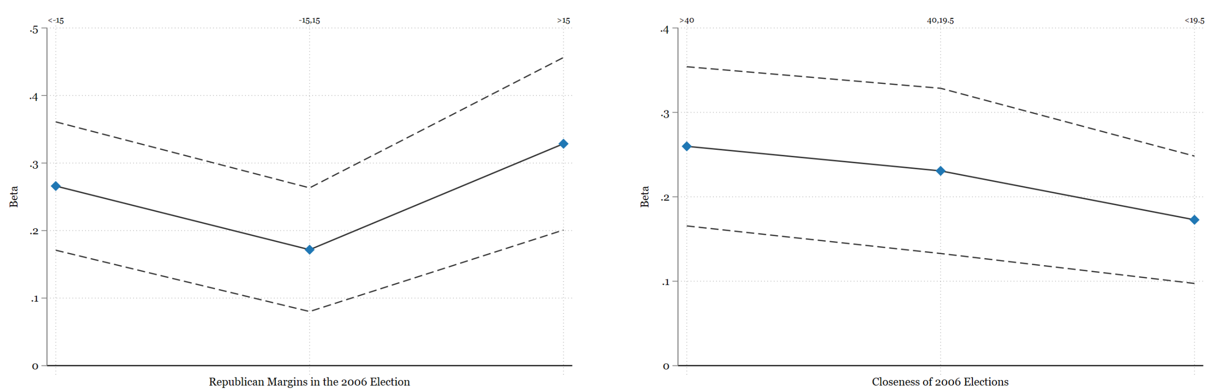

Figure 4: Regressions across bins of counties by electoral results in 2006

The left graph displays the results by 2006 Republican margin. On the left is the effect of newcomers on Republican vote share in counties that Republicans lost by 15 points or more, Democratic strongholds. Moving right, the estimates reflect the effect of new unauthorized migrants on Republican vote share where Republicans have done better. None of the estimates are statistically distinguishable from the others. In the right graph, the left side are the counties where either party won by a landslide in 2006. In each successive group, the electoral margin narrows. The results are similar. None of the estimates across the bins are statistically distinguishable from the others. Unauthorized migrants prompt the shift to the right, across the political spectrum. Past electoral results are not a source of heterogeneity in the electoral effects of unauthorized migrants. The identified effect of unauthorized migration could have changed very close elections.

5.3 Policy Change

Even though voters are changing their behavior in federal elections, this may be driven by a national narrative. Changes in federal elections may not be changing local policy. To explore policy changes in response to unauthorized migration, we study county-level public expenditures. Examining public spending gives us leverage on two questions of interest. First, it allows us to explore whether the inflow of new unauthorized migrants creates a reduction in the provision of local public goods. Second, we are able to explore whether the changes in public spending are consistent with a party that is more fiscally conservative, opposes redistribution, and focuses on law and order. We find evidence of reallocation in public expenditure consistent with more conservative policy. In response to new unauthorized migrants, counties spend less in total dollars per child on public education (log), and they increase the proportion of spending on police and the court system. Namely, local politicians respond to unauthorized migration by limiting spending on public education and investing in security.

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

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

Table 3: 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	0.13
	(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*	0.23	0.42**	0.26**
	(0.01)	(0.02)	(0.02)	(0.02)	(0.07)	(0.61)	(0.14)	(0.10)
Std. Coefficient	-0.05	-0.07	-0.10	0.06	0.12	0.01	0.15	0.21
$\hat{\beta} * \bar{x}$	-0.02	-0.02	-0.03	0.02	0.08	0.13	0.23	0.15
C. 2SLS Push factors								
Newcomers, pct. pop.	-0.02*	-0.04**	-0.04*	0.04*	0.11	0.77	0.39**	0.22*
	(0.01)	(0.02)	(0.01)	(0.02)	(0.07)	(0.56)	(0.15)	(0.10)
Std. Coefficient	-0.04	-0.07	-0.07	0.06	0.09	0.04	0.14	0.17
$\hat{\beta} * \bar{x}$	-0.01	-0.02	-0.02	0.02	0.06	0.42	0.22	0.12
Observations	5338	5338	5328	5334	5266	5328	5334	5266
Dep. Var., Mean	1.57	1.53	1.96	-1.44	-2.95	40.93	5.45	1.41
Dep. Var., Sd	0.38	0.38	0.34	0.49	0.87	11.54	1.85	0.85
Ind. Var., Mean	0.55	0.55	0.55	0.55	0.55	0.55	0.55	0.55
Ind. Var., Sd	0.68	0.68	0.68	0.68	0.68	0.68	0.68	0.68
Inst. 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. 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. Standard errors are clustered at the CBSA level. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

Local revenue and direct expenditure go down in response to unauthorized migration consistent with conservative policy and the preference for less redistribution. The second stage estimates (Panel B) suggest that a mean flow of unauthorized migrants reduces revenue by 2 percentage points (Column 1, std coeff: -0.05) and reduces direct expenditure by 2 percentage points (Column 2, std coeff: -0.07) as well.[10]

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

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

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

6 Robustness Checks

Our empirical strategy relies on the assumption that the observed effects in the variables of interest are solely due to the instrument via the endogenous variable. Namely, we assume

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

that counties with more predicted migrants were not already in a different trend due to, for example, persistent impacts of the initial shares or the evolution of other observed or unobserved key variables. (Borusyak et al., 2022; Cunningham, 2021; Goldsmith-Pinkham et al., 2020).

We test this hypothesis in Table 4. First, in row B, we test for pre-trends by regressing the instrument on pre-period outcomes—lagged 12 years, 3 periods. The values for the midterm elections are results in 1998, 2002, and 2006; for the presidential year elections in 2000, 2004, and 2008; and for the fiscal outcomes, values for 2002 and 2007. Unlike most migration studies that leverage shocks (Rozo and Vargas, 2021; Tabellini, 2020; Sequeira et al., 2020), our setting is characterized by continuity. Migration from Mexico to the US is a century-old phenomenon that saw a consistent increase from the early 1990s until the mid 2000s. Moreover, our variables of interest tend to be persistent and move only marginally in the short run. Reassuringly, however, our instrument is statistically unrelated with all but one lagged variable: vote shares for the Republican presidential candidates.

Second, we test for differential trends by interacting several pre-period characteristics with period indicators in rows C–F. The intention is to explore whether the observed effect is being driven by the evolution of key pre-period characteristics rather than by our instrument. Row C, in particular, controls by the share of Mexican population without US citizenship in 2000, obtained from the US Census, interacted with period dummies. This control aims to model the evolution of unauthorized Mexicans given the stock Mexican-born residents in 2000. Conditioning on such projection does not statistically alter our results. Panels D, E, and F condition on the share of Hispanics in 2000, the rate of high school completion among adults in 2000 and exposure to the China shock in 2006, constructed with Peter K. Schott's Data, County Business Patterns and Acemoglu et al. (2016) replication files. Neither of these three variables alters the magnitude or significance of our results, except for Column 2.

Third, we explore whether migration exposure was non-random. Non-random exposure

Table 4: Robustness checks

	Midterms	Pres year		Log pc		Share of Expend	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	House	House	Pres	D. Exp	Educ	Police	Judicial
A. Reduced form, baseline							
Instrument	9.85***	4.04**	5.10***	-0.05**	-0.06**	0.49**	0.31**
	(1.22)	(1.37)	(0.78)	(0.02)	(0.02)	(0.17)	(0.12)
B. Lagged outcome (LO)							
Instrument	0.99	2.91	3.69***	-0.00	-0.02	-0.03	-0.02
	(2.01)	(2.04)	(0.55)	(0.02)	(0.01)	(0.20)	(0.11)
C. Mex non-citizen, sh							
Instrument	9.85***	8.09***	8.26***	-0.04	-0.06*	0.42*	0.33*
	(1.36)	(2.31)	(1.10)	(0.03)	(0.03)	(0.19)	(0.15)
D. Hispanics, sh							
Instrument	10.33***	4.91**	6.51***	-0.04*	-0.06**	0.48**	0.29*
	(1.33)	(1.77)	(0.77)	(0.02)	(0.02)	(0.18)	(0.12)
E. Adult HS completion							
Instrument	11.19***	5.64***	7.26***	-0.05**	-0.06**	0.51**	0.32**
	(1.25)	(1.31)	(0.59)	(0.02)	(0.02)	(0.17)	(0.12)
F. China shock							
Instrument	8.41***	2.71	3.54***	-0.05**	-0.07**	0.53**	0.33**
	(1.35)	(1.47)	(0.76)	(0.02)	(0.02)	(0.16)	(0.12)
G. Simulated instrument							
Instrument	9.84***	7.94	7.52***	-0.05	-0.13***	0.43	0.49***
	(2.51)	(4.51)	(1.48)	(0.05)	(0.04)	(0.26)	(0.15)
H. Spatial lag							
Instrument	7.96***	4.22	3.92***	-0.05*	-0.05	0.42**	0.25
	(1.49)	(2.35)	(1.18)	(0.02)	(0.03)	(0.16)	(0.13)
I. Stock Mex foreign							
Instrument	10.03***	4.67***	5.19***	-0.05**	-0.06**	0.50**	0.31**
	(1.25)	(1.40)	(0.80)	(0.02)	(0.02)	(0.16)	(0.11)
J. Stock Hispanics							
Instrument	9.07***	2.50	4.81***	-0.05*	-0.06*	0.57**	0.33*
	(1.18)	(1.39)	(0.81)	(0.02)	(0.02)	(0.20)	(0.14)
K. No-outliers							
Instrument	11.17***	4.24*	6.08***	-0.06*	-0.06*	0.64**	0.32
	(1.37)	(1.77)	(0.87)	(0.03)	(0.03)	(0.21)	(0.17)
L. No pop weights							
Instrument	10.69***	5.74***	6.31***	-0.04	-0.06**	0.56**	0.24**
	(1.27)	(1.08)	(0.77)	(0.02)	(0.02)	(0.19)	(0.09)
*M. County-group * period FE*							
Instrument	10.11***	6.03***	5.85***	-0.09*	-0.09**	0.64	0.28*
	(1.45)	(1.17)	(0.82)	(0.03)	(0.03)	(0.33)	(0.13)

Dependent variables in columns 1–3 are the vote share for the Republican Party in different federal elections. Dependent variables in column 4–5 are the log of per capita (per child population in column 4) expenditure. Columns 6–7 are the share of direct expenditure. All estimations are reduced form. Sources: Dave Leip's US Election Data; Annual Survey of State and Local Government Finances; US Census; USDA's Economic Research Service; Peter K. Schott's Data; County Business Patterns; ACS 5 from the Social Explorer; Acemoglu et al. (2016). and QCEW. Panel 1 is reduced form estimation. Panel 2 estimates effect on lagged dependent variables. Panel 3 controls for pre-2007 features interacted with period dummies. Panel 4 includes simulated instrument following Borusyak and Hull (2020). Panel 5 controls for the 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<0.05,**p<0.01,***p<0.001

would occur if migrants from certain Mexican municipalities had simultaneously sorted in politically biased counties and had a different growth rate over the period of study. In row G, we implement the correction proposed by Borusyak and Hull (2020). We constructed a counterfactual instrument by taking the average of 2,000 simulated instrument shifters, created by multiplying the original shares by each of 2,000 permutations of the LOO shifters from other county-municipality dyads in the same period. The results remain largely significant.

To rule out the possibility of spatial effects and spillovers, row H controls for the spatial lag–average of neighboring counties–of the LOO instrument. Most estimates are robust to this control.

We also test for a different link between migration and political outcomes. To disentangle the role of stocks versus flows, row G explicitly conditions on the estimated share of residents who were born in Mexico the year before our periods started, obtained from ACS 5 2005–09, 2009–13, and 2013–17. While this variable theoretically captures both authorized and unauthorized migrants (and is noisier for smaller counties), it correlates strongly with the instrument (ρ or around 0.86). Conditioning on the share of Mexican-born residents does not change the estimates. Row J uses a similar control, the share of residents who identified as Hispanics. Finally, rows K–M present estimates excluding outliers (percentiles 1 and 99 of the predicted migration distribution), without using predicted population weights and exploiting variation within groups of politically similar counties (in the state) following Bazzi et al. (2021). The estimates remain consistent.

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

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

7 Mechanisms

We now explore possible channels for the conservative electoral and policy response to migrants. We interrogate existing explanations for the backlash. We consider two sets of explanations. The first explores how migrants could trigger the backlash. The movement to the right may be driven by economic losses, and right-leaning politicians' willingness to blame the migrants for those losses. We examine the labor market consequences of unauthorized migration, and the broader welfare implications at the country level. Our second set of explanations explores what about the electorate is changing. Migration might be driving underlying population change, changes in ideology or partisanship, or the values and policy preferences of those voting. Altogether, we find evidence that job loss, poverty, out-migration, and value change all explain some of the political shift. Our findings are consistent with explanations for conservative reactions that rely on economic loss, out-group bias, or perception of threat—be it social, cultural, or economic.

7.1 Employment and Wages

We consider to what extent economic losses in employment or wages can explain the conservative shift. Labor market theories of the conservative electoral reaction suggest that migrants decrease employment and wages among similarly skilled US natives (Peri and Sparber, 2009; Blau and Mackie, 2017; Borjas and Edo, 2021), and politicians promise anti-migrant policy to attract those who lost in the labor market. Like much of the existing literature on labor and immigration, we show small changes in labor market outcomes as a results of unauthorized migrants. Unauthorized migration reduces employment in construction and hospitality. Further, US natives may switch from these migrant-intensive industries to manufacturing, which is less accessible to unauthorized migrants. We find no effect on wages for all sectors, except agriculture, another migrant-intensive industry. The small magnitude of the labor market effects likely does not explain most of the political response.

