Unpacking the political impact of low-skilled immigration *

Wagner F. Oliveira†

Dec 2024

Abstract

Previous evidence shows that low-skilled immigration can induce electoral shifts to the right-wing. However, it is still unclear whether this is purely a short-run phenomenon, or whether it tends to persist over time. It is also unclear whether native economic responses to immigration shocks can explain this voting behavior. This paper aims at filling those two gaps by looking at presidential elections in the United States between 1992 and 2016. Our identification strategy relies on dynamic shift-share instruments, which allow us to disentangle between short-run effects and longer-term adjustments. We show that unemployment among low-skilled natives and local welfare expenditure per capita increase in the short run as a result of low-skilled immigration, but those effects fade over time, while voting is persistently shifted towards Republicans. Causal mediation estimates suggest that 60% of the short-run voting effect can be explained by those variables, but if these were the only channels at play, we would expect some attenuation in electoral shifts. Survey evidence points to the impressionable years hypothesis as a possible mechanism behind those findings.

Keywords: immigration, voting behavior, labor market, public finance.
JEL Codes: F22, J61, D72, H72

*I am grateful to Francisco Costa for all support and guidance. I also thank Samuel Bazzi, Pablo Muñoz, Raphael Corbi, Rodrigo Soares, Claudio Labanca, Sandra Rozo, Katrin Sommerfeld, Michael Hankinson, Bruno Barsanetti, Vasco Yasenov, Rodrigo Pinto, Marcelo Sant'Anna, André Trindade, Sophie Mathes, and participants at the Junior Economics of Migration Seminar (2022), 2021 EWMES, 2021 NEUDC, 2021 LACEA-LAMES, 2021 UEA Summer School, European and North American meetings and FGV EPGE's Applied Micro workshops for valuable comments and suggestions.

†PhD candidate at FGV EPGE - Brazilian School of Economics and Finance, Rio de Janeiro, Brazil. E-mail: *wagner.oliveira@fgv.edu.br*. This study was financed in part by the Coordenação de Aperfeiçoamento de Pessoal de Nível Superior – Brasil (CAPES) – Finance Code 001.

1 Introduction

In his inaugural speech, on January 20th 2017, former United States' President Donald Trump said that "we must protect our borders from the ravages of other countries (...) destroying our jobs. Protection will lead to great prosperity and strength. (...) We will bring back our borders. We will bring back our wealth, and we will bring back our dreams. (...). We will get our people off of welfare and back to work, rebuilding our country with American hands and American labor" (Politico.com, 2017). Not only in his speech, but throughout his whole campaign, the anti-immigration stance was clear and typically justified rhetorically on economic grounds. In the U.S., like in other high-income countries, immigration is a salient and polarizing theme on the political agenda. This is not a coincidence to the fact that those countries host large foreign-born populations.

There is a growing body of evidence showing that more immigration[1] is associated with shifts to the political right-wing, typically associated with anti-immigrant discourses (Alesina & Tabellini, 2021). The skill level of immigrants is an important driver of those effects. Previous evidence shows that only low-skilled immigration (those without a high school degree) shifts voters towards anti-immigration platforms, while high-skilled immigration has the opposite effect (Mayda et al., 2022).

However, immigration shocks may have short-run consequences and also trigger dynamic adjustments over time. In terms of political effects, our prior is that large and sudden inflows are more likely to trigger backlashes, while earlier waves of immigrants have a higher chance of integration and can thus have opposite effects[2]. Besides that, dynamic aspects are also present in the economic consequences of immigration for the host country. The interplay of those phenomena might be relevant to the understanding of the possible economic roots of the political impact of immigration.

[1] We use the word "immigrants" to refer both to economic migrants and refugees, referring to the host country. Our paper does not discriminate among these two categories, although there might be differences in how people politically react to economic or refugee migration. For the specific case of refugees, see Becker and Ferrara (2019).

[2] Similar questions arise in research intended to test if Allport (1954)'s contact hypothesis verifies in practice. For example Steinmayr (2021)'s findings for Austria suggest that mere exposure to refugees generates political backlashes, while close contact and sustained interactions do not.

This paper aims at disentangling the short-run political response to contemporaneous low-skilled immigration increases from the dynamic adjustment component from earlier waves of immigration. Also, we want to understand to what extent this effect is driven by changes in the living conditions of natives. In other words, we ask: do natives respond to economic shocks brought by immigration by voting for candidates that explicitly have an anti-immigration platform? Or does immigration affect voting only through non-economic concerns?

To correctly identify the short-term causal effects of immigration and its longer-term[3] adjustment component, we use a dynamic shift-share instrumental variable (SSIV) strategy, given that standard Bartik estimates typically employed in migration studies might conflate these two effects Jaeger et al. (2019). We apply the same framework to both political and economic outcomes to see if there are relevant changes in natives' economic conditions taking place *pari passu* with voting behavior shifts. We then provide a measure of the contribution of those economic changes to explain the political backlash using a causal mediation analysis framework for IV settings in which the same instrument is used to identify both intermediary and final outcomes (Dippel, Gold, et al., 2020, 2022).

Our setting is a panel of U.S. commuting zones, economic-defined areas that are geographically adequate to study labor market effects[4]. We analyze presidential election years from 1992 to 2016. Our main political outcome is the Republican vote share for presidents. In the U.S., the Republican Party is historically associated with a more restrictive attitude towards immigration, as shown by Tabellini (2020).

We consider two possible sets of economic mediators. We first look at native labor market outcomes, which is a natural choice given this is one of the most discussed issues in the migration literature. We analyze commonly used variables such as unemployment rates and hourly wages, breaking them down into specific groups according to

[3]Whenever we refer to longer-term or medium-run effects, we are analyzing the period in which dynamic adjustments from immigration take place, which is typically within a decade Jaeger et al. (2019). This is different from analyzing historical long-run effects of immigration as in Giuliano and Tabellini (2021), for example.

[4]Besides that, the economic variables we use are not available at the county level with a citizenship breakdown.

skill levels. We then look at the provision of public services by local governments, given that competition for public resources is frequently used as a ground for anti-immigration reasoning. We look both at revenue and expenditure sides, breaking them down into categories such as taxes, intergovernmental transfers, education and welfare spending.

We find a positive effect of low-skilled immigration on Republican voting taking place mostly in the short run. We also find that the economic conditions of specific groups of natives have been affected in the short run. This particular type of immigration also increased unemployment among low-skilled natives and local welfare spending per capita. This is suggestive evidence that economic factors are relevant to understanding the short-run shift to the right due to immigration. In line with those findings, we show that effects are higher in places with a larger ratio of low- to high-skilled natives and a larger share of government expenditure over income before our period of study. Those heterogeneous patterns corroborate the idea that some groups of natives might see immigrants as competitors for jobs and public resources in the short run.

However, in the medium run, a major part of those economic effects fades away, possibly due to general equilibrium adjustment dynamics, while the effect on voting persists. We interpret this as evidence of a hysteresis phenomenon in which voters can change political preferences permanently from temporary economic shocks.

Our mediation analysis results reinforce those findings. After summarizing economic outcomes in a latent variable, we estimate that 60% of the short-run voting effect can be explained by a combination of employment and welfare spending changes. In the medium run, adjustment dynamics in economic variables would imply a higher reduction of the Republican shift over time if this channel was the only relevant one. This reinforces evidence of persistence in voting behavior that cannot be explained only by changes in the economic conditions of natives. The policy implication of this finding is that targeting only the perception of natives towards immigrants might not be sufficient. We must also clearly understand who are the winners and losers from temporary shocks related to immigration in the short run.