To measure the labor market effects of migrants, we focus on several indicators. We use the Quarterly Census of Employment and Wages (QCEW), which reports the annual average employment and weekly wages for multiple sectors and super-sectors. We examine total average annual employment and wages and break out the super-sectors of construction, manufacturing, leisure and hospitality, and agriculture (which also includes forestry, fishing, and hunting). The employment variables are measured in logs per working-age population (ages 15–64, US Census). Wages correspond to 2010 dollars.[11]

Table 5 presents the results on employment and wages. Panel A displays the same pattern for the baseline OLS estimates, generally biased toward zero and less precise. Panels B and C present LATE estimates for the leave one-CBSA-out and push factor instruments, respectively. Substantively, the impact on employment is modest in a few sectors, and the impact on wages is isolated mostly to agriculture. A mean flow in unauthorized migrants decreases employment per working age person by 2% in the construction industry (Panel B, column 2, std coeff: -0.07)) and by 1% in hospitality and leisure (Panel B, column 4, std coeff: -0.03). At the same time, mean migrant flows increase employment in manufacturing. This finding suggests some native workers switch to manufacturing, an industry that likely advantages those in formal employment. A mean flow of newcomers increases manufacturing

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

employment by 3% (Panel B, Column 3), leaving the main effect on total employment a precise zero (Panel B, column 1). There is a small, but measurable substitution effect; unauthorized migration decreases formal employment in construction and hospitality and increases formal employment in manufacturing.

The results for construction are consistent with the literature and the dynamic of this particular sector (Peri and Sparber, 2009). Day labor in construction is often readily available to Mexican newcomers. Contingent work has low barriers to entry, and Mexican communities often use informal organization to facilitate day labor, which is disproportionately in construction (Valenzuela, 2003). Our interpretation of the data recorded by QCEW is that they capture informal employment less well, and formal employment in construction is a fraction of the actual construction jobs. Hospitality may be similar. Since wages do not change in these sectors, it may be simply that the decline in employment reflects loss of formal employment by US natives and gain of informal employment by arriving unauthorized migrants.

The results for the impact on wages are displayed in Table 5 on the right side. The 2SLS estimates using both instruments (Panels B and C) reveal negative impacts on agricultural wages. A mean flow of migrants drives agricultural wages down by $22.07 weekly (Panel B, Column 10, std coeff: -0.15). Using the push factor instrument, we find a decrease in construction wages as well. A mean flow of migrants drives construction wages down by $11.45 weekly (Panel C, Column 7, std coeff: -0.07). These wage results however, are among the least robust results in our analysis. In part, this may be due to the large number of temporary (authorized) workers in the agriculture sector.

Together, these findings are consistent with literature on the impact of unauthorized migrants on labor market outcomes. Scholars generally find small reductions in wages and employment, often limited to a few immigrant-intensive sectors (Hanson, 2009; Monras, 2020; Blau and Mackie, 2017). These wage and employment decreases are hardly the magnitude of decreased wages associated with other global flows (Autor et al., 2016). However, they

Table 5: Effect of arrival of unauthorized Mexican migrants on employment among working age population and weekly wages 2010-2018

	Employment, (log per working age pop)					Weekly Wages (2010 USD)				
	(1) Total	(2) Constr	(3) Manufact	(4) Hosp and leis	(5) Agric	(6) Total	(7) Constr	(8) Manufact	(9) Hosp and leis	(10) Agric
A. OLS										
Newcomers, pct pop.	0.01	-0.03	0.07***	-0.01*	0.03	3.98	-14.35	12.37	-4.83	-18.30
	(0.01)	(0.02)	(0.02)	(0.01)	(0.06)	(14.27)	(9.58)	(24.00)	(4.45)	(12.19)
B. 2SLS Loo										
Newcomers, pct pop.	-0.00	-0.05**	0.08***	-0.02*	-0.09	-1.78	-22.58	16.02	-8.06	-42.74*
	(0.01)	(0.02)	(0.02)	(0.01)	(0.08)	(18.66)	(12.82)	(33.34)	(6.44)	(19.38)
Std. Coefficient	-0.00	-0.07	0.06	-0.03	-0.04	-0.00	-0.06	0.03	-0.04	-0.14
$\hat{\beta} * \bar{x}$	-0.00	-0.02	0.03	-0.01	-0.05	-0.82	-10.47	7.43	-3.73	-22.07
C. 2SLS push factors										
Newcomers, pct pop.	0.00	-0.04	0.09***	-0.02*	-0.05	-10.30	-24.69*	-6.55	-8.76	-38.76*
	(0.01)	(0.02)	(0.02)	(0.01)	(0.09)	(12.63)	(9.80)	(21.47)	(5.97)	(18.84)
Std. Coefficient	0.00	-0.05	0.07	-0.03	-0.02	-0.02	-0.07	-0.01	-0.04	-0.12
$\hat{\beta} * \bar{x}$	0.00	-0.02	0.04	-0.01	-0.03	-4.76	-11.45	-3.04	-4.05	-20.02
Observations	8003	7388	7376	7906	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., Mean	0.42	0.42	0.42	0.42	0.48	0.42	0.42	0.42	0.42	0.48
Inst., Sd	0.57	0.57	0.57	0.57	0.61	0.57	0.57	0.57	0.57	0.61

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

do demonstrate 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 may well account for some voters punishing pro-immigrant politicians.

7.2 County Welfare

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

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

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

The baseline OLS estimates in Panel A follow a similar pattern as in most of our analysis. OLS is generally biased toward zero, and the 2SLS estimates (Panels B and C) are larger in magnitude. There is no effect on GDP per capita (Column 1) or unemployment (Column 3). The poverty rate increases. A mean flow of unauthorized migrants increases the poverty rate in the county by 4% (Panel B, Column 4, std coeff: 0.14) following the midterm election. This raises the possibility that unauthorized migration may be impacting poverty through

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

	County Economy (log)				
	(1) GDP pc	(2) Median household income	(3) Unemployment rate	(4) Poverty rate log	(5) SNAP recipients per capita log
A. OLS					
Newcomers, pct. pop.	-0.01	-0.02	0.01	0.06***	0.03
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)
B. 2SLS Loo					
Newcomers, pct. pop.	-0.02	-0.02	0.04	0.09***	0.06**
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Std. Coefficient	-0.03	-0.05	0.05	0.14	0.07
$\hat{\beta} * \bar{x}$	-0.01	-0.01	0.02	0.04	0.03
C. 2SLS push factors					
Newcomers, pct. pop.	-0.03	-0.03*	0.04	0.09***	0.04
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Std. Coefficient	-0.04	-0.06	0.05	0.14	0.04
$\hat{\beta} * \bar{x}$	-0.01	-0.01	0.02	0.04	0.02
Observations	7884	8019	8019	8019	8019
Dep. Var., Mean	3.89	10.88	1.69	2.59	2.51
Dep. Var., Sd	0.44	0.26	0.45	0.39	0.51
Ind. Var., Mean	0.47	0.46	0.46	0.46	0.46
Ind. Var., Sd	0.59	0.59	0.59	0.59	0.59
Inst., Mean	0.42	0.42	0.42	0.42	0.42
Inst., Sd	0.57	0.57	0.57	0.57	0.57

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

less redistributive policy. It may also simply be cumulative.

Two less robust findings reinforce the interpretation that unauthorized migrants are increasing inequality and poverty among US Natives. With the push factor specification, unauthorized migrants decrease median household income, slightly, suggesting greater inequality (Panel C, Column 2). With the leave-one-out specification, the use of supplemental food benefits (SNAP) increases because of unauthorized migration. A mean flow of newcomers increases SNAP recipients by 3% (Panel B, Column 5, std coeff: 0.07). SNAP has the virtue of being among the most responsive federal entitlement programs, and it is unavailable to unauthorized migrants. Participation among non-citizens is minimal, and since the arrivals we are studying are from the previous four years, it is unlikely they have US-born citizen children who qualify.[12]

Together, these estimates mean that the county as a whole is as productive as it would have been without the unauthorized migrants, but the bottom part of the distribution is worse off, suggesting greater inequality. These findings are consistent with employment analysis, but they are substantively larger. Moreover, appendix O shows that the effect of migration on poverty seems to be higher in counties that saw little or no growth in construction employment. Increasing poverty and inequality due to unauthorized migration is important for understanding the conservative electoral response. It represents another channel that anti-immigrant politicians could exploit. Further, these findings are consistent with economic and group threat explanations for the conservative reaction.

7.3 Composition of the Electorate and Values

Another explanation for why we observe the shift to the right is that the population changes in response to unauthorized migrant arrivals or that their policy preferences change. We find evidence for both of these mechanisms. Unauthorized migration flows change the electorate,

[12]U.S. Department of Agriculture Food and Nutrition Service. Supplemental Nutrition Assistance Program: Guidance on Non-Citizen Eligibility. Found on the internet at https://www.nilc.org/wp-content/uploads/2019/05/Non-Citizen_Guidance_063011.pdf

through population decline and out-migration. Among the people who remain, the presence of unauthorized migrants causes a decrease in universalist values. These findings suggest that those US natives who remain in the county may be more prone to out-group bias and have a preference for less universal redistribution. Despite their values changing in response to migration, natives' partisan identities and ideology do not change as a result of the unauthorized migration.

We explore several ways in which the composition of the electorate and values could change. One idea is that unauthorized migration may prompt more left-leaning people to leave a county and more right-leaning people to stay in the county. In this case we observe a shift to the right in voting, but it is driven by the out-migration of left-leaning voters. A second possibility is that US natives change their ideology or partisan affiliation in response to unauthorized newcomers. In this case, we observe the shift to the right because people have adopted a more conservative ideology or identify more with the Republican party. Last, it may be that unauthorized migrants trigger a change in preferences for redistribution. That is, US natives are voting for Republicans because they are less inclined toward redistribution.

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

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

We find aggregate declines in population, increases in out-migration, and movement away from universalist values toward communal values in response to unauthorized migration. Table 7 displays the results. As with the main estimates, OLS (Panel A) is generally biased toward zero. The 2SLS estimates in Panels B and C are larger. Columns 1 and 2 explore population changes. Column 1 shows that population declines in response to unauthorized migration. A mean flow of unauthorized migrants causes a 1% decrease in county population (Panel B, Column 1, std coeff: -0.01). Column 2 shows that some of the population decline is attributable to out-migration. A mean flow of unauthorized migrants causes a 2% increase in out-migration (Panel B, Column 2, std coeff: 0.08). These estimates are consistent with some sorting occurring.

There is little evidence that people are becoming more conservative or identify more with the Republican party because of unauthorized migration. Columns 3 and 4 in Table 7 display the results for impacts on Republican identity and conservative ideology. While the changes in voting behavior are robust findings, the preference changes revealed by the elections do not translate into changes in ideology or partisan identification. That is, while flows of unauthorized migrants prompt US natives to vote for Republican candidates, they do not prompt greater proportions of the population to identify with the Republican party nor to become more conservative in ideology.

Nevertheless, the values of US natives do change in response to unauthorized migrants. The final three columns in Table 7 explore the impact of unauthorized migrants on universalist (as compared to communal) values. In the full sample of counties, a mean flow

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

	Pop (log)	Pc log	Preferences (scaled)		Universalist values		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Count	Out migration	Republican Identity	Ideology Conservative	Full sample	Below median out-migration	Above median out-migration
A. OLS							
Newcomers, pct. pop.	-0.02**	0.03**	0.03	0.03	-0.09*	-0.12*	-0.01
	(0.01)	(0.01)	(0.02)	(0.02)	(0.04)	(0.05)	(0.07)
B. 2SLS Loo							
Newcomers, pct. pop.	-0.02**	0.04***	0.03	0.02	-0.13**	-0.12	-0.09
	(0.01)	(0.01)	(0.03)	(0.03)	(0.04)	(0.06)	(0.07)
Std. Coefficient	-0.01	0.08	0.10	0.09	-0.16	-0.16	-0.08
$\hat{\beta} * \bar{x}$	-0.01	0.02	0.01	0.01	-0.06	-0.07	-0.03
C. 2SLS push factor							
Newcomers, pct. pop.	-0.02*	0.04***	0.02	0.02	-0.15***	-0.15*	-0.09
	(0.01)	(0.01)	(0.03)	(0.03)	(0.04)	(0.06)	(0.07)
Std. Coefficient	-0.01	0.08	0.08	0.07	-0.18	-0.20	-0.08
$\hat{\beta} * \bar{x}$	-0.01	0.02	0.01	0.01	-0.07	-0.08	-0.03
Observations	8019	8017	1943	1943	5712	2897	2809
Dep. Var., Mean	12.93	-2.94	-0.04	-0.02	0.15	0.16	0.14
Dep. Var., Sd	1.59	0.28	0.16	0.16	0.50	0.46	0.56
Ind. Var., Mean	0.46	0.46	0.50	0.50	0.47	0.55	0.30
Ind. Var., Sd	0.59	0.59	0.61	0.61	0.60	0.63	0.48
Inst., Mean	0.42	0.42	0.48	0.48	0.43	0.52	0.23
Inst., Sd	0.57	0.57	0.60	0.60	0.57	0.62	0.40

The dependent variable in column 1 is the log of county population. The dependent variable in column 2 is out-migration, calculated as the log of out-migration divided by county population. Dependent variables in columns 3–4 are average county preferences, normalized and shrunk following Enke (2020). Dependent variables in columns 5–7 are the average relative importance of universalist values, taken from by Enke (2020). Columns 5 is calculated over the full sample, and observations in columns 6–7 are divided according to whether they are above or below the partialled-out median of out-migration. Sources: Enke, (2020); Cooperative Election Study; US Census Bureau: Population Division and Small Area; US Census Bureau: 2007-2011, 2011-2015, and 2015-2019 American Community Surveys 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. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

of unauthorized migrants shifts counties 0.06 standardized units toward communal values (Panel B, Column 5, std coeff: -0.16).

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

Last, we examine how changes in values might be related to out-migration. Work on emigration from developing countries shows that out-migration prompts distrust and makes overcoming collective action problems more difficult (Sellars, 2019). However, we find evidence that points in the opposite direction. Counties where there is little out-migration seem to drive the values changes in response to unauthorized migrants. In counties with below-median out-migration, a mean flow of unauthorized migrants prompts a shift toward communal values of 0.07 standardized units (Panel C, Column 6, std coeff: -0.20). These results are less precise than in the full sample. However, in comparison to counties with above-median out-migration (Column 7), the evidence suggests that nearly all of the values change is occurring in places with little out-migration.

To explore the relationship between the different mechanisms at play, we estimate the impact of migration on universalist values and out-migration according to whether the county is above or below the median poverty rate of its state. Change of values and out-migration appear to be substitute responses to unauthorized migration. Counties that have above median poverty rates tend to have substantively more pronounced effects on universalist values but more muted effects on out-migration. Conversely, counties that have below median poverty rates tend to have more muted impacts on values but higher impacts on out-migration (See Appendix P for empirical results). That is in high poverty counties, people tend to stay put, but they become more communal. Whereas in low poverty counties, people tend to migrate, and values do not change as much.[13]

7.4 Robustness for mechanisms

In Appendix Q we conduct the same robustness checks on the mechanisms. The results hold even better. The mechanisms identified are robust to controlling for differential trends, simulated instrument, the stock of Mexican-born people at the beginning of the period, removing outliers, and not using population weights. All but one (out-migration)[14] complies with the parallel trends assumption. Two of them have an opposite effect: contemporaneous predicted migration is negatively associated with wages in agriculture and poverty rate, suggesting a reversal in the trend.

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

[13]This may be because there are two forces at work in counties with out-migration. The unauthorized inflows may trigger more communal values, while the out-migration triggers more universalist values and these are canceled out.

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

or deportation.

8 Taxation and the Social Safety Net

We have documented that the inflow of recent unauthorized migrants increased the vote share of the Republican party in federal elections, reduced public expenditure in education and increased relative expenditure on police and the judiciary. We have shown that the effects are most likely driven by a decrease of employment in key sectors, like construction and hospitality, which caused a significant increase in poverty rate. As a result, counties saw either a decrease in universalist values or an increase in out-migration.

In this section, we explore whether these effects are heterogeneous. Our hypothesis is that counties with more progressive tax structures or a more generous social safety net are better able to mitigate economic shocks and compensate economic losers. The impacts of unauthorized migration should be lower in places with progressive taxation and more robust redistribution. We find suggestive evidence that this is the case.

To capture the more progressive tax structure, we divide counties according to their ratio of revenue generated from income vs sales tax. According to the Institute on Taxation and Economic Policy (ITEP), US income tax is the most progressive local tax, whereas sales tax is the most regressive (Wiehe et al., 2018). Counties with a higher share of revenues from sales tax have a more regressive tax structure. The tax measures are ideal from the perspective that tax law changes are incremental, and therefore the county-level tax measure reflects redistribution preferences from the past.

Table 8 presents the effects of unauthorized Mexican migration by a county's income to sales tax ratio. Columns 1 to 4 show impacts on vote share for the Republican party, and Columns 5 to 10 show impacts on local expenditures. A substantive pattern emerges in this analysis. Counties with below median income to sales tax ratios have larger shifts to the right, and larger increases in allocations to the police and the judiciary. Due to the smaller sample size, these differences are not generally statistically significant, but they are meaningful. To illustrate, a mean migration flow causes a nearly 5 percentage point shift in favor of the midterm Republican candidates in low ratio counties (more regressive), but only a 3.2 percentage point shift in high ratio counties (more progressive).

Table 8: Political and policy effects by ratio of income vs sales tax

	Midterms, GOP		President, GOP		Edu (log pc)		Police, share		Judicial, share	
	(1) Low tax ratio	(2) High tax ratio	(3) Low tax ratio	(4) High tax ratio	(5) Low tax ratio	(6) High tax ratio	(7) Low tax ratio	(8) High tax ratio	(9) Low tax ratio	(10) High tax ratio
A. OLS										
Newcomers, pct. pop.	7.66***	5.06***	2.98***	2.87**	-0.01	-0.02	0.19	-0.04	0.10	0.10
	(0.91)	(1.52)	(0.84)	(0.89)	(0.02)	(0.02)	(0.18)	(0.16)	(0.20)	(0.08)
B. 2SLS Loo										
Newcomers, pct. pop.	10.45***	6.92***	4.62***	3.54***	-0.03	-0.04	0.40	0.13	0.16	0.23*
	(1.10)	(1.71)	(0.75)	(1.01)	(0.03)	(0.03)	(0.22)	(0.17)	(0.24)	(0.09)
Std. Coefficient	0.34	0.22	0.20	0.13	-0.07	-0.07	0.15	0.05	0.13	0.18
$\hat{\beta} * \bar{x}$	4.98	3.20	2.31	1.68	-0.02	-0.02	0.23	0.07	0.09	0.13
C. 2SLS push factor										
Newcomers, pct. pop.	10.11***	5.71***	4.71***	3.33***	-0.03	-0.02	0.53*	0.03	0.18	0.15
	(1.25)	(1.60)	(0.77)	(0.95)	(0.03)	(0.02)	(0.22)	(0.18)	(0.26)	(0.08)
Std. Coefficient	0.33	0.18	0.20	0.12	-0.06	-0.03	0.20	0.01	0.15	0.12
$\hat{\beta} * \bar{x}$	4.82	2.64	2.36	1.58	-0.01	-0.01	0.30	0.01	0.10	0.08
Observations	3860	3868	3430	3541	2574	2584	2582	2582	2550	2550
Dep. Var., Mean	52.83	46.43	50.43	43.90	1.89	2.01	5.39	5.52	1.28	1.50
Dep. Var., Sd	19.36	18.05	15.55	15.86	0.31	0.35	1.85	1.85	0.88	0.82
Ind. Var., Mean	0.48	0.46	0.50	0.47	0.58	0.55	0.57	0.55	0.57	0.55
Ind. Var., Sd	0.64	0.58	0.66	0.58	0.72	0.66	0.71	0.66	0.71	0.66
Inst., Mean	0.37	0.46	0.38	0.48	0.43	0.53	0.42	0.53	0.42	0.53
Inst., Sd	0.52	0.61	0.54	0.61	0.59	0.68	0.58	0.68	0.59	0.68

Dependent variable in columns 1–2 is the share of GOP vote in midterm House elections. Dependent variables in columns 3–4 is the share of GOP vote in Presidential elections. Dependent variables in columns 5–6 is the in log 2010 dollars if per child education expenditures. Dependent variables in columns 7–8 and 9–10 are shares of total direct expenditures in the police and the judiciary respectively. Observations in columns 1, 3, 5, 7 and 9, and 2, 4, 6, 8, and 10 are divided according to whether they are above or below the relative contribution of income vs sales tax in 2007 following the Institute for Taxation and Economic Policy. Above equals more importance of income tax, which suggests a more progressive fiscal policy. Above suggests a more progressive fiscal policy. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

Table 9 shows that the same pattern emerges when examining the mechanisms. In employment, low tax ratio counties (more regressive), have smaller gains in manufacturing, larger losses in construction, and smaller losses in hospitality and leisure. The pattern holds for poverty and universalist values, as well. Unauthorized migrants cause larger increases in poverty and more pronounced declines in universalist values in low tax ratio counties.

Table 9: Effects on employment, poverty, out-migration and moral values by ratio of income vs sales tax

	Emp, manufacturing		Emp, construction		Emp, leisure & hosp		Poverty rate		Universalist values	
	(1) Low tax ratio	(2) High tax ratio	(3) Low tax ratio	(4) High tax ratio	(5) Low tax ratio	(6) High tax ratio	(7) Low tax ratio	(8) High tax ratio	(9) Low tax ratio	(10) High tax ratio
A. OLS										
Newcomers, pct. pop.	0.06***	0.09**	-0.05*	0.03	0.01	-0.02	0.08***	0.03	-0.13*	-0.03
	(0.02)	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.06)	(0.06)
B. 2SLS Loo										
Newcomers, pct. pop.	0.06**	0.09*	-0.07**	0.01	0.00	-0.01	0.11***	0.07**	-0.20**	-0.03
	(0.02)	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.06)	(0.06)
Std. Coefficient	0.05	0.07	-0.09	0.02	0.00	-0.02	0.18	0.11	-0.25	-0.03
$\hat{\beta} * \bar{x}$	0.03	0.04	-0.03	0.01	0.00	-0.00	0.05	0.03	-0.10	-0.01
C. 2SLS push factor										
Newcomers, pct. pop.	0.08***	0.11***	-0.06**	0.03	0.00	-0.02	0.11***	0.06*	-0.21**	-0.06
	(0.02)	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.03)	(0.06)	(0.06)
Std. Coefficient	0.07	0.09	-0.08	0.05	0.00	-0.03	0.18	0.10	-0.25	-0.07
$\hat{\beta} * \bar{x}$	0.04	0.05	-0.03	0.02	0.00	-0.01	0.05	0.03	-0.10	-0.03
Observations	3481	3658	3499	3642	3805	3844	3876	3876	2560	2948
Dep. Var., Mean	-3.10	-3.00	-3.58	-3.62	-2.75	-2.71	2.65	2.55	0.06	0.19
Dep. Var., Sd	0.71	0.69	0.49	0.41	0.45	0.38	0.39	0.38	0.53	0.46
Ind. Var., Mean	0.48	0.46	0.48	0.46	0.48	0.46	0.48	0.46	0.49	0.47
Ind. Var., Sd	0.64	0.58	0.64	0.58	0.64	0.57	0.64	0.57	0.64	0.58
Inst., Mean	0.37	0.46	0.37	0.46	0.37	0.46	0.37	0.46	0.38	0.47
Inst., Sd	0.53	0.61	0.53	0.61	0.52	0.61	0.52	0.60	0.53	0.61

Dependent variable in columns 1–2 is the log of per working age employment rate in manufacturing. Dependent variable in columns 3–4 is the log of per working age employment rate in construction. Dependent variable in columns 5–6 is the log of per working age employment rate in leisure and hospitality. Dependent variable in columns 7–8 is the log of poverty rate. Dependent variable in columns 9–10 is the prevalence of universalist values following Enke (2020). Observations in columns 1, 3, 5, 7 and 9, and 2, 4, 6, 8, and 10 are divided according to whether they are above or below the relative contribution of income vs sales tax in 2007, following the Institute for Taxation and Economic Policy. Above equals more importance of income tax, which suggests a more progressive fiscal policy. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

These two tables provide suggestive evidence that the effect of migration is smaller in counties with more progressive taxation. The effects, however, are statistically different only for employment in construction. Our interpretation is that counties with more progressive taxation appear to be able to compensate the economic losers, lessening the negative labor market and welfare impacts of unauthorized migration.

We explore two other, state-level, measures of progressive taxation and a generous social safety net. First, we divide counties according to the index of fair taxation of the state in which they are located, produced by ITEP following a similar criteria as our county measure. Measures of regressive and progressive taxation are not well correlated with state partisanship, so we are not comparing conservative and liberal counties. Figure 13 in Appendix R shows that our state-level categories are not a good proxy for contemporary political leanings. Over the last 40 years there has been substantial devolution of fiscal authority from states to counties and cities (Gainsborough, 2003; Xu and Warner, 2016). The county measure (described before) reflects variation because of devolution, whereas the state measure captures taxation at the level at which it is legislated. Second, we divide states according to the share of the poor population covered by the federal program Temporary Assistance to Needy Families (TANF) (Gaines et al., 2021). This federal cash transfer program is administered at the state-level. Like Medicaid, state governments have discretionary ability to determine the operational rules (i.e. generosity of the program). The TANF to poverty ratio is a more direct measure of redistribution, and evidence shows that the implementation has been devolved as well (Fording et al., 2007). Regardless of the measure, the substantive results do not change.

Appendix R presents the estimates using the state-level taxation and safety net criteria instead. Tables 21 and 22 divide counties according to whether the state they are located in is above or below the median of the tax equality index created by ITEP. Tables 23 and 24 divide counties according to whether the state in which they are located is above or below the median of the TANF to poverty ratio. The main trends hold. The coefficients for the below mean group (low tax states or low TANF states) are, in general, larger in magnitude than that of the above mean (high) group. Despite finding statistical differences only for a handful of estimations across the three criteria, the results are remarkably consistent. The effects of unauthorized Mexican migration seem to be more modest in counties that are better able to compensate economic losers.

9 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 newcomer migrants, county vote share for the Republican party increased in House and presidential elections. Local government agencies divested in education and increased relative spending in policing and the administration of justice. We contend that there are two main explanations for this political and policy response. Migration created concentrated economic losses. Employment in construction, and in hospitality and leisure, "migrant-intensive" sectors, decreased. As a result, the poverty rate rose. The second reason is that the composition and preferences of counties changed in response to migration. Higher poverty rates prompted either an increase in out-migration or a decline in universalist values.

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, spatial lags, as well as not weighting by predicted population, removing outliers, and exploiting change 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, making the parallel trends assumption more likely.