To better understand why the voters' shift is persistent, we analyzed individual-

level survey data. We find that after 2008, the association between identifying as a Republican and being less liberal toward immigration intensified. There are two different trends taking place in this period. The first is a consolidation of the anti-immigration stance by older white Americans. The second is a reduction of liberal attitudes towards immigration for younger non-white Americans, especially Hispanics and people born to foreign-born parents. While we are not able to say which of those trends are dominant, we find evidence for two hypotheses that might be underlying our econometric findings. First, people receiving negative shocks when they enter the labor market (impressionable years) might change their political preferences more permanently. Second, new generations of immigrant descendants (mostly Latin) might be converging with those born from native parents in terms of their political preferences.

This work is closely related with the literature exploring the political impact of immigration and refugee inflows. Evidence of an increase in voting shares for anti-immigration platforms are found for example by Otto and Steinhardt (2014), Barone et al. (2016), Halla et al. (2017), Viskanic (2017), Baerg et al. (2018), Vertier and Viskanic (2018), Dustmann et al. (2019), Tabellini (2020), Steinmayr (2021) and Mayda et al. (2022). Alesina and Tabellini (2021) provide an extensive review of this literature and distinguish between two major sets of possible motivations to explain the political effects of immigration: Economics and Culture. By "Culture", the authors summarise all non-economic factors such as fear of diversity and linguistic distance[5]. They review the evidence-based literature on the issue and find that those non-economic factors seem to be more relevant to explaining the political effects of immigration, although economic factors might play a role in specific contexts and circumstances[6].

Our contribution to the literature is threefold. First, since studies frequently em-

[5]Politicians can strategically behave fueling those drivers as a platform to increase their chances to win, as in Rozo and Vargas (2021). A similar mechanism is discussed by Campante et al. (2021) in the context of the Ebola outbreak, in which Republican politicians in the U.S. associated the virus with an anti-immigration platform.

[6]This literature is mostly focused on high-income countries. The only study we found so far for a developing country is Rozo and Vargas (2021) for Colombia. In that case, the political effect of immigration might be partly explained by economic mechanisms due to the high degree of labor market informality and the sudden flows of refugees.

ploy SSIV strategies to identify effects, they are likely generating conflated estimates of shorter and longer-run dynamics (Jaeger et al., 2019). By employing the dynamic shift-share approach, we get more reliable estimates of the short-run effects of immigration on voting behavior and also on how it adjusts to earlier waves of immigration.

The second contribution is to connect findings of the literature on the effects of immigration on the labor market (Card, 1990; Friedberg & Hunt, 1995; Card, 2001; Borjas, 2003; Ottaviano & Peri, 2012; Lewis & Peri, 2015; Dustmann et al., 2016; Borjas, 2017; Peri & Yasenov, 2019) and on the provision of public goods (Speciale, 2012; Dustmann & Frattini, 2014; Preston, 2014; Blau & Mackie, 2017; Mayda et al., 2020) with findings related to voting and ideology. We do that by showing that the native economic conditions' response to immigration shocks can be relevant in explaining political shifts.

The third contribution is to shed light on the literature exploring the drivers of attitudes towards immigration, mostly survey-based. While some studies find a strong economic motivation for an anti-immigrant sentiment (Scheve & Slaughter, 2001; Dustmann & Preston, 2006; Mayda, 2006; Facchini & Mayda, 2009), some others find that non-economic factors are more relevant (Citrin et al., 1997; Chandler & Tsai, 2001; Hainmueller & Hiscox, 2010; Hainmueller & Hopkins, 2014; Hainmueller et al., 2015). Instead of relying on self-declared information, our study applies an econometric approach to observational data on real-world economic outcomes to measure whether economic changes influence an anti-immigration vote. This might be relevant for its policy implications.

After this introduction, the paper is divided as follows: we start by presenting a conceptual framework in Section 2. Section 3 presents the background context of our setting and the data used for the empirical application. Section 4 presents both the empirical strategy to identify the dynamic effect of immigration on voting and its main findings. In Section 5, we apply the same framework to discuss whether aspects related to the labor market and the provision of public goods might explain this effect. We also include the causal mediation analysis exercise and survey evidence to shed more light on the possible mechanisms. In section 6, we discuss those findings in light of the related literature. Section 7 concludes.

2 Conceptual framework

This section is intended to provide a conceptual background on the dynamic relation between immigration, electoral behavior, and economic conditions. Although we do not build a formal model, we rely on previous literature to derive theoretical predictions regarding the phenomena we study.

We start by acknowledging that people consider several factors when voting, including previous party identification and preferences over a wide range of issues such as economic policies, individual rights, climate change, and so on (Ansolabehere et al., 2008). They also take into account economic conditions, both in general terms - namely, the economic performance of the country[7] - or their own, such as their employment condition, their access to public goods and services, among others (Markus, 1988; Healy et al., 2017). Besides that, voting patterns depend on voters' individual characteristics such as age, educational level, position in the income distribution, ethnicity, or even religion (Dahlberg, 2013).

Among those factors, we highlight that the presence of immigrants in the country might drive preferences for immigration policies and thus influence voting behavior. In countries with larger shares of immigrants in the population, such as the United States, the debate on how restrictive or liberal should be the immigration policy is one of the main topics on the political agenda. As a result, attitudes and perceptions towards immigrants work as drivers of vote preferences (Hawley, 2013).

It's not rare to invoke economic concerns with immigration as a motivation for changing vote behavior, especially those related to the labor market and the provision of public goods[8]. Consider for example that voters might be concerned that more immigrants would induce negative effects in the labor market or increase competition for

[7] Voters can decide based on the economic performance of the country, but not necessarily on factors that are under government control. For more details, see Campello and Zucco Jr (2016).

[8] Those concerns can be related to personal circumstances but there is evidence (Citrin et al., 1997; Hainmueller & Hiscox, 2010; Hainmueller & Hopkins, 2014) that public attitudes towards immigration are more motivated in a *sociotropic* way - immigrants affecting the country or the local economy as a whole.

public resources, especially in health, education, or welfare spending categories[9]. If voters indeed observe these phenomena, they might respond by voting for candidates that promise to restrict immigration.

Nevertheless, the relationship between immigration and economic conditions might be different depending on the time horizon. Indeed, one of the most discussed controversies in the literature is whether immigration affects the labor market in the host country negatively or positively (Dustmann et al., 2016). Negative effects typically arise after short-run shocks and affect particular groups of natives. They are rationalized with partial equilibrium models in which labor demand is fixed and employment conditions adjust to sudden increases in labor supply. If native and migrant workers are perfect substitutes, then unemployment may rise and wages may fall. But if they complement each other (for example, if migrants are low-skilled and natives are high-skilled), then the effects might be positive. On the other hand, immigration may also induce general equilibrium dynamics independently of the degree of skill complementarity. By increasing aggregate demand through population increases, immigration can benefit the whole economy, thus providing positive outcomes for natives that might take longer to show up (Chassamboulli & Palivos, 2014; Battisti et al., 2018; Iftikhar & Zaharieva, 2019).