These results contribute to a growing literature on backlash against migrants from developing countries. While responses to different groups of immigrants have been studied in the US, scholars had yet to quantitatively estimate the impacts of unauthorized migrants, the migrants whose presence has been most politicized. We document responses that are closer to reactions of Europeans to refugees, a group of migrants about whom debate has been highly politicized (Barone et al., 2016). Unlike the findings in Rozo and Vargas (2021), we cannot conclude that the response is explained by a radicalization of citizens at the extreme of the political distribution. Rather, we find little evidence of heterogeneity across the political spectrum. Our estimates, in line with those of Baerg et al. (2018), Mayda et al. (2022a), Mayda et al. (2022b) and Tabellini (2020), document consistent shifts to the right regardless of a county's past elections outcomes.

Rather than electoral outcomes, a county's taxation and redistribution is a notable source of heterogeneity. The social, political, and economic impacts of unauthorized migrants are concentrated in counties that have the least capacity for redistribution. Since the tax structure of a county changes slowly, it helps explain what prior party votes share does not. Places with more progressive taxation appear better able to compensate those who lose with the arrival of unauthorized migrants. These counties see no statistically significant construction job loss, small losses of employment in hospitality, and gains in employment in manufacturing that exceed the job losses. These counties also appear to show smaller impacts on poverty and no impact on the decline of universalist values. Rather, counties with regressive taxation appear to account for most of the negative impacts of unauthorized migrants and the substantive shift to the right. In counties with regressive taxation, unauthorized migrant arrivals cause more formal job loss, increase poverty, and substantially decrease support for universalist values. We argue that these counties that do not compensate economic losers are most destabilized by unauthorized migration. Ironically, in places with regressive taxation, unauthorized migrants prompt changes in values away from universal redistribution and the kinds of policies that elsewhere mute the negative impacts of unauthorized migrants.

From the standardized coefficients, it is clear that the effects of migration on the political and policy outcomes are larger than the effect estimated with the mechanisms. The persistent conservative reaction to unauthorized Mexican newcomers cannot be fully explained by the many mechanisms we have explored. Future areas of research should document additional explanations for this shift to the right. For example, understanding attitudes toward migration at the county level could inform these results. Furthermore, emerging work in behavioral economics (Ajzenman et al., 2022, 2021) and political psychology (Enos, 2017) on heightened reactions might help to explain the relative sizes of the impacts we estimate.

References

Abrajano, M. A. and Hajnal, Z. (2017). *White backlash: immigration, race, and American politics*. Princeton University Press, Princeton, NJ.

Acemoglu, D., Autor, D., Dorn, D., Hanson, G. H., and Price, B. (2016). Import Competition and the Great US Employment Sag of the 2000s. *Journal of Labor Economics*, 34(S1):S141–S198.

Adão, R., Kolesár, M., and Morales, E. (2019). Shift-Share Designs: Theory and Inference. *The Quarterly Journal of Economics*, 134(4):1949–2010.

Ajzenman, N., Dominguez, P., and Undurraga, R. (2021). Immigration, Crime and Crime Perceptions. http://www.ajzenman.com/wp-content/uploads/2021/11/ADU_Immigration_Perceptions-1.pdf.

Ajzenman, N., Dominguez, P., and Undurraga, R. (2022). Immigration and Labor Market (Mis)Perceptions. *AEA Papers and Proceedings*, 112:402–408.

Alesina, A., Baqir, R., and Easterly, W. (1999). Public Goods and Ethnic Divisions. *The Quarterly Journal of Economics*, 114(4):1243–1284.

Alesina, A. and Giuliano, P. (2009). Preferences for Redistribution. *NBER*, page w14825.

Alesina, A., Miano, A., and Stantcheva, S. (2022). Immigration and Redistribution. *The Review of Economic Studies*.

Alesina, A. and Tabellini, M. (2021). The Political Effects of Immigration: Culture or Economics? *SSRN Electronic Journal*.

Allen, T., Dobbin, C. d. C., and Morten, M. (2018). Border Walls. *NBER*, (w25267).

Alsan, M., Eriksson, K., and Niemesh, G. (2020). Understanding the Success of the Know-Nothing Party. *NBER*, (w28078).

Alsan, M. and Yang, C. (2019). Fear and the Safety Net: Evidence from Secure Communities. *NBER*, (w24731).

Annual Survey of State and Local Government Finances (2010). 2010 Annual Survey of Local Government Finances Methodology.

Autor, D. H., Dorn, D., and Hanson, G. H. (2016). The China Shock: Learning from Labor-Market Adjustment to Large Changes in Trade. *Annual Review of Economics*, 8(1):205–240.

Baerg, N. R., Hotchkiss, J. L., and Quispe-Agnoli, M. (2018). Documenting the unauthorized: Political responses to unauthorized immigration. *Economics & Politics*, 30(1):1–26.

Barone, G., D'Ignazio, A., de Blasio, G., and Naticchioni, P. (2016). Mr. Rossi, Mr. Hu and politics. The role of immigration in shaping natives' voting behavior. *Journal of Public Economics*, 136:1–13.

Bazzi, S., Ferrara, A., Fiszbein, M., Pearson, T. P., and Testa, P. A. (2021). The Other Great Migration: Southern Whites and the New Right. *NBER*, (w29506).

Bazzi, S., Gaduh, A., Rothenberg, A. D., and Wong, M. (2019). Unity in Diversity? How Intergroup Contact Can Foster Nation Building. *American Economic Review*, 109(11):3978–4025.

Bhandari, R., Feigenberg, B., Lubotsky, D., and Medina-Cortina, E. (2021). Projecting Trends in Undocumented and Legal Immigrant Populations in the United States. *NBER*, (NB21-16).

Blandhol, C., Bonney, J., Mogstad, M., and Torgovitsky, A. (2022). When is TSLS Actually

LATE? *NBER*, (w29709).

Blau, F. D. and Mackie, C. (2017). *The Economic and Fiscal Consequences of Immigration.* National Academies Press, Washington, D.C. Pages: 23550.

Borjas, G. and Edo, A. (2021). Gender, Selection into Employment, and the Wage Impact of Immigration. Technical Report w28682, National Bureau of Economic Research, Cambridge, MA.

Borjas, G. J. and Cassidy, H. (2019). The wage penalty to undocumented immigration. *Labour Economics*, 61:101757.

Borusyak, K. and Hull, P. (2020). Non-Random Exposure to Exogenous Shocks: Theory and Applications. *NBER*, (w27845).

Borusyak, K., Hull, P., and Jaravel, X. (2022). Quasi-Experimental Shift-Share Research Designs. *The Review of Economic Studies*, 89(1):181–213.

Boustan, L. P. (2010). Was Postwar Suburbanization "White Flight"? Evidence from the Black Migration. *Quarterly Journal of Economics*, 125(1):417–443.

Boustan, L. P., Fishback, P., and Kantor, S. (2010). The Effect of Internal Migration on Local Labor Markets: American Cities during the Great Depression. *Journal of Labor Economics*, 28(4):719–746.

Bruno, A. and Storrs, K. L. (2005). Consular Identification Cards: Domestic and Foreign Policy Implications, the Mexican Case, and Related Legislation. Technical report, Congressional Research Service, The Library of Congress, Washington, DC.

Burstein, A., Hanson, G., Tian, L., and Vogel, J. (2020). Tradability and the Labor-Market Impact of Immigration: Theory and Evidence From the United States. *Econometrica*, 88(3):1071–1112.

Caballero, M. E., Cadena, B. C., and Kovak, B. K. (2018). Measuring Geographic Migration Patterns Using Matrículas Consulares. *Demography*, 55(3):1119–1145.

Callaway, B. and Sant'Anna, P. H. (2021). Difference-in-Differences with multiple time periods. *Journal of Econometrics*, 225(2):200–230.

Card, D. (2005). Is the New Immigration Really so Bad? *The Economic Journal*, 115(507):F300–F323.

Card, D., Dustmann, C., and Preston, I. (2012). Immigration, Wages and Compositional Amenities. *Journal of the European Economic Association*, 10(1):78–119.

Clemens, M. A., Lewis, E. G., and Postel, H. M. (2018). Immigration Restrictions as Active Labor Market Policy: Evidence from the Mexican Bracero Exclusion. *American Economic Review*, 108(6):1468–1487.

Cortes, P. (2008). The Effect of Low-Skilled Immigration on U.S. Prices: Evidence from CPI Data. *Journal of Political Economy*, 116(3):381–422.

Couttenier, M., Hatte, S., Thoenig, M., and Vlachos, S. (2021). Anti-Muslim Voting and Media Coverage of Immigrant Crimes. *The Review of Economics and Statistics*, pages 1–33.

Cox, A. B. and Miles, T. J. (2013). Policing Immigration. *The University of Chicago Law Review*, 80(1):87–136.

Cunningham, S. (2021). *Causal Inference: The Mixtape*. Yale University Press.

Derenoncourt, E. (2022). Can You Move to Opportunity? Evidence from the Great Migration. *American Economic Review*, 112(2):369–408.

Dinarte Diaz, L. I., Jaume, D. J., Medina-Cortina, E., and Winkler, H. (2022). Neither by

Land nor by Sea : The Rise of Electronic Remittances during COVID-19. *World Bank*, 10057.

Durand, J. (2016). *Historia mínima de la migración México-Estados Unidos*. El Colegio de México.

Durand, J., Massey, D. S., and Zenteno, R. M. (2001). Mexican Immigration to the United States: Continuities and Changes. *Latin American Research Review*, 36(1):107–127. Publisher: Latin American Studies Association.

Dustmann, C., Vasiljeva, K., and Piil Damm, A. (2019). Refugee Migration and Electoral Outcomes. *The Review of Economic Studies*, 86(5):2035–2091.

East, C. N., Hines, A. L., Luck, P., Mansour, H., and Velásquez, A. (2022). The Labor Market Effects of Immigration Enforcement. https://docs.iza.org/dp11486.pdf.

Edo, A., Giesing, Y., Öztunc, J., and Poutvaara, P. (2019). Immigration and electoral support for the far-left and the far-right. *European Economic Review*, 115:99–143.

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

Enke, B., Rodríguez-Padilla, R., and Zimmermann, F. (2020). Moral Universalism and the Structure of Ideology. *NBER*, (w27511).

Enos, R. D. (2014). Causal effect of intergroup contact on exclusionary attitudes. *Proceedings of the National Academy of Sciences*, 111(10):3699–3704.

Enos, R. D. (2017). *The Space between Us: Social Geography and Politics*. Cambridge University Press.

Facchini, G. and Mayda, A. M. (2009). Does the Welfare State Affect Individual Attitudes toward Immigrants? Evidence across Countries. *Review of Economics and Statistics*, 91(2):295–314.

Fording, R., Soss, J., and Schram, S. (2007). Devolution, Discretion, and the Effect of Local Political Values on TANF Sanctioning. *Social Service Review*, 81(2):285–316.

Gaines, A. C., Hardy, B., and Schweitzer, J. (2021). How Weak Safety Net Policies Exacerbate Regional and Racial Inequality. Technical report, Center for American Progress.

Gainsborough, J. F. (2003). To Devolve or Not To Devolve? Welfare Reform in the States. *Policy Studies Journal*, 31(4):603–623.

Goldsmith-Pinkham, P., Sorkin, I., and Swift, H. (2020). Bartik Instruments: What, When, Why, and How. *American Economic Review*, 110(8):2586–2624.

Gonzalez-Barrera, A. (2021). Before COVID-19, more Mexicans came to the U.S. than left for Mexico for the first time in years. *Pew Research Center*. https://www.pewresearch.org/fact-tank/2021/07/09/before-covid-19-more-mexicans-came-to-the-u-s-than-left-for-mexico-for-the-first-time-in-years/.

Hainmueller, J. and Hopkins, D. J. (2014). Public Attitudes Toward Immigration. *Annual Review of Political Science*, 17(1):225–249.

Halla, M., Wagner, A. F., and Zweimüller, J. (2017). Immigration and Voting for the Far Right. *Journal of the European Economic Association*, 15(6):1341–1385.

Hanson, G. H. (2009). The Economic Consequences of the International Migration of Labor. *Annual Review of Economics*, 1(1):179–208.

Hanson, G. H., Scheve, K., and Slaughter, M. J. (2007). Public Finance and Individual Preferences over Globalization Strategies. *Economics & Politics*, 19(1):1–33.

Hanson, G. H. and Spilimbergo, A. (1999). Illegal Immigration, Border Enforcement, and Relative Wages: Evidence from Apprehensions at the U.S.-Mexico Border. *American*

Economic Review, 89(5):1337–1357.

Harmon, N. A. (2018). Immigration, Ethnic Diversity, and Political Outcomes: Evidence from Denmark. *The Scandinavian Journal of Economics*, 120(4):1043–1074.

Hill, S. J., Hopkins, D. J., and Huber, G. A. (2019). Local demographic changes and US presidential voting, 2012 to 2016. *Proceedings of the National Academy of Sciences*, 116(50):25023–25028.

Jaeger, D. A., Ruist, J., and Stuhler, J. (2018). Shift-Share Instruments and the Impact of Immigration.

Laglagaron, L. (2010). Protection through Integration: The Mexican Government's Efforts to Aid Migrants in the United States. Technical report, Migration Policy Institute, Washington, DC.

Lind, D. (2014). Why cities are rebelling against the Obama administration's deportation policies. *Vox*. https://www.vox.com/2014/6/6/5782610/secure-communities-cities-counties-ice-dhs-obama-detainer-reform.

Massey, D. S., Durand, J., and Pren, K. A. (2015). Border Enforcement and Return Migration by Documented and Undocumented Mexicans. *Journal of Ethnic and Migration Studies*, 41(7):1015–1040.

Massey, D. S., Rugh, J. S., and Pren, K. A. (2010). The Geography of Undocumented Mexican Migration. *Mexican Studies/Estudios Mexicanos*, 26(1):129–152.

Mathema, S. (2015). Providing Identification to Unauthorized Immigrants. The State and Local Landscape of Identification for Unauthorized Immigrants. Technical report, Center for American Progress.

Mayda, A. M., Peri, G., and Steingress, W. (2022a). The Political Impact of Immigration: Evidence from the United States. *American Economic Journal: Applied Economics*, 14(1):358–389.

Mayda, A. M., Senses, M., and Steingress, W. (2022b). The fiscal impact of immigration in the United States: Evidence at the local level.

Mitchell, C. (2011). Illinois County Defies Feds On Immigrant Detention. *NPR*.

Monras, J. (2015). Economic Shocks and Internal Migration. *SSRN Electronic Journal*.

Monras, J. (2020). Immigration and Wage Dynamics: Evidence from the Mexican Peso Crisis. *Journal of Political Economy*, 128(8):3017–3089.

Munshi, K. (2003). Networks in the Modern Economy: Mexican Migrants in the U. S. Labor Market. *The Quarterly Journal of Economics*, 118(2):549–599.

Márquez Lartigue, R. (2021). Public-Consular Diplomacy at Its Best: The Case of the Mexican Consular ID Card Program.

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

Otto, A. H. and Steinhardt, M. F. (2014). Immigration and election outcomes — Evidence from city districts in Hamburg. *Regional Science and Urban Economics*, 45:67–79.

Passel, J. S. and Cohn, D. (2018). U.S. Unauthorized Immigrant Total Dips to Lowest Level in a Decade. Technical report, Pew Research Center.

Peri, G. and Sparber, C. (2009). Task Specialization, Immigration, and Wages. *American Economic Journal: Applied Economics*, 1(3):135–169.

Riek, B. M., Mania, E. W., and Gaertner, S. L. (2006). Intergroup Threat and Outgroup Attitudes: A Meta-Analytic Review. *Personality and Social Psychology Review*, 10(4):336–353.

Rodrik, D. (2021). Why Does Globalization Fuel Populism? Economics, Culture, and the Rise of Right-Wing Populism. *Annual Review of Economics*, 13(1):133–170.

Rozo, S. V. and Vargas, J. F. (2021). Brothers or invaders? How crisis-driven migrants shape voting behavior. *Journal of Development Economics*, 150:102636.

Ruggles, S., Flood, S., Goeken, R., Schouweiler, M., and Sobek, M. (2022). IPUMS USA: Version 12.0. Version Number: 12.0 Type: dataset.

Sellars, E. A. (2019). Emigration and Collective Action. *The Journal of Politics*, 81(4):1210–1222.

Sequeira, S., Nunn, N., and Qian, N. (2020). Immigrants and the Making of America. *The Review of Economic Studies*, 87(1):382–419.

Tabellini, M. (2020). Gifts of the Immigrants, Woes of the Natives: Lessons from the Age of Mass Migration. *The Review of Economic Studies*, 87(1):454–486.

United Nations Department of Economic and Social Affairs (2021). *International Migration 2020: Highlights*. OCLC: 1302005218.

Valenzuela, A. (2003). Day Labor Work. *Annual Review of Sociology*, 29(1):307–333.

Wassink, J. and Massey, D. S. (2022). The New System of Mexican Migration: The Role of Entry Mode–Specific Human and Social Capital. *Demography*, 59(3):1071–1092.

Wiehe, M., Davis, A., Davis, C., Gardner, M., Gee, L. C., and Grundman, D. (2018). Who Pays? A Distributional Analysis of the Tax Systems in All 50 States, 6th Edition. Technical report, Institute on Taxation & Economic Policy.

Xu, Y. and Warner, M. E. (2016). Does devolution crowd out development? A spatial analysis of US local government fiscal effort. *Environment and Planning A: Economy and Space*, 48(5):871–890.

Appendix A Secure Communities

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

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

Once in place, we study the program as a dependent variable and examine the intensive margin. We ask whether new flows of unauthorized migrants change how local authorities use the program. As the program became established, it was subject to political manipulation. As of 2013, local authorities were required to participate in Secure Communities. However, before it was mandatory, the program became politicized. States tried to opt out. Some counties sought to circumvent the program by refusing to submit fingerprints for individuals with no or little criminal background (Mitchell, 2011). Other counties argued that detainers from ICE were requests that could be denied and announced they would decline. County officials argued that the program was facilitating deportation of non-criminals and undermining police relations in immigrant communities (Lind, 2014; Mitchell, 2011). In 2014 the program was scaled back after federal courts held that ICE detainers were optional,[15] and counties could be held liable for due process violations of individuals detained solely at the request of ICE.[16]

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

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

Appendix B Evolution of estimated number of unauthorized Mexican migrants

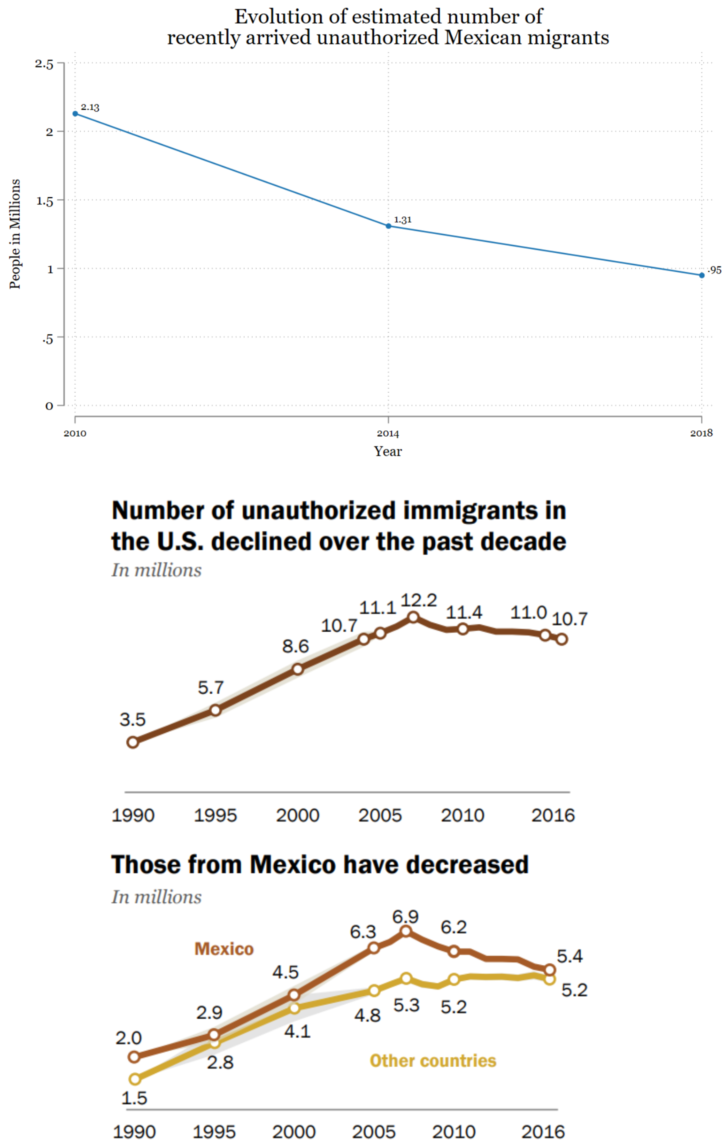

Figure 5: Evolution of estimated number of unauthorized Mexican migrants using Consular data. Source: Passel and Cohn (2018)

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

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

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

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

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

Appendix D Description of Specifications for Secure Communities

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

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

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

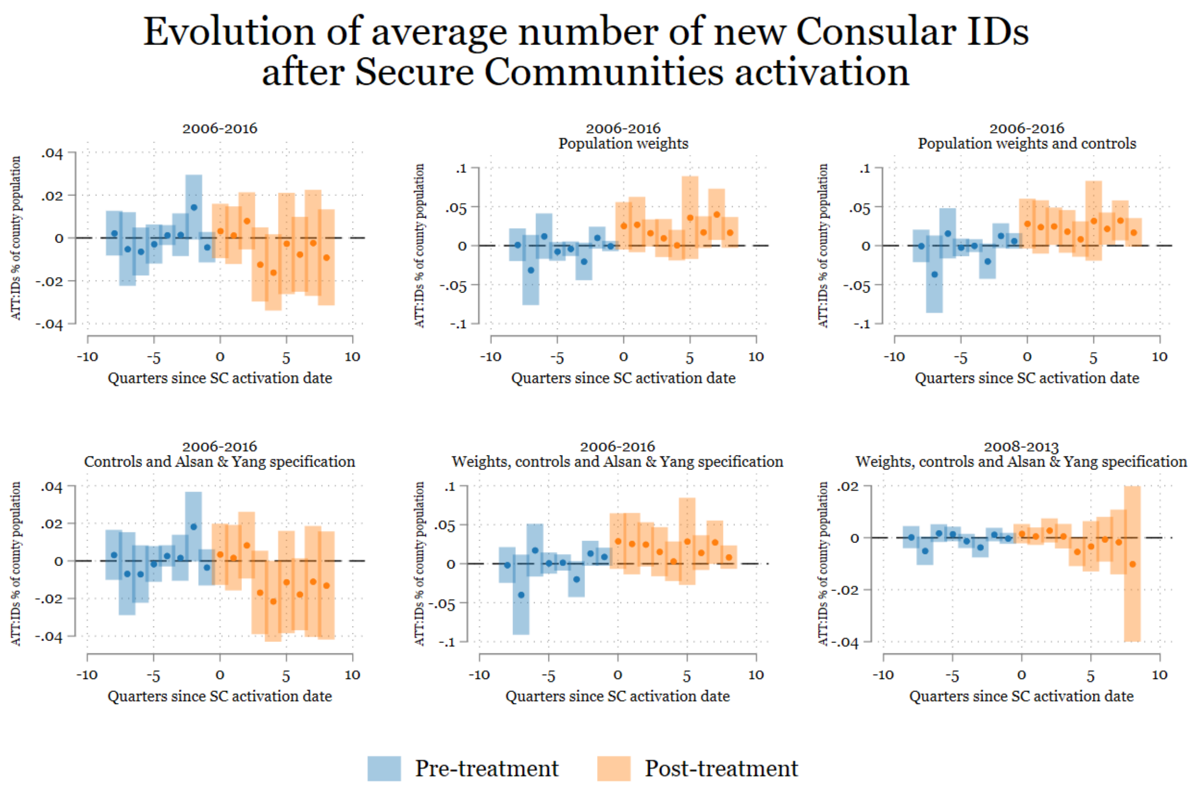

Figure 6: Secure Communities

Appendix E Authorized vs. unauthorized migrants

Table 11: Correlation between the LOO instrument and ACS 5 estimates of unauthorized and authorized recent Mexican migration

	(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

Sources: SRE, 2022 and Ruggles et al. (2022). The ACS 5 unauthorized sample is comprised of people born in Mexico without citizenship who arrived in the US less than five years before and with no college degree and between 16 and 64 years old. The ACS-5 authorized sample is comprised of people born in Mexico without citizenship who arrived in the US less than five years before with college degree. The Consular sample is comprised of unique new observations per period per CBSA. Standard errors clustered at the county level. Estimations control for county and state-year fixed effects and are weighted by predicted county population. Variables are proportion of county population.

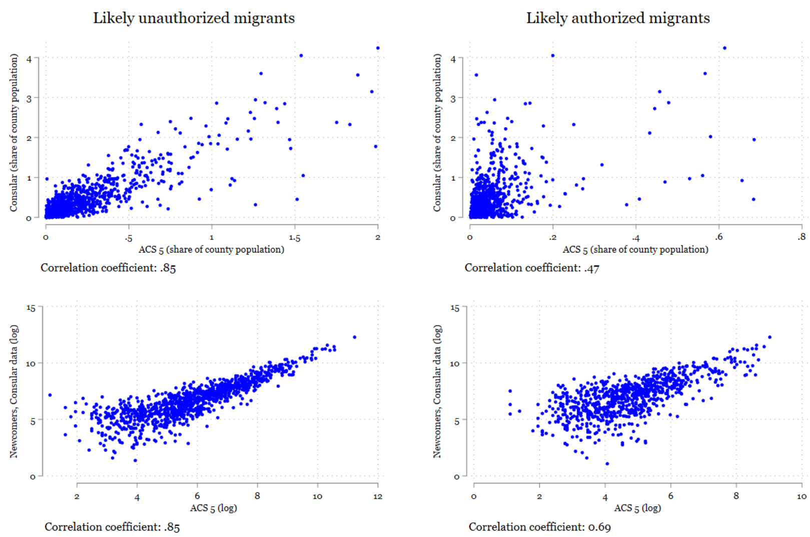

Figure 7: Comparison between estimates from Consular data and estimates of authorized and unauthorized migrants from ACS 5

Appendix F Correlation between observed migration and other populations

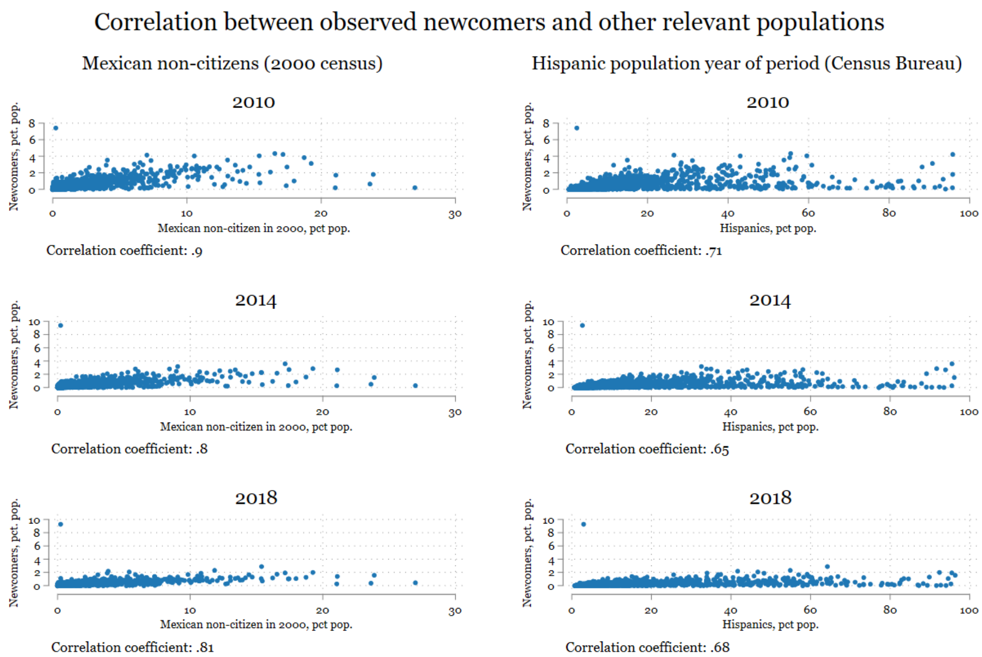

Figure 8: Correlation between observed migration and other populations

Appendix G Summary statistics

Table 12: Summary statistics

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

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

Appendix H Data for push factors

Our second identification strategy predicts migration from each Mexican municipality in each period. In particular, we regress the observed number of migrants on a set of time-varying variables. Then, we use the fitted values as the shifters, which we the interact with the original shares to construct the measure of predicted migration at the county level for each period. Our time-varying variables come from 4 different datasets.