This is also true for the relationship between immigration and the provision of public services (Preston, 2014). Increased competition for public resources might happen if immigrants are net beneficiaries of public spending. This is more likely to happen as soon as immigrants arrive, especially if they are poorer. However, they can also increase public revenue through taxation of their income, consumption, or even investments in the host country, in which case they can be net contributors (Dustmann & Frattini, 2014). This is more likely to happen with better-integrated immigrants. Since the public budget is not fixed as immigration increases, the resulting balance depends on immigrants' characteristics such as skill and age, and on whether the effects are observed in shorter or longer time frames.

If these economic changes triggered by immigration indeed take place, they

[9]For an analysis of public education expenditure and immigration in the EU, see Speciale (2012).

might affect voting behavior differently in the short run or the longer run. On one hand, voters can choose a candidate based on recent economic outcomes. They can punish the incumbent party if they perceive local economic conditions (or their own) to be worse, or they can reward it if they feel that society (or themselves) is (are) better-off (Healy et al., 2017). On the other hand, voters might keep the memory of a negative (or positive) shock in the short run and change voting behavior more persistently (Krosnick & Alwin, 1989; Giuliano & Spilimbergo, 2014), even if the effects of these shocks fade over time (Autor et al., 2020).

Immigration can affect voting preferences indirectly through economic outcomes, but it can also have a direct effect. Voters could attribute worse economic conditions to immigration even if that is not the case. They might end up choosing to vote for candidates that are explicitly anti-immigration if they are convinced of this rhetoric (Citrin et al., 1997). Also, immigration *per se* may directly affect native preferences for redistribution, which are typically misperceived (Alesina & Stantcheva, 2020; Alesina et al., 2022). Finally, voters might choose immigration policies based on other non-economically related motivations such as a fear of cultural diversity, negative *a priori* perceptions of immigrants, and concerns related to increasing crime, among others (Chandler & Tsai, 2001).

Our empirical question is whether (and in which direction) real changes in the economic conditions of natives induced by immigration can explain voting patterns, and how that relation behaves dynamically. In other words, by assuming that immigration affects voting, we argue that part of this effect is possibly mediated by immigration-induced economic changes that might take place in the short run but also show dynamic adjustment patterns. The unexplained part is possibly associated with the direct mechanisms mentioned above. We aim to shed light on these issues with an econometric approach that enables us to perform two tasks. First, to disentangle between shorter and longer-run dynamics. Second, to use real economic variables to disentangle among indirect effects (mediated by those possibly economic changes, which are more easily measured) and direct ones (not related to those economic changes, harder to measure).

3 Background and Data

In this section, we describe the data used in the empirical application, detailing its main sources and some processes to assemble information. We also briefly discuss the context studied and present some descriptives and correlations of our main variables.

3.1 Data

Our setting is based on a panel of U.S. commuting zones[10] for election years between 1992 and 2016[11]. To build variables related to immigrant shares, labor market outcomes, and a variety of economic and demographic controls, we used data from the U.S. Decennial Census of 1990 and 2000, together with the American Community Surveys (ACS) for 2008, 2012 and 2016[12]. We collected this data from the Integrated Public Use Micro Samples (IPUMS), using the 5% samples in the case of the censuses (Ruggles et al., 2020). We also used the 1980 census to build immigrant shares for the instrument and to collect baseline variables for heterogeneity analyses. All monetary values were adjusted to price variation with the Dec/1999 Consumer Price Index (CPI) deflator.

To build the trade exposure instrument[13] used as a control, we used David Dorn's crosswalk files from Autor and Dorn (2013) and Autor et al. (2013, 2018), together with UN Comtrade Data collected through its API. Differently from the original application of this instrument, we built it in levels instead of differences, so that it matches the structure of the panel. The crosswalk files are also used to aggregate Public Use Microdata Areas (PUMA) from IPUMS to commuting zones.

[10] Although earlier studies use a county-level panel (Mayda et al., 2022), we use commuting zones to have a more adequate geographical boundary for a labor market area, since we are also using economic variables as outcomes for immigration.

[11] In most of our analysis, however, we will consider the subperiod of 2000-2016 because we use the 1992 and 1996 observations of lagged immigrant shares as regressors. We replicate results in the two samples showing that removing the first two election years does not qualitatively change our results.

[12] We matched 1992 election data with 1990 census variables and we linearly interpolated data to obtain information for 1996 and 2004.

[13] We use it to take account of the possible impact of import competition from China in the U.S. elections (Autor et al., 2020; Che et al., 2022).

We obtained public spending information through the US Census Bureau (2020) Financial Statistics of States and Local Governments. The data is available in a cumulative file from 1967 to 2012. For the 2017 data, a separate file contains disaggregated information about all public revenue, spending, and debt categories. We aggregated it to keep comparability with the categories available in earlier years. The data is complete for all local units only for years ending in 2 or 7, so we interpolated to obtain data matching election years. We used the sum of local governments, excluding state and federal government finances, i.e., we considered counties, cities, townships, special districts, and independent school districts[14], all aggregated to the commuting zone level.

We collected data for Presidential election results from two sources: the U.S. Political Stats from the Congressional Quarterly Press Library and the MIT Election Data and Science Lab (MEDSL, 2018). We obtained data for each presidential election between 1992 and 2016. We dropped observations from Alaska and Hawaii due to the absence of data in some years, ending up with a balanced panel of 722 commuting zones in seven years (5,054 observations).

3.2 Context and correlations

We study the relation between immigration and electoral behavior in the United States setting. Although this is a well-known setting, it is worth highlighting some of the main characteristics of the U.S. political system. Presidents are heads of State and government in the U.S., managing the executive branch of the federal government. This means that many policy guidelines - including migration policies and border security - may be modified or at least subject to a proposal of change by the president's office. Presidential elections take place every four years. The president is elected indirectly through an Electoral College, composed of 538 state electors that tend to follow the popular vote, which is not compulsory. In this system, the vote majority does not mean winning the election, but the absolute majority of electors, although they tend to be correlated. Typically, presidential elections are a race between a Democrat or a Republican candidate previously

[14]We followed earlier studies using this data, such as Feler and Senses (2017).

selected through a system of primaries.

These characteristics make our setting suitable for identifying preferences for immigration policies with a specific party. Our main outcome of interest is the Republican vote share in presidential elections, used as a proxy for being less liberal towards immigration. Although political preferences regarding immigration policy might be driven by a range of economic and geographic factors (Facchini & Steinhardt, 2011), and may even differ inside the same party, there is evidence that Republican politicians in the U.S. tend to be more restrictive towards immigration (Conconi et al., 2020). Besides that, survey evidence suggests that Republican-leaning voters in the U.S. tend to have more negative views towards immigrants and to be more concerned that more immigrants are worse for the economy (Pew Research Center, 2015). We come back to this point when discussing the mechanisms with survey data in Section 5.5.

The average Republican vote share in U.S. commuting zones increased from 38% to 47% between 1992 and 2016[15]. We also observe an increase in the average share of immigrants in U.S. commuting zones of 74% in the same period. This phenomenon is particularly strong for high-skilled immigrants (those with a high school degree or more), whose presence doubled in the period, while low-skilled immigrants increased by 34% until 2008, and then decreased 8% until 2016.