1. **The University of Delaware's temperature and precipitation data**. We calculated the mean temperature and precipitation for each data point (recording station) within Mexico from 1950 until 2017. We then calculated the mean and the standard deviation of values for every period (2007-10, 2011-14 and 2015-17). To deal with municipalities with more than one data point, we took the average of all the points within one municipality. For the municipalities with no data points, we assigned them the values of the neighboring stations. Our dataset has information for 2,456 municipalities, for three periods, and the variables are mean period precipitation, mean period temperature, std period precipitation, std period temperature.

2. **The National Institute of Statistics and Geography's (INEGI) deaths data**. We used the yearly statistics of general deaths for the years 2005-2020. This dataset contains the number of deceases per municipality and the causes of such deceases. 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 municipality values for each of 2,493 municipalities –Mexico City's information comes at the delegation level, but we average the values in the final dataset– for each period (2007-10, 2011-14 and 2015-18).

3. **The National Institute of Statistics and Geography's (INEGI) Economic Census for 2009, 2014 and 2019**. Every five years, INEGI gathers data about the economic activity in each Mexican municipality corresponding to the previous year. Among others, the dataset has information on total investment, total production, number of employed people, wages, input expenditures and stocks for different sectors and subsectors. We constructed municipal totals for every year for the following variables: number of economic units, total production, value added, total investment, total workers, total women workers, total yearly hours worked, total employees and share of workers that are women. Moreover, for all the relevant variables, we obtained the per capita indicators. Our dataset has information for 2,465 municipalities for each period (2009, 2014 and 2019).

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. On the one hand, we use the dataset of poverty indicators. For every municipality, every year, there is data on the rates of poverty and extreme poverty, as well as indicators of underdevelopment in education, health, and housing. There is data from 2,469 municipalities for each period (2010, 2015 and 2020). On the other hand, we use the dataset of underdevelopment estimates. Among others,

it has information about the share of: adults that do not know how to read and write, children that do not go to school, households without basic health, households without concrete floors, households without toilets, households without electricity and households without washing machine. There is data from 2,469 municipalities for each period (2010, 2015 and 2020).

Once we had combined all these push factors, we created square terms to model potential non-linearities. The following table displays the summary statistics for these variables.

We then merged this dataset with the data on the observed number of migrants from each municipality in each period. Given that the variable of interest is censored at zero, we aimed to predict observed migration using a Poisson regression. To avoid over-fitting, however, we first implemented a Lasso correction. Out of the 54 variables included in the regression, 53 were selected, all but housing underdevelopment.

Table 13: Summary statistics, push factors

	Mean	Std	Min	Max	Obs
Share adults that cannot read	11.98	8.76	.73	66.55	7326
Share of kids no school	5.21	3.26	0	42.3	7326
Share of HH, no access to health	25.54	15.84	.88	98.14	7326
Share of HH, no concrete floors	9.83	10.21	0	79.37	7326
Share of HH, no toilets	6.19	7.9	0	91.81	7326
Share of HH, no electricity	2.98	4.56	0	68.69	7326
Share of HH, no washing machine	49.06	24.85	4.56	100	7326
Social underdevelopment	.01	1	-1.85	6.83	7326
Poverty rate	65.08	21.14	2.73	99.94	7320
Extreme poverty rate	20.93	18.42	0	97.46	7323
Education underdevelopment	28.44	10.43	2.63	65.38	7323
Health underdevelopment	24.65	15.9	.93	98.23	7323
Housing underdevelopment	44.66	30.19	.04	100	7323
Mean precipitation	81.83	51.59	6.34	348.14	7323
Std precipitation	10.45	6.78	.01	43.41	7323
Deviation from historic precip	-11.11	10.21	-66.43	84.01	7323
Mean temperature	20.248	4.13	9.85	29.867	7323
Std temperature	.29	.18	0	1.09	7323
Deviation from historic temp	.27	.39	-1.43	2.16	7323
Homicides	9.54	58.45	0	1998.25	7420
Change in homicides since 2005	5.33	37.88	-109.25	1723.5	7398
Homicide rate	18.05	31.08	0	840.34	7328
Neo-natal deaths	10.61	80.62	0	3977.41	7420
Neo-natal death rate	10.33	22.88	0	929.87	7328
Maternal death	.3	2.32	0	117.31	7420
Maternal death rate	.34	1.53	0	85.03	7328
Infant death rate	16.35	31.46	0	1298.36	7328
Economic units	1811.6	12823.49	2	613963.1	7331
Economic units, per capita	.03	.03	0	.4	7323
Total production	6463.16	72209.38	-558.43	4287579	7327
Total production, per capita	.04	.27	-.01	11.24	7319
Value added	2891.44	38738.59	-12455.91	2327419	7327
Value added, per capita	0	.13	-.14	7.33	7319
Total investment	225.29	3137.17	-16160.32	192598.2	7327
Total investment, per capita	0	.01	-.04	.39	7319
Total workers	9598.1	92629.86	5	4836362	7327
Total workers per capita	.1	.09	0	1.43	7319
Total women workers	3210.99	13721.62	2	259198	7324
Total women workers, per capita	.04	.04	0	.81	7316
Yearly hours worked	22753.07	222108.6	8.71	1.18e+07	7327
Yearly hours worked, per capita	.21	.2	0	2.82	7319
Total employees	5515.56	56280.58	0	2964527	7327
Total employees, per capita	.04	.06	0	1.41	7319
Share of women workers	47.73	10.94	6.43	98.65	7331
Population	51109.17	305502.4	93	1.38e+07	7328
Log population	9.45	1.56	4.53	16.44	7328
Population, square	9.59e+10	3.80e+12	8649	1.92e+14	7328
Homicide rate, square	1291.42	11645.62	0	706164.8	7328
Poverty rate, square	4681.93	2637.59	7.48	9987.65	7320
Extreme poverty rate, square	777.46	1252.2	0	9498.46	7323
Social underdevelop, square	.99	1.77	0	46.61	7326
Share adults that cannot read sq	220.32	332.33	.53	4428.55	7326
Total production, square	5.26e+09	2.45e+11	0	1.84e+13	7327
Mean temperature, square	427.06	167.66	97.02	892.02	7323

Appendix I Spatial auto-correlation in number of observed migrants

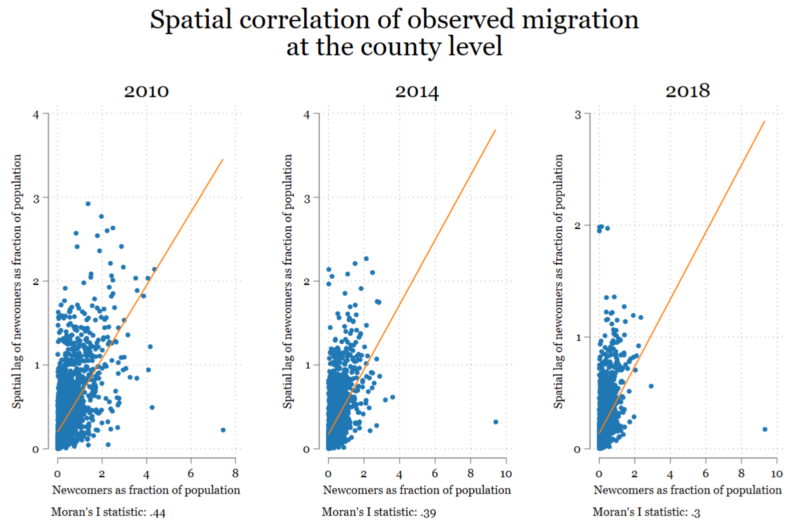

Figure 9: Spatial auto-correlation

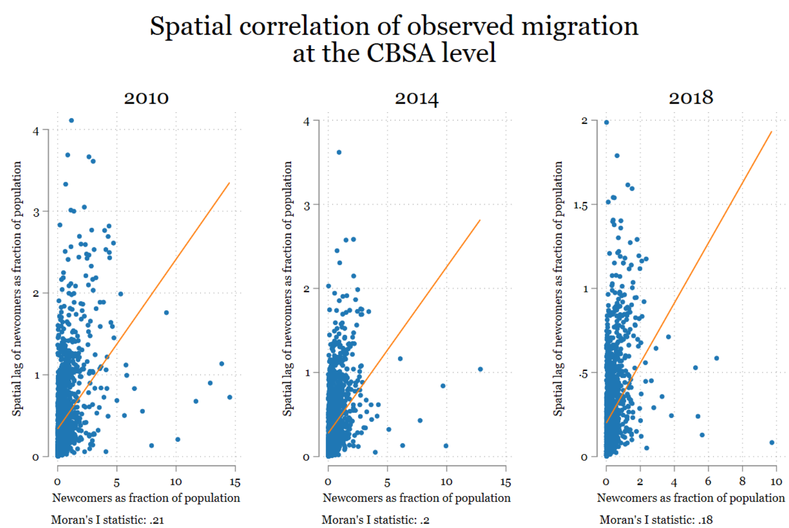

Figure 10: Spatial auto-correlation

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

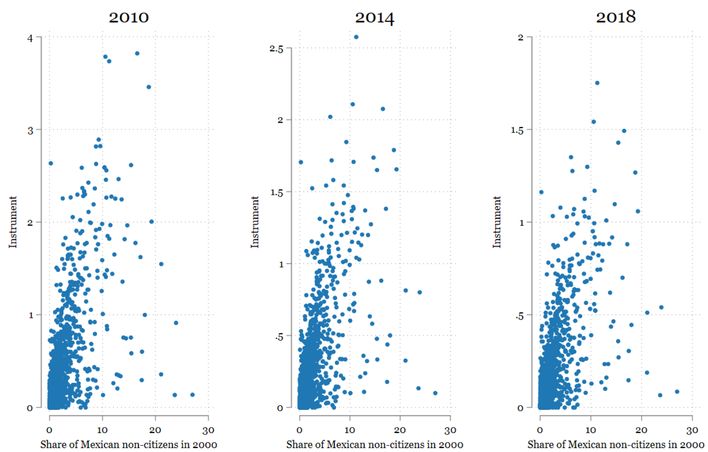

Figure 11: Correlation between instrument and estimated share of Mexican non-citizens in 2000

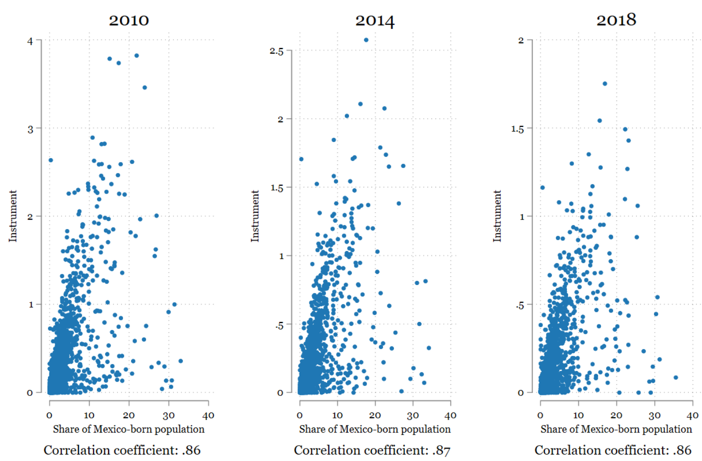

Figure 12: Correlation between instrument and estimated share of Mexico-born people at the beginning of period

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

Table 14: Association between economic prosperity and observed migration

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

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

Appendix L Short vs. long term effects

Table 15: Long vs short-term effects

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

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

Appendix M Alternative standard errors

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

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

Tables 16 presents the 2SLS results of these approaches with the main results. None of them consistently changes the significance.

Table 16: Alternative standard error calculation

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

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

Appendix N Effects on crime

The perception of immigrants as a criminal threat is widely theorized. Studying crime interrogates whether crime increases in response to unauthorized migrants and whether county officials are reasonable to invest in policing and the judiciary. Our crime information comes from the Jacob Kaplan's Concatenated Files, retrieved from the National Archive of Criminal Justice Data. This unofficial data-set condenses the information of yearly "Offenses Known and Clearances by Arrest (Return A)" by crime reported by the Uniform Crime Reporting Program Data. We use total crime, all crime included in the violent crime index, and all crime included in the property crime index. Since these crime data are noisy, we aggregate counts for 2010–11, 2014–15, and 2018–19. We construct our measures by dividing the counts by county population and then take the natural logarithm.