The composition of low-skilled immigrants by country of origin changed considerably in our study period. The share of Mexicans increased from 43.7% in 1992 to 53.7% in 2016. Immigrants from other countries in Central America and the Caribbean (including Cuba and the West Indies) increased from 16.8% to 21.6%. On the other hand, the presence of Europeans significantly decreased from 15.4% to 3.9%. This fact is relevant for our empirical strategy, as we discuss later in Section 4.1.

There were also significant changes in living conditions for particular groups of natives in this period. In terms of labor market outcomes, we do not observe any specific trends in overall wages, but a slight decrease for low-skilled natives. We also observe a

[15]In this paragraph, all averages mentioned are weighted by the voting-age population in each commuting zone at baseline.

significant increase in unemployment in the same group in 2012. Local revenue and local expenditure per capita increased over time, but welfare spending decreased, especially in 2016. In Appendix Table A1 we report more detailed descriptive statistics on the main variables used in this paper.

During this period, we observe considerable variation in low-skilled immigration and voting for the Republican Party across regions and over time. In Appendix Figure A5, after accounting for differences in the voting-age population in each locality, we split our period of analysis and make two scatterplots. We plot the 2004-2016 change in the Republican vote share on the contemporaneous 2004-2016 change in low-skilled immigration, and then on the lagged 1992-2004 change in low-skilled immigration. In this way, we capture contemporaneous and lagged correlations respectively. We find a positive correlation in the short run, but a negative one in the longer run. In other words, commuting zones that experienced a recent rise in low-skilled immigration also increased voting for Republicans on average. This correlation seems to be stronger in many Mid-West and South-West localities, as shown in Appendix Figures A1 and A2. However, previous changes in immigration are associated with a lower Republican vote share.

On the other hand, we also observe correlations between this type of migration and variables reflecting labor market conditions or the provision of public goods related to particular groups of natives. The 2004-2016 change in the low-skilled native unemployment rate is positively correlated with the contemporaneous change in low-skilled immigration, as shown by Appendix Figure A6. Nevertheless, there is no visual correlation in the longer run. Visual inspection suggests that regions with the highest positive variation in immigration are not necessarily experiencing an overall increase in unemployment. Some of them are experiencing decreases, as shown in Appendix Figure A3. As for the local expenditure on welfare per capita (shown in Appendix Figure A7), we observe a slightly positive correlation in the short run and a negative one in the longer run. Also, if anything, regions experiencing increases in low-skilled immigration seem to show decreases in expenditure per capita, as observed in Appendix figure A4. All those patterns suggest that we need further investigation on whether there is a causal chain

connecting low-skilled immigration, economic conditions of natives, and voting patterns in shorter and longer periods.

In a nutshell, we explore a setting in which a considerable share of the population switched to a right-wing vote in 25 years. In many regions where we observed this pattern, there was a contemporaneous increase in the share of immigrants without a high school degree. On the other hand, there was also significant variation in economic variables measuring labor market conditions and the local state capacity to provide public goods. Although there are seemingly weak correlations between immigration and those economic variables, we argue that they might mask considerable heterogeneities concerning particular groups of natives and differences between short-run effects and longer-run dynamic adjustments. Those differences might be relevant to understanding the dynamics of voting patterns.

4 Estimating the effect of immigration on voting

We start our analysis by estimating the effect of low-skilled immigration on voting, both in the short run and the dynamic adjustment component. In subsection 4.1 we discuss the empirical strategy for causal identification of those effects and, in subsection 4.2, we present the results of this estimation procedure. We show some robustness checks in subsection 4.3.

4.1 Empirical method

Consider the problem of estimating the effect of immigration on the Republican vote share in a panel of U.S. commuting zones. We start by using an Ordinary Least Squares (OLS) regression based on equation 1 below, indexed by commuting zone i and election year t.

$$r_{it} = s_i + q_t + \beta_l l_{it} + \beta_x X_{it} + \epsilon_{it} \tag{1}$$

The main regressor is l_{it}, the share of low-skilled immigrants over the adult population (aged 18 or more), a measure of updated immigrant stock. More specifically, those

are foreign-born people who did not complete high school, including both naturalized citizens and non-citizens[16]. The primary outcome is the Republican vote share in a presidential race (r_{it}). We also include commuting zone (s_i) and election year (q_t) fixed effects to control respectively for time-invariant local characteristics and year-specific aggregated shocks. We eventually include a set of time-variant economic and demographic covariates X_{it}, which are included as an interaction of baseline characteristics with time dummies to avoid the effect of correlation with intermediate outcomes. We cluster standard errors at the commuting zone level to deal with potential correlation within our geographic units over time. Finally, we weight the regression by the voting-age (adult citizen) population so that coefficients can be interpreted as the effect on the average voter.

Those OLS estimates may naturally suffer from endogeneity issues. For example, immigrants may be endogenously attracted to locations where natives' attitudes are more welcoming, and this is correlated with voting for a political party that is less restrictive towards immigration. A standard method to deal with this kind of endogeneity is the shift-share instrumental variable (SSIV) design, which is often employed in migration studies (Altonji & Card, 1991; Bartik, 1991; Card, 2001; Lewis & Peri, 2015). It consists in creating an instrument by estimating predicted shares of immigrants from two interacted components: the national-level changes in the immigrant population for each country of origin over time; and the previous distribution of immigrants from each country of origin in each location. Immigrant shares predicted with this technique should only affect any local economic or political outcome through observed shares of immigrants.

The instrument combines two sources of exogenous variation. The first one comes from country-of-origin-level factors affecting global migration trends, which are plausibly uncorrelated with regional characteristics. The second one is the degree of regional exposure of those aggregated shocks, determined by the stock of immigrants before our period of study. In a thought experiment, we can think of this instrument as a counterfactual scenario in which additional immigrants leaving their countries of ori-

[16]One could argue that part of this effect comes from increasing the pool of voters with the naturalized immigrants that would naturally vote against the anti-immigration platform. But Mayda et al. (2022) show evidence that the effect comes from existing voters shifting and not from a composition effect due to newcomers.

gin for different reasons would choose their location inside the host country based only on past settlements from the same origin. As with any other instrument, the estimated coefficient will have a Local Average Treatment Effect (LATE) interpretation, in which the population of compliers is composed of regions in which this counterfactual scenario corresponds more closely to the reality, as long as the first stage is strong enough.

In practice, since we focus on the low-skilled immigrant population, we consider an approach similar to Mayda et al. (2022) by using a skill-specific SSIV. The only difference in our case is that we focus on the low-skilled population both in the share and in the shift components of the instrument, while they use overall shares interacted with skill-specific shifts. This approach considers possible differences in the past settlement motivation for migrating that might exist between skill groups and may contribute to the instrument's relevance. We start by calculating the predicted low-skilled immigrant population, given by equation 2.

$$\hat{L}_{it} = \sum_{c} sh^{L}_{c,i,80} L_{ct} \tag{2}$$

Here, c indexes each country of origin[17], $sh^{L}_{c,i,80}$ is the share of low-skilled immigrants from country c at commuting zone i in 1980, and L_{ct} is the aggregate population of low-skilled immigrants from country c at time t varying among election years between 1992 and 2016. We perform a similar process with the high-skilled immigrants and with the native population, as shown in equation 3.

$$\hat{H}_{it} = \sum_{c} sh^{H}_{c,i,80} H_t \quad and \quad \hat{N}_{it} = sh_{US,i,80} N_t \tag{3}$$

Finally, we aggregate native and immigrant predicted populations to obtain the total predicted population and use it to obtain instruments in terms of population shares,

[17]We use 26 country groups instead of countries to avoid a possibly high frequency of zeros for given country of origin-commuting zone pairs. We constructed groups based on the U.S. Census birth place variable, keeping countries with large flows separated. We plot the relative frequency of each country group among the low-skilled immigrant population of 1980 in Figure A8 in the appendix.

as shown in equations 4 and 5, where the superscript $(.)^{SS}$ denotes "shift-share".

$$\hat{Pop}_{it} = \hat{L}_{it} + \hat{H}_{it} + \hat{N}_{it} \tag{4}$$

$$l_{it}^{SS} = \hat{L}_{it} / \hat{Pop}_{it} \;\; and \;\; h_{it}^{SS} = \hat{H}_{it} / \hat{Pop}_{it} \tag{5}$$

The fact that this kind of instrument combines two possible sources of exogenous variation created some concern in recent studies. On one hand, Goldsmith-Pinkham et al. (2020) stresses the condition of exogenous shares as sufficient for identification[18]. On the other hand, Borusyak et al. (2022) highlights that even if shares are endogenous, shock exogeneity can be used for identification as long as there are sufficiently independent as-good-as-random shocks[19]. These two papers agree that applied researchers should stick to one research design and defend the validity of the instrument with specific diagnostic tests and robustness checks. We will come back to this issue in subsection 4.3.

Regarding specifically the literature on immigration, Jaeger et al. (2019) argue that shorter and longer-run effects of immigration might be conflated when using SSIVs. This is particularly important since scholars are typically interested in studying the effects of immigration on employment. Although the labor market can be directly affected in the short run, immigration might also trigger general equilibrium adjustment dynamics that take time to show up. The authors use a stylized labor market model to show that when we use standard shift-share instruments without any adjustment, the estimated effects might be more positive than the real ones since part of the effect captures adjustments from previous immigration flows.

To better understand the dynamic effects of immigration on voting, and how that relates to local economic dynamics, our main identification strategy will rely on Jaeger et al. (2019)'s procedure to disentangle the short-run effect from the dynamic adjustment component. We will run two-stage least-squares (2SLS) regressions in which we add

[18] Although not explicitly, Mayda et al. (2022) seem to rely on this condition for identification. They show that the effects are robust to the exclusion of any country of origin group from the instrument, especially the most relevant ones according to the Rotemberg weights criterion.

[19] In this case, we should be more concerned with correlation among residuals in regions with similar exposure weights. Adão et al. (2019) propose inference procedures that take this structure into account.

lagged low-skilled immigrant shares, instrumented by the lagged instrument. The coefficient of contemporaneous low-skilled immigration will capture short-term effects while the coefficient on the lag will capture adjustment dynamics. As the authors show for the U.S., this adjustment might take place within a decade. In that sense, we will focus on specifications with a two-period lag (which corresponds to a period of 8 years in our setting)[20]. Importantly, the method is more applicable in contexts where the composition of immigrants changes substantially through the period of study, which is the case of low-skilled immigrants in the U.S. between 1992 and 2016, as shown in Section 3.2.

Formally, we estimate two first stages given by equations 6 and 7, and a second stage is given by equation 8. We will also estimate the standard shift-share approach without the lags as a benchmark. In our preferred specifications throughout the paper, we set $k = 2$, meaning we use the current and the two-period lagged share of immigrants, both instrumented by the current and the lagged instrument. Both the predicted current share and the predicted lagged share are regressors in the second stage. Our coefficients of interest are β_1, the short-run effect of low-skilled immigration on voting, and β_2, which will capture the dynamic adjustment effect within 8 years.

$$l_{it} = s_i + q_t + \gamma_1 l_{it}^{SS} + \gamma_2 l_{it-k}^{SS} + \gamma_x X_{it} + \eta_{it} \tag{6}$$

$$l_{it-k} = s_i + q_t + \delta_1 l_{it}^{SS} + \delta_2 l_{it-k}^{SS} + \delta_x X_{it} + \varepsilon_{it} \tag{7}$$

$$r_{it} = s_i + q_t + \beta_1 \hat{l}_{it} + \beta_2 \hat{l}_{it-k} + \beta_x X_{it} + \epsilon_{it} \tag{8}$$

4.2 Findings

We start by documenting a positive association between low-skilled immigration and voting for the Republican Party in U.S. commuting zones, by showing OLS results in Table 1. The estimate is robust across many specifications. A one percent increase in

[20]We report tables using a one-period lag (4 years) in the appendix. Although many results are similar, we do not expect that the dynamic adjustment will take place in this window. Using more than one lag simultaneously results in a considerable loss of precision.

the share of low-skilled immigrants in a commuting zone is associated with an increase in Republican voting by around 0.9 and 1.2 percentage points.

From columns 1 to 4, we report estimates considering the full sample (from 1992 to 2016). Column 1 shows the results for the benchmark specification using only election year and commuting zone fixed effects. In column 2, we weight the regression for the baseline voting-age population, so that estimates take into account different population sizes and better reflect the average voter.

We include a set of time-varying controls[21] in column 3, to take account of other possible explanations for voting for Republicans. Most of them are used to estimate electoral effects in other settings (e.g. DellaVigna and Kaplan (2007)). We use the share of male population, share of married, share of African-Americans, share of urban population, labor market participation rate, and average per capita household income. We also include the share of low-skilled natives to make sure that the effect is coming from immigrants and not just from the skill level. Taking into account the relevance of controlling for other shift-share components (Borusyak et al., 2022), we also include as controls a Bartik employment instrument (Bartik, 1991) to capture sector-specific shocks on the labor market and an import exposure instrument based on Autor et al. (2013) to capture possible effects of a "China shock" on voting[22]. In Appendix Table A2, we show the estimates for each covariate in column 3 of Table 1. Effects are robust to leaving each control variable out, as shown in Appendix Figure A9 considering specification 3[23].

Many of those demographic and economic controls might be intermediary outcomes, in the sense that they can also be affected by immigration, and thus confound results. To avoid the so-called "bad control" issue, our preferred specification (column 4 onward) substitutes covariates' yearly values with their baseline (1992) values interacted with time dummies. Compared to column 3, we get a similar coefficient, but slightly more precisely estimated.

[21] We use the same covariates as Mayda et al. (2022) to generate comparable results, except for the unemployment rate, since it is an intermediary outcome of interest in this paper.
[22] Which is shown by Autor et al. (2020) and Che et al. (2022) to be relevant.
[23] The range of coefficients go from 1.01 to 1.18, with very similar confidence intervals across specifications, except the one with no controls, in which we lose precision, but the magnitude is still very similar.