Table 17: Effects of arrival of unauthorized Mexican migrants on crime (2010-2018) and on immigration enforcement (2008-2013)

	Crime (log pc)		
	(1) All	(2) Violent	(3) Property
A. OLS			
Newcomers, pct. pop.	-0.01	-0.02	0.01
	(0.02)	(0.03)	(0.03)
B. 2SLS Loo			
Newcomers, pct. pop.	-0.02	-0.01	-0.01
	(0.02)	(0.03)	(0.03)
Std. Coefficient	-0.01	-0.01	-0.01
$\hat{\beta} * \bar{x}$	-0.01	-0.00	-0.00
C. 2SLS push factors			
Newcomers, pct. pop.	-0.04	-0.02	-0.03
	(0.02)	(0.03)	(0.03)
Std. Coefficient	-0.02	-0.01	-0.02
$\hat{\beta} * \bar{x}$	-0.02	-0.01	-0.01
Observations	7847	7789	7830
Dep. Var., Mean	-3.49	-5.87	-3.89
Dep. Var., Sd	0.94	1.02	0.92
Ind. Var., Mean	0.46	0.46	0.46
Ind. Var., Sd	0.60	0.60	0.60

Dependent variables are two year averages of the log of per capita total crime, violent crime index and property crime index. Sources: Jacob Kaplan's Concatenated Files: Uniform Crime Reporting Program Data: Offenses Known and Clearances by Arrest (Return A), 1960-2020. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population as described earlier. Source: SRE. Instruments are as described earlier. Standard errors are clustered at the CBSA level. The estimations control for county and state-period fixed effects and are weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

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

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

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

Columns 1 through 4 in Table 17 display the results of the analysis on Secure Communities. The baseline OLS estimates in Panel A indicate that the arrival of more unauthorized migrants in a county is associated with an increase of fingerprints submissions, an increase in matches (with persons in ICE's database) and with subsequent removals (deportations). The second stage and reduced form estimates are generally larger in magnitude (Panel B and C). The second stage estimates suggest that in response to a mean inflow of migrants, police departments increase the number of fingerprint submissions per foreign born population by 33% (Panel B, Column 1, std coeff: 0.27). Furthermore, counties increased the number of matches from ICE by 62% (Panel B, Column 2, std coeff: 0.38), and subsequent removals (deportations) increased by 62%, as well (Panel B, Column 3, std coeff: 0.33). These findings suggest that as more unauthorized migrants arrive in a county, police and sheriff's departments use Secure Communities more often 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. We are exploring additional sources of data to investigate the relationships further.

Appendix O Employment change and poverty

Table 18: Effects on poverty by changes in construction and hospitality employment

	Poverty rate (log)		Poverty rate (log)	
	(1) Below mean change construction	(2) Above mean change construction	(3) Below mean change hospitality	(4) Above mean change hospitality
A. OLS				
Newcomers, pct. pop.	0.09***	0.05***	0.05*	0.06***
	(0.02)	(0.01)	(0.02)	(0.01)
B. 2SLS Loo				
Newcomers, pct. pop.	0.13***	0.09***	0.13**	0.09***
	(0.02)	(0.02)	(0.04)	(0.02)
Std. Coefficient	0.19	0.13	0.11	0.14
$\hat{\beta} * \bar{x}$	0.04	0.04	0.03	0.04
C. 2SLS push factor				
Newcomers, pct. pop.	0.14***	0.08***	0.12**	0.09***
	(0.02)	(0.02)	(0.04)	(0.02)
Std. Coefficient	0.21	0.12	0.10	0.14
$\hat{\beta} * \bar{x}$	0.04	0.04	0.03	0.04
Observations	2184	5814	1098	6906
Dep. Var., Mean	2.80	2.58	2.63	2.59
Dep. Var., Sd	0.40	0.38	0.40	0.39
Ind. Var., Mean	0.29	0.48	0.23	0.47
Ind. Var., Sd	0.59	0.59	0.33	0.60
Inst., Mean	0.17	0.44	0.12	0.42
Inst., Sd	0.40	0.58	0.22	0.57

Dependent variable is log of poverty rate. Observations in Columns 1-2 are divided according to whether they are above or below the median of growth in employment in construction. Observations in Columns 3-4 are divided according to whether they are above or below the median of growth in employment in hospitality and leisure. Sources: US Census Bureau: Population Division and Small Area; QCEW. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population as described in Section 3. Source: SRE. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

Appendix P Substitution between effects on values and out-migration

Table 19: Effects of arrival of unauthorized Mexican migrants 2010-2018 on values and out-migration, by poverty

	Universalist values		Out-migration pc (log)	
	(1) Below median poverty rate	(2) Above median poverty rate	(3) Below median poverty rate	(4) Above median poverty rate
A. OLS				
Newcomers, pct. pop.	-0.03	-0.10	0.05***	0.00
	(0.05)	(0.08)	(0.01)	(0.02)
B. 2SLS Loo				
Newcomers, pct. pop.	-0.06	-0.18*	0.04*	0.03
	(0.06)	(0.09)	(0.02)	(0.02)
Std. Coefficient	-0.06	-0.24	0.08	0.06
$\hat{\beta} * \bar{x}$	-0.02	-0.10	0.01	0.02
C. 2SLS push factor				
Newcomers, pct. pop.	-0.07	-0.19*	0.04*	0.02
	(0.06)	(0.08)	(0.02)	(0.02)
Std. Coefficient	-0.08	-0.25	0.08	0.04
$\hat{\beta} * \bar{x}$	-0.03	-0.11	0.02	0.01
Observations	3195	2514	4009	4005
Dep. Var., Mean	0.11	0.20	-2.89	-3.00
Dep. Var., Sd	0.47	0.52	0.25	0.31
Ind. Var., Mean	0.39	0.57	0.39	0.55
Ind. Var., Sd	0.51	0.68	0.51	0.67
Inst., Mean	0.36	0.51	0.36	0.49
Inst., Sd	0.50	0.65	0.50	0.64

The dependent variables in Columns 1-2 is the average relative importance of universalist values, taken from by Enke (2020). The dependent variable in Columns 3-4 out-migration, calculated as the log of out-migration divided by county population. Columns 1-3 and 2-4 are divided according to whether they are above or below the median of poverty rate in the state in the period. Around 500 counties are below median in some periods and above median in others. To avoid singletons, we take the mode value per county. Sources: Enke, (2020); US Census Bureau: Population Division and Small Area; US Census Bureau: 2007-2011, 2011-2015, and 2015-2019 American Community Surveys 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. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

Appendix Q Robustness checks for mechanisms

Table 20: Robustness checks for mechanisms

	Emp (log)		Wages		Log of rate		Log	Values
	(1) Const	(2) Hosp and leis	(3) Const	(4) Hosp and leis	(5) Poverty	(6) SNAP	(7) Out-mig	(8) Universalist
A. Reduced form, baseline								
Instrument	-0.06**	-0.02*	-26.18	-9.35	0.11***	0.07**	0.04***	-0.16**
	(0.02)	(0.01)	(14.80)	(7.42)	(0.02)	(0.02)	(0.01)	(0.05)
B. Lagged outcome (LO)								
Instrument	0.03	-0.01	-1.74	-17.55**	-0.07**	0.02	0.13***	
	(0.02)	(0.02)	(9.10)	(5.96)	(0.03)	(0.01)	(0.03)	
C. Mex non-citizen, sh								
Instrument	-0.14***	0.01	-56.51***	-21.43**	0.13***	0.12***	0.04	-0.21**
	(0.03)	(0.01)	(15.17)	(6.60)	(0.02)	(0.03)	(0.02)	(0.07)
D. Hispanics, sh								
Instrument	-0.08***	-0.01	-39.46**	-7.84	0.11***	0.09***	0.04**	-0.18***
	(0.02)	(0.01)	(13.40)	(7.10)	(0.02)	(0.02)	(0.01)	(0.05)
E. Adult HS completion								
Instrument	-0.07***	-0.02	-34.09*	-9.79	0.12***	0.08***	0.04**	-0.19***
	(0.02)	(0.01)	(14.08)	(7.06)	(0.02)	(0.02)	(0.01)	(0.05)
F. China shock								
Instrument	-0.06**	-0.02	-21.65	-4.84	0.10***	0.08***	0.05***	-0.12*
	(0.02)	(0.01)	(15.00)	(7.36)	(0.02)	(0.02)	(0.01)	(0.05)
G. Simulated instrument								
Instrument	-0.17***	0.02	-24.52	7.68	0.10***	0.18***	0.08*	-0.28**
	(0.04)	(0.03)	(25.61)	(11.50)	(0.02)	(0.03)	(0.04)	(0.10)
H. Spatial lag								
Instrument	-0.06*	0.00	-16.45	-6.57	0.10***	0.07**	0.03*	-0.14*
	(0.03)	(0.01)	(16.00)	(7.50)	(0.02)	(0.03)	(0.01)	(0.06)
I. Stock Mex foreign								
Instrument	-0.06**	-0.02*	-24.67	-9.17	0.11***	0.06**	0.04**	-0.13*
	(0.02)	(0.01)	(14.78)	(7.43)	(0.02)	(0.02)	(0.01)	(0.05)
J. Stock Hispanics								
Instrument	-0.06**	-0.04*	-31.87*	-16.54*	0.12***	0.09***	0.03*	-0.17**
	(0.02)	(0.02)	(14.58)	(8.10)	(0.02)	(0.03)	(0.01)	(0.05)
K. No-outliers								
Instrument	-0.07*	-0.04**	-30.75	-16.06*	0.14***	0.09**	0.06***	-0.19**
	(0.03)	(0.01)	(17.19)	(7.23)	(0.02)	(0.03)	(0.02)	(0.07)
L. No pop weights								
Instrument	-0.09**	-0.01	-37.68***	-3.95	0.13***	0.11***	0.01	-0.25**
	(0.03)	(0.01)	(11.12)	(3.13)	(0.01)	(0.02)	(0.03)	(0.09)
*M. County-group * period FE*								
Instrument	-0.02	-0.01	-8.87	2.96	0.12***	0.10***	0.01	-0.11
	(0.04)	(0.02)	(17.33)	(6.01)	(0.02)	(0.03)	(0.04)	(0.14)

Dependent variables in columns 1-2 are the the log of average annual employment divided by working age population. Dependent variables in column 3-4 are the annual average weekly wages in 2010 USD. Column 5 is the log of poverty rate the year after the end of the period. Column 6 is the log of SNAP coverage the year after the end of the period. The dependent variable in Column 7 is out-migration, calculated as the log of out-migration divided by county population. Column 8 is the average county value, as reported by Enke (2020). All estimations are reduced form. Sources: Dave Leip's US Election Data; Annual Survey of State and Local Government Finances; US Census; ACS 5 from the Social Explorer; USDA's Economic Research Service; Peter K. Schott's Data; County Business Patterns; Acemoglu et al. (2016). Enke 2020. and QCEW. Panel 1 is reduced form estimation. Panel 2 estimates effect on lagged dependent variables. Panel 3 controls for pre-2007 features interacted with period dummies. Panel 4 includes simulated instrument following Borusyak and Hull (2020). Panel 5 controls for the 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<0.05,**p<0.01,***p<0.001

Appendix R Alternative measures of progressive taxation or safety net

Table 21: Political and policy effects by state tax equality

	Midterms, GOP		President, GOP		Edu (log pc)		Police, share		Judicial, share	
	(1) Low tax equality	(2) High tax equality	(3) Low tax equality	(4) High tax equality	(5) Low tax equality	(6) High tax equality	(7) Low tax equality	(8) High tax equality	(9) Low tax equality	(10) High tax equality
A. OLS										
Newcomers, pct. pop.	6.31***	6.70***	4.22***	2.16*	-0.04	-0.02	0.39**	0.05	0.23*	0.04
	(0.83)	(1.52)	(0.91)	(0.86)	(0.02)	(0.02)	(0.15)	(0.17)	(0.12)	(0.12)
B. 2SLS Loo										
Newcomers, pct. pop.	8.34***	8.63***	6.38***	2.49*	-0.06*	-0.05	0.75***	0.18	0.44***	0.14
	(1.05)	(1.76)	(0.88)	(0.98)	(0.02)	(0.02)	(0.22)	(0.17)	(0.13)	(0.14)
Std. Coefficient	0.28	0.25	0.25	0.09	-0.16	-0.08	0.25	0.07	0.32	0.14
$\hat{\beta} * \bar{x}$	3.57	4.30	2.78	1.31	-0.03	-0.03	0.38	0.11	0.22	0.08
C. 2SLS push factor										
Newcomers, pct. pop.	8.07***	7.66***	6.26***	2.43**	-0.05	-0.03	0.78***	0.11	0.44***	0.06
	(1.16)	(1.73)	(0.92)	(0.88)	(0.02)	(0.02)	(0.21)	(0.18)	(0.13)	(0.14)
Std. Coefficient	0.27	0.22	0.24	0.09	-0.13	-0.05	0.26	0.05	0.32	0.06
$\hat{\beta} * \bar{x}$	3.46	3.82	2.73	1.28	-0.02	-0.02	0.40	0.07	0.22	0.03
Observations	4650	3345	4331	2905	3108	2220	3114	2220	3090	2176
Dep. Var., Mean	52.85	43.39	50.25	41.10	1.87	2.07	5.48	5.42	1.61	1.18
Dep. Var., Sd	17.53	20.11	15.52	15.97	0.24	0.40	1.99	1.70	0.92	0.70
Ind. Var., Mean	0.43	0.50	0.44	0.52	0.51	0.60	0.51	0.60	0.51	0.60
Ind. Var., Sd	0.59	0.59	0.60	0.61	0.67	0.69	0.66	0.69	0.67	0.69
Inst., Mean	0.37	0.46	0.38	0.48	0.43	0.53	0.42	0.53	0.42	0.53
Inst., Sd	0.52	0.61	0.54	0.61	0.59	0.68	0.58	0.68	0.59	0.68