Table 1: OLS results - Effects of low-skilled immigration on voting

	(1)	(2)	(3)	(4)	(5)	(6)
	Republican vote share (Presidential elections)					
Share of low-skilled immigrants at t	0.852***	1.135***	1.121***	1.060***	1.222***	1.239***
	(0.177)	(0.431)	(0.265)	(0.186)	(0.210)	(0.211)
Share of low-skilled immigrants at $t-2$						-0.242
						(0.190)
Election Year FE	X	X	X	X	X	X
Commuting Zone FE	X	X	X	X	X	X
Weighted by voting pop		X	X	X	X	X
Controls (yearly)			X			
Controls (baseline)				X	X	X
Observations	5054	5054	5054	5054	3610	3610
R-squared	0.871	0.870	0.914	0.952	0.968	0.968

The table reports OLS regressions of the low-skilled immigration and its lags (previous election years) on the share of Republican votes in presidential elections. In column (3), we included a set of controls with their yearly values for each commuting zone, while from column (4) onward we used their baseline (1992) values interacted flexibly with time dummies. We restricted the panel to 2000 to 2016 from column (5) onward in order to have comparable samples with specifications containing lags. Clustered standard errors at the commuting zone level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

In column 5, we remove the two first election years (1992 and 1996) from the data in order to have a comparable sample with column 6, in which we include lagged low-skilled immigrant shares. Removing the first two periods slightly increases the point estimate. The inclusion of the two-period lag has virtually no effect on the contemporaneous coefficient. The specification in column 6 suggests that immigration that took place 8 years before our measurement of voting shares has an attenuation effect on the right-wing shift, but this effect is not significant[24].

Nevertheless, as explained in Section 4.1, we know that those estimates may be biased due to endogeneity. To address that, we use a dynamic shift-share approach, in

[24]The coefficient is positive if we use the one-period lag, but it is still close to zero and not significant (see Appendix Table A3).

which the share of low-skilled immigrants is instrumented by a past settlement SSIV and its lagged version. In order to check for the instrument strength, we report first stage results in Table 2. Columns 1 and 2 show the result of the traditional shift-share approach, in which both immigration and instrument are measured at time t. In column 1, we report results for the full sample, while in column 2, we report the same without the first two years (1992 and 1996). We include the lagged instrument at $t-2$ in column 3. Finally, we report the first stage results using the lag of low-skilled immigrant shares as the independent variable in column 4[25].

Table 2: First Stage of the Dynamic shift-share for low-skilled immigration

	(1)	(2)	(3)	(4)
	Share of low-skilled immigrants at:			
	t			$t-2$
Shift-share instrument at t	0.393**	1.176***	1.041***	1.142***
	(0.161)	(0.099)	(0.153)	(0.106)
Shift-share instrument at $t-2$			-0.367***	0.380***
			(0.091)	(0.122)
First Stage F-stat	5.94	140.76	66.72	77.91
R-squared	0.985	0.992	0.993	0.991
Observations	5054	3610	3610	3610

The table reports dynamic shift-share first stage regressions of the low-skilled immigration and its lag (2 election election years before) on the shift-share instrument for low-skilled immigration (and its lag). We restricted the panel to 2000 to 2016 from column (2) onward in order to have comparable samples with specifications including lags. All regression specifications have baseline controls interacted with time dummies, commuting zone and election year fixed effects and are weighted by the voting-age population. Clustered standard errors at the commuting zone level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Except for column 1, in which we use the standard shift-share approach in the full sample, our first stage results are reassuring in terms of the instrument strength, as

[25] Although it might not be very straight forward to interpret coefficients of future immigration on past immigration, those regressions are necessary to estimate the dynamic shift-share approach (Jaeger et al., 2019). The high degree of serial correlation might explain why the coefficient in the first row of column 4 is positive and significant, for example.

stated by the first stage F-statistic and the R-squared. The instrument is always positively correlated with the immigrant shares, and more so in the restricted sample (2000-2016). The inclusion of the lagged instrument does not significantly change results for the contemporaneous estimates but makes the first stage a bit weaker compared to the standard shift-share in column 2. Those regressions also show that past values of the instrument seem to negatively affect current values of immigrant shares. Results are very similar if we use instead the $t-1$ lag, reported in Appendix Table A3.

Dynamic 2SLS estimates reported in Table 3 corroborate the positive association between low-skilled immigration and Republican voting in the short run. All specifications have fixed effects, baseline controls interacted with time dummies and population weights. We again start reporting standard shift-share coefficients (without lags) in columns 1 (1992-2016) and 2 (2000-2016). We find that a 1% increase in the share of low-skilled immigrants significantly increases the Republican vote share in presidential elections by 1.2 percentage points in the full sample. Reflecting the stronger first stage, the estimate in the restricted sample (2.9 percentage points) is more than twice higher compared to the respective OLS estimate, and more precisely estimated. This is evidence that some endogeneity was present, meaning that low-skilled immigrants might have self-selected into areas whose population was more favorable to flexible migration policies, proxied by a democrat vote.

In column 3, we include two-period lagged low-skilled immigrant shares. Since SSIV estimates might conflate short-run and dynamic adjustment effects, the contemporaneous coefficient in those regressions is more likely to capture the short-run impact of low-skilled immigration on voting. When we include the two-period lag (8 years), the result suggests that the actual short-run estimate is higher, with a dynamic attenuation conflating the standard Bartik estimate, although this component is not statistically significant. If we instead use the one-period lag, the magnitude of this component is slightly lower, and still not significant (results reported in Appendix Table A3).

Table 3: 2SLS results - Effects of low-skilled immigration on voting

	(1)	(2)	(3)
	Rep. vote share (Pres. elections)		
Share of low-skilled immigrants at t	1.159*	2.861***	3.224***
	(0.641)	(0.476)	(0.669)
Share of low-skilled immigrants at $t-2$			-0.426
			(0.588)
Observations	5054	3610	3610
Anderson-Rubin test P-value	0.071	0.000	0.000
First Stage F-stat at t	5.94	140.76	66.72
First Stage F-stat at t-2			77.91

The table reports dynamic shift-share 2SLS estimates of the Republican vote share in Presidential elections on the low-skilled immigration and its lag (two election years before) instrumented by the predicted low-skilled immigration (and its respective lag). We restricted the panel to 2000 to 2016 from column (2) onward in order to have comparable samples with specifications including lags. All regression specifications have baseline controls interacted with time dummies, commuting zone and election year fixed effects and are weighted by the voting-age population. Clustered standard errors at the commuting zone level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

In a nutshell, we concluded that the right-wing shift derived from low-skilled immigration in the U.S. is a short-run phenomenon by applying the procedure suggested by Jaeger et al. (2019). Controlling for lagged immigration shares does not significantly change the results of using the standard SSIV approach. We interpret this as evidence that the effect is persistent and not attenuated by general equilibrium dynamics over time.

4.3 Robustness

This section presents some robustness checks for the identification strategy. We start by checking how robust our results are to the weak instrument problem. To do that, we report the P-value of the Anderson-Rubin test for inference under weak instruments in Table 3. This statistic is preferred to the usual T-Ratio inference in the just-identified case (Moreira, 2009). For all specifications except column 1, the 95% confidence intervals

do not cover zero, which is reassuring for our results. For the first specification, the effect is significant at 10% under both T-ratio or Anderson-Rubin inference procedures.

One concern when using SSIVs is that the identification strategy might rely on the exogeneity of the initial shares[26]. Based on an equivalence between the 2SLS estimate using a SSIV and a GMM estimate using each share as an instrument, Goldsmith-Pinkham et al. (2020) show that the SSIV coefficient is equivalent to a weighted[27] average of coefficients associated with each share. To check whether the results are dependent on the shares of a specific country of origin, we followed Mayda et al. (2022) and ran the standard SSIV 2SLS regression, but leaving one country out of the instrument at a time. The result is shown in Appendix Figure A10 and is quite reassuring: the point estimate is basically the same no matter what country we remove from the instrument. The only remarkable difference shows up when we remove Mexico. We lose precision, and the coefficient becomes non-significant. This is because Mexico accounts for the highest number of low-skilled immigrants in this period, as shown in Appendix Figure A8.

We also check whether our results are consistent with earlier studies analyzing the same setting. To do that, we show OLS (Panel A) and 2SLS (Panel B) estimates with the standard SSIVs (without lags) for low-skilled and high-skilled immigrant shares together in Appendix Table A4. Results in column 1 of Panel A are directly comparable with the work of Mayda et al. (2022)[28]. We find qualitatively similar results, with small quantitative differences. Restricting the sample to 2000 onwards (columns 3 and 4) or using controls at their baseline values interacted with time dummies (columns 2 and 4) essentially generates the same results for low-skilled immigration. Adding the high-skilled component to the regression generates the same result as our baseline specification. Finally, 2SLS results are in the same direction, except that high-skilled immigration coefficients become non-significant in the restricted sample.

[26] Even if the shares are endogenous, we could rely on the shock exogeneity and adjust the inference procedures (Adão et al., 2019; Borusyak et al., 2022). We are currently working on a battery of robustness checks based on this possibility.

[27] The so-called Rotemberg weights can be interpreted as a sensitivity-to-misspecification parameter measuring how relevant each country of origin is for the identifying variation.

[28] They show estimates separately by the type of election only for OLS. The 2SLS estimates pool together presidential, House, and Senate elections.

The similarity with Mayda et al. (2022) is reassuring given that our strategy has some remarkable differences. Our unit of analysis is the commuting zone (not the county); we build the instrument using skill-specific immigrant shares (not total immigrant shares); and we focused on presidential elections (not pooled House, Senate, and presidential elections). The authors analyze the possible mechanisms behind this effect and perform other robustness checks. Given the resemblance with our results, we will not advance further in this direction as they did. Our goal is to evaluate whether possible immigration-induced changes in the labor market conditions and in the provision of public goods for specific groups can explain electoral results, and how economic differences between short-run phenomena and dynamic adjustment components can be relevant.

5 Decomposing the effect of immigration on voting

This section aims to understand the possible underlying economic mechanisms behind the effect of low-skilled immigration on voting. We start by applying the same dynamic SSIV approach with different sets of outcome variables: labor market and local public finance outcomes. We then employ a single IV causal mediation framework to measure the contribution of those channels to explain short-run and dynamic adjustment effects on voting patterns.

5.1 Effects of immigration on labor market outcomes

We want to check whether possible changes in the labor market conditions of natives associated with increasing shares of low-skilled immigrants might coexist with the effects we find on voting patterns. To do that, we consider similar 2SLS regressions with the dynamic SSIV approach to capture the short and longer-run effects of low-skilled immigration on the native unemployment rate and the log of native hourly wages[29]. To take account of possible differences for particular groups of natives, we will also consider

[29] From 2008 onwards, the ACS does not provide the number of weeks worked, only a variable containing pre-specified ranges. We estimated the number of weeks from the 2007 ACS in each range and then used this average number of weeks to obtain a weekly wage. We then divided by the usual hours worked per week.

the same variables restricted for different skill groups (low- and high-skilled).

We do not observe significant effects on the overall unemployment rate among natives, as shown in columns 1 and 2 of Panel A in Table 4. However, specific groups were differentially affected. Indeed, results in column 4 of Panel A suggest that unemployment among low-skilled natives increased in the short run by between 1.25 percentage points. Besides that, some degree of adjustment took place: more than two-thirds of the short-run increase in unemployment fades within 8 years. We also find a small decrease in unemployment of high-skilled natives (column 5), but the results of our dynamic specification (column 6) are not significant.

Table 4: 2SLS results - Effects of low-skilled immigration on labor market outcomes

	All skills		Low-skilled		High-skilled	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Native Unemployment Rate						
Share of low-skilled immigrants at t	-0.144	0.038	0.501	1.252***	-0.235**	-0.101
	(0.134)	(0.103)	(0.477)	(0.340)	(0.109)	(0.083)
Share of low-skilled immigrants at $t-2$		-0.213		-0.881*		-0.156
		(0.136)		(0.524)		(0.104)
Panel B: Native Log Average Hourly Wages						
Share of low-skilled immigrants at t	3.367***	1.912***	3.696	2.042	3.407***	1.975***
	(1.215)	(0.656)	(3.156)	(2.508)	(1.176)	(0.652)
Share of low-skilled immigrants at $t-2$		1.705		1.938		1.679
		(1.345)		(2.945)		(1.291)
Observations	3610	3610	3610	3610	3610	3610

The table reports dynamic shift-share 2SLS estimates of labor market outcomes on the low-skilled immigration and its lag (two election years before) instrumented by the predicted low-skilled immigration (and its respective lag). We restricted the panel to 2000 to 2016 in order to have comparable samples with specifications including lags. All regression specifications have baseline controls interacted with time dummies, commuting zone and election year fixed effects and are weighted by the voting-age population. Clustered standard errors at the commuting zone level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

For native hourly wages, we observe a short-run overall increase derived from low-skilled immigration (columns 1 and 2 of Panel B in Table 4). Results on wages of low-skilled natives (columns 3 and 4) are very noisy and not significant. We infer that increases in overall wages are coming from high-skilled natives. They increase mostly in the short run, as shown in column 6, but the coefficient on the dynamic adjustment component is not significant.

In summary, after we control for a dynamic adjustment component, we find evidence of rising unemployment among low-skilled natives in the short run due to increased low-skilled immigration. This happens *pari passu* with the immigration-induced shift towards Republicans. However, part of these negative unemployment effects fade over time, while voting changes are more persistent. On the other hand, high-skilled natives seem to benefit from the immigration of low-skilled workers in the short run, since wages increase for this group, thus suggesting a skill complementarity channel[30].

5.2 Effects of immigration on local public finance outcomes

We now move to the provision of public services as a potential channel to explain the effect of low-skilled immigration on voting. On the revenue side, immigrants can contribute by paying taxes on labor and private consumption. On the spending side, they can increase public expenditure if they consume public goods and services. If voters perceive immigrants as competitors for public resources potentially changing the distribution between natives and immigrants, they might be prone to vote for a platform supporting restrictive immigration policy.

Besides that, some revenue or expenditure categories might respond differently to immigration. For example, immigration may affect the balance between tax revenue and transfers from other entities (the Federal or the State government), depending on

[30]Results using the $t-1$ lag of the immigrant share are reported in Panel A of Appendix Table A5. In general, they are not very clear and less precise, which is indicative that dynamic effects need more time to show up. We find the same increase in unemployment among low-skilled natives in the short run, but the dynamic component, although also negative, is not significant. We do not find any significant effect on wages.

the degree of flexibility of local public finances. On the expenditure side, immigrants - particularly low-skilled ones - might demand some public services more than others, like education or welfare benefits[31], which are provided locally.

To test whether changes in the presence of low-skilled immigrants affected the local distribution of public resources, we run the same dynamic 2SLS regression using per capita local public finance variables. By using per capita variables, we can assure we are not capturing just mechanical movements due to direct population increases from immigration. We use both total revenue and expenditure as outcomes and some specific categories (taxes, transfers, education, and welfare spending).

Overall, we do not observe consistent and sizable effects on public finances when we look at total revenue and expenditures in columns 1-2 and 7-8 of Table 5. We observe significant dynamic adjustment effects for total revenue per capita but not significantly for taxes or transfers. If anything, there is a decrease in transfer revenue in the short run. Regarding expenditures, we do not observe any significant relationship between total expenditure or education spending with low-skilled immigration.