Dependent variable in columns 1–2 is the share of GOP vote in midterm House elections. Dependent variables in columns 3–4 is the share of GOP vote in Presidential elections. Dependent variables in columns 5–6 is the in log 2010 dollars if per child education expenditures. Dependent variables in columns 7–8 and 9–10 are shares of total direct expenditures in the police and the judiciary respectively. Observations in columns 1, 3, 5, 7 and 9, and 2, 4, 6, 8, and 10 are divided according to whether they are above or below the median of state tax equality according to the Institute for Taxation and Economic Policy. Above suggests a more progressive fiscal policy. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

Table 22: Effects on employment, poverty, out-migration and moral values by state tax equality

	Emp, manufacturing		Emp, construction		Emp, leisure & hosp		Poverty rate		Universalist values	
	(1) Low tax equality	(2) High tax equality	(3) Low tax equality	(4) High tax equality	(5) Low tax equality	(6) High tax equality	(7) Low tax equality	(8) High tax equality	(9) Low tax equality	(10) High tax equality
A. OLS										
Newcomers, pct. pop.	0.05***	0.09***	-0.06**	-0.00	-0.00	-0.02*	0.09***	0.04*	-0.15*	-0.04
	(0.02)	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.06)	(0.05)
B. 2SLS Loo										
Newcomers, pct. pop.	0.07**	0.08**	-0.09**	-0.02	-0.01	-0.03	0.13***	0.07**	-0.22***	-0.06
	(0.02)	(0.03)	(0.03)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.07)	(0.06)
Std. Coefficient	0.05	0.07	-0.11	-0.03	-0.02	-0.03	0.21	0.10	-0.26	-0.07
$\hat{\beta} * \bar{x}$	0.03	0.04	-0.04	-0.01	-0.01	-0.01	0.05	0.03	-0.10	-0.03
C. 2SLS push factor										
Newcomers, pct. pop.	0.09***	0.10***	-0.07**	-0.00	-0.01	-0.03**	0.12***	0.06**	-0.23***	-0.08
	(0.02)	(0.02)	(0.03)	(0.03)	(0.01)	(0.01)	(0.02)	(0.02)	(0.07)	(0.05)
Std. Coefficient	0.07	0.08	-0.09	-0.00	-0.01	-0.05	0.20	0.09	-0.27	-0.10
$\hat{\beta} * \bar{x}$	0.04	0.05	-0.03	-0.00	-0.00	-0.02	0.05	0.03	-0.10	-0.04
Observations	4253	3123	4260	3128	4609	3297	4674	3345	3114	2598
Dep. Var., Mean	-3.00	-3.13	-3.60	-3.63	-2.73	-2.78	2.66	2.53	0.10	0.20
Dep. Var., Sd	0.73	0.72	0.48	0.41	0.42	0.44	0.36	0.40	0.51	0.48
Ind. Var., Mean	0.43	0.50	0.43	0.50	0.43	0.50	0.43	0.50	0.44	0.50
Ind. Var., Sd	0.59	0.59	0.59	0.59	0.59	0.59	0.59	0.59	0.60	0.59
Inst., Mean	0.37	0.46	0.37	0.46	0.37	0.46	0.37	0.46	0.38	0.47
Inst., Sd	0.53	0.61	0.53	0.61	0.52	0.61	0.52	0.60	0.53	0.61

Dependent variable in columns 1–2 is the log of per working age employment rate in manufacturing. Dependent variable in columns 3–4 is the log of per working age employment rate in construction. Dependent variable in columns 5–6 is the log of per working age employment rate in leisure and hospitality. Dependent variable in columns 7–8 is the log of poverty rate. Dependent variable in columns 9–10 is the prevalence of universalist values following Enke (2020). Observations in columns 1, 3, 5, 7 and 9, and 2, 4, 6, 8, and 10 are divided according to whether they are above or below the median of state tax equality according to the Institute for Taxation and Economic Policy. Above suggests a more progressive fiscal policy. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

Table 23: Political and policy effects by state Tanf-poverty ratio

	Midterms, GOP		President, GOP		Edu (log pc)		Police, share		Judicial, share	
	(1) Low Tanf ratio	(2) High Tanf ratio	(3) Low Tanf ratio	(4) High Tanf ratio	(5) Low Tanf ratio	(6) High Tanf ratio	(7) Low Tanf ratio	(8) High Tanf ratio	(9) Low Tanf ratio	(10) High Tanf ratio
A. OLS										
Newcomers, pct. pop.	6.66***	6.24**	3.89***	2.11*	-0.03*	-0.02	0.30*	0.07	0.10	0.16*
	(0.81)	(1.91)	(0.77)	(1.07)	(0.02)	(0.02)	(0.14)	(0.19)	(0.14)	(0.08)
B. 2SLS Loo										
Newcomers, pct. pop.	8.39***	8.66***	5.24***	3.14*	-0.05**	-0.05	0.57**	0.22	0.24	0.29***
	(1.09)	(2.11)	(0.79)	(1.25)	(0.02)	(0.03)	(0.20)	(0.18)	(0.16)	(0.08)
Std. Coefficient	0.29	0.26	0.21	0.12	-0.16	-0.09	0.20	0.09	0.19	0.24
$\hat{\beta} * \bar{x}$	4.52	3.47	2.98	1.29	-0.03	-0.02	0.37	0.10	0.16	0.14
C. 2SLS push factor										
Newcomers, pct. pop.	8.22***	7.24***	5.15***	3.03**	-0.04*	-0.03	0.58**	0.14	0.26	0.16*
	(1.15)	(2.06)	(0.80)	(1.15)	(0.02)	(0.02)	(0.19)	(0.20)	(0.17)	(0.08)
Std. Coefficient	0.28	0.22	0.21	0.11	-0.13	-0.05	0.20	0.06	0.20	0.13
$\hat{\beta} * \bar{x}$	4.43	2.90	2.92	1.24	-0.03	-0.01	0.37	0.07	0.17	0.07
Observations	4875	3120	4344	2892	3264	2064	3262	2072	3218	2048
Dep. Var., Mean	54.68	42.74	51.68	41.27	1.80	2.11	5.78	5.16	1.39	1.42
Dep. Var., Sd	18.17	18.78	15.70	15.44	0.22	0.36	1.98	1.68	0.89	0.82
Ind. Var., Mean	0.54	0.40	0.57	0.41	0.64	0.48	0.64	0.47	0.65	0.47
Ind. Var., Sd	0.62	0.56	0.64	0.57	0.69	0.66	0.69	0.66	0.69	0.66
Inst., Mean	0.37	0.46	0.38	0.48	0.43	0.53	0.42	0.53	0.42	0.53
Inst., Sd	0.52	0.61	0.54	0.61	0.59	0.68	0.58	0.68	0.59	0.68

Dependent variable in columns 1–2 is the share of GOP vote in midterm House elections. Dependent variables in columns 3–4 is the share of GOP vote in Presidential elections. Dependent variables in columns 5–6 is the in log 2010 dollars if per child education expenditures. Dependent variables in columns 7–8 and 9–10 are shares of total direct expenditures in the police and the judiciary respectively. Observations in columns 1, 3, 5, 7 and 9, and 2, 4, 6, 8, and 10 are divided according to whether they are above or below the median of state Tanf-poverty ratio according to the Center for American Progress. Above suggests a more progressive fiscal policy. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

Table 24: Effects on employment, poverty, out-migration and moral values by state Tanf-poverty ratio

	Emp, manufacturing		Emp, construction		Emp, leisure & hosp		Poverty rate		Universalist vales	
	(1) Low Tanf ratio	(2) High Tanf ratio	(3) Low Tanf ratio	(4) High Tanf ratio	(5) Low Tanf ratio	(6) High Tanf ratio	(7) Low Tanf ratio	(8) High Tanf ratio	(9) Low Tanf ratio	(10) High Tanf ratio
A. OLS										
Newcomers, pct. pop.	0.06***	0.11**	-0.05**	0.01	-0.00	-0.04**	0.09***	0.02	-0.14**	-0.01
	(0.01)	(0.03)	(0.02)	(0.03)	(0.01)	(0.01)	(0.01)	(0.02)	(0.05)	(0.06)
B. 2SLS Loo										
Newcomers, pct. pop.	0.06***	0.10*	-0.07**	-0.02	-0.01	-0.04*	0.12***	0.05*	-0.21***	-0.01
	(0.02)	(0.04)	(0.02)	(0.03)	(0.01)	(0.02)	(0.02)	(0.03)	(0.06)	(0.06)
Std. Coefficient	0.05	0.08	-0.09	-0.03	-0.01	-0.05	0.21	0.07	-0.25	-0.02
$\hat{\beta} * \bar{x}$	0.03	0.04	-0.04	-0.01	-0.01	-0.02	0.06	0.02	-0.12	-0.01
C. 2SLS push factor										
Newcomers, pct. pop.	0.07***	0.12***	-0.06**	0.01	-0.00	-0.05**	0.11***	0.05	-0.22***	-0.04
	(0.02)	(0.03)	(0.02)	(0.03)	(0.01)	(0.02)	(0.02)	(0.03)	(0.06)	(0.06)
Std. Coefficient	0.06	0.10	-0.08	0.01	-0.01	-0.07	0.20	0.07	-0.25	-0.06
$\hat{\beta} * \bar{x}$	0.04	0.05	-0.03	0.00	-0.00	-0.02	0.06	0.02	-0.12	-0.02
Observations	4397	2979	4427	2961	4804	3102	4899	3120	3143	2569
Dep. Var., Mean	-3.07	-3.06	-3.58	-3.65	-2.73	-2.77	2.68	2.52	0.04	0.24
Dep. Var., Sd	0.75	0.71	0.49	0.40	0.46	0.40	0.36	0.40	0.53	0.45
Ind. Var., Mean	0.54	0.40	0.54	0.40	0.54	0.40	0.54	0.40	0.55	0.40
Ind. Var., Sd	0.62	0.56	0.62	0.56	0.62	0.56	0.62	0.56	0.62	0.56
Inst., Mean	0.37	0.46	0.37	0.46	0.37	0.46	0.37	0.46	0.38	0.47
Inst., Sd	0.53	0.61	0.53	0.61	0.52	0.61	0.52	0.60	0.53	0.61

Dependent variable in columns 1–2 is the log of per working age employment rate in manufacturing. Dependent variable in columns 3–4 is the log of per working age employment rate in construction. Dependent variable in columns 5–6 is the log of per working age employment rate in leisure and hospitality. Dependent variable in columns 7–8 is the log of poverty rate. Dependent variable in columns 9–10 is the prevalence of universalist values following Enke (2020). Observations in columns 1, 3, 5, 7 and 9, and 2, 4, 6, 8, and 10 are divided according to whether they are above or below the median of state Tanf-poverty ratio according to the Center for American Progres. Above suggests a more progressive fiscal policy. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.05,**p<0.01,***p<0.001

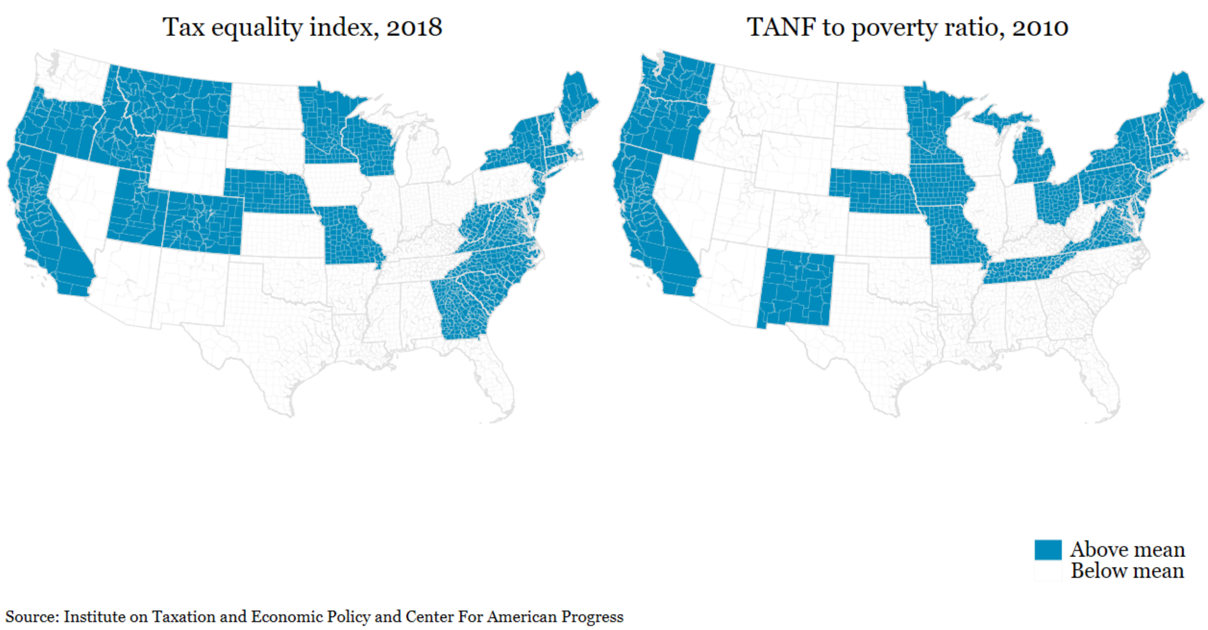

Figure 13: Map of categories of states