We want to highlight the public welfare spending in columns 11-12 of Table 5. In the specification where we include the lagged immigrant share, we observe that welfare expenditure significantly increases in the short run by US$ 1.86 per capita. The effect is virtually compensated by a dynamic adjustment effect in 8 years (US$ -1.78). This is in line with earlier findings on unemployment among low-skilled natives. A possible story rationalizing these findings is that this specific group might have lost jobs and temporarily increased demand for welfare benefits. Also, immigrants themselves might have increased demand for welfare in the short run, increasing perceived competition for public resources. As time goes by, these effects fade, possibly due to general equilibrium adjustment dynamics, which explains why the standard SSIV estimate is virtually zero[32].

[31] This category includes locally-administered TANF (Temporary Assistance for Needy Families), other cash assistance programs (including refugee assistance, poverty, emergency and housing expense reliefs), construction and maintenance of welfare institutions, among other expenses (US Census Bureau, 2020).

[32] Results using the $t-1$ lag of the immigrant share are reported in Panel B of Appendix Table A5. They also reveal null effects in all revenue and expenditure categories except welfare spending. In this case, the direction of the short-run and dynamic effects is the same, but both larger in magnitude.

Table 5: 2SLS results - Effects of low-skilled immigration on local public finance outcomes

	Total revenue		Taxes		Transfers	
	(1)	(2)	(3)	(4)	(5)	(6)
Share of low-skilled immigrants at t	1.368	-4.031	-1.443	-0.520	-0.883	-2.645*
	(3.674)	(3.548)	(2.349)	(2.701)	(2.229)	(1.544)
Share of low-skilled immigrants at $t-2$		6.330**		-1.082		2.067
		(3.171)		(2.252)		(2.640)
	Total expenditure		Education exp.		Welfare exp.	
	(7)	(8)	(9)	(10)	(11)	(12)
Share of low-skilled immigrants at t	-0.132	-2.984	3.273	1.193	0.335	1.860**
	(3.621)	(3.635)	(2.608)	(1.616)	(0.825)	(0.834)
Share of low-skilled immigrants at $t-2$		3.344		2.439		-1.788***
		(3.186)		(2.623)		(0.650)
Observations	3610	3610	3610	3610	3610	3610

The table reports dynamic shift-share 2SLS estimates of local public finance outcomes on the low-skilled immigration and its lag (two election years before) instrumented by the predicted low-skilled immigration (and its respective lag). We restricted the panel to 2000 to 2016 in order to have comparable samples with specifications including lags. All regression specifications have baseline controls interacted with time dummies, commuting zone and election year fixed effects and are weighted by the voting-age population. Clustered standard errors at the commuting zone level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

To summarize, we find that in places with more low-skilled immigrants, specific groups of natives might have been negatively affected in the short run in terms of their economic conditions, while Republican vote shares increased. However, within a decade, both the negative consequences for employment and the increase in welfare expenditure per capita almost completely fade, but not the Republican voting shift. These findings suggest that the dynamic adjustment of local economies to immigration is an important feature to explain the political effects of immigration.

5.3 Heterogeneity

To see how those effects vary with respect to specific characteristics of the population, we ran a heterogeneity analysis in the same spirit of Mayda et al. (2022). The difference is that instead of comparing the effects of high and low-skilled immigration, we compare the short-run and the dynamic adjustment components. We do that by interacting the immigrant share (and its lag) with the heterogeneity variable measured in 1980 (before our study period) and replicating the same with the instrument. We look at both Republican voting as an outcome and the two variables potentially mediating this effect (unemployment rate among low-skilled natives and welfare expenditure per capita). We removed the urban share and the low-skilled native share from the set of controls since we use variants of those variables for heterogeneity purposes. In the first column of the following tables, we report the estimates with that reduced set of controls as a benchmark.

We look at four variables: the low-to-high-skilled ratio among natives, to capture the idea of skill complementarity (we expect negative effects to be more pronounced where natives and immigrants are potential substitutes in terms of skills); the share of citizens living in rural areas, to check if the effect is higher in places where actual contact with immigrants might be less frequent; the ratio of government spending over total income, to capture the idea that natives might be more likely to see immigrants as competitors for public resources in places where the State provides more; and finally the share of low-skilled whites, to take into account groups that were particularly relevant to explain the recent right-wing shift in the U.S.

We report results on voting in Appendix Table A6. First, they suggest that the short-run shift to the right is more prominent in places where the low- to high-skilled native ratio is higher, which is expected based on the threat of potential substitution. Those places also exhibit some attenuation of the effect in the medium run, which is not present in places with more high-skilled natives in the population. Second, the short-run right-wing shift is considerably higher in places with a higher government expenditure share of income, indicating that natives might indeed choose to vote based on the fear of

competition for public resources. Third, effects are mostly driven by places with a higher share of the low-skilled white population, a particularly relevant group to explain the Republican shift in the U.S. Finally, we find no significant differences in terms of the rural population share.

Results on unemployment of low-skilled natives are shown in Appendix Table A7. Our findings are most in line with the ones reported for voting outcomes. First, both the short-run increase and the attenuation in the medium run take place in regions with a higher low-to-high-skilled native ratio, in line with the idea of temporary competition with natives (although the medium-run interaction coefficient is not significant). Second, unemployment among low-skilled natives increases more in places with a higher government spending ratio. It also decreases more in the medium run (although not significant). Since the government's share of income is negatively correlated with the share of private jobs, those places might be more subject to competition in the short run. Third, effects are again mostly driven by places with a larger share of low-skilled white population, especially the short-run coefficient. Again, the rural share of citizens does not seem to be a relevant heterogeneity driver.

Finally, Appendix Table A8 shows the results on welfare expenditure per capita. We find the effects in the short run to be mostly driven by rural areas. Taken together with our earlier findings, this is evidence that unemployment and welfare effects are possibly not necessarily happening in the same places. In this case, the low-to-high-skilled ratio, the government expenditure to income ratio, and the share of low-skilled whites do not seem to be relevant heterogeneity variables.

In summary, the effects we find on voting and unemployment are mostly driven by places with previously more low-skilled natives relative to high-skilled ones, where the government had a larger share of income, and where there are relatively more low-skilled whites. The rural share seems to play a role only in the increase of welfare expenditure coming from immigration in the short run, but not for the other variables.

5.4 Causal Mediation Analysis

Until now, we observed that after disentangling short-run and dynamic adjustment components of SSIV estimates, we find that local economic conditions of natives changed at the same time that Republican vote shares increased due to low-skilled immigration inflows. But can we indeed measure the contribution of the labor market and provision of public goods channels to explain the voting effect? In this section, we will employ a causal mediation analysis method[33] to answer that question.

5.4.1 Mediation method

Since our identification relies on an Instrumental Variable (IV) strategy, we apply a framework of causal mediation analysis using a single IV to analyze the channels through which those effects take place (Dippel et al., 2022). We use the method to calculate direct and indirect (mediated) components of the "total" effect of low-skilled immigration on voting in the short and the medium run[34].

Let Y be our outcome variable (Republican vote share), T the matrix of treatment variables (contemporaneous and lagged low-skilled immigrant shares), M a mediator variable (in our case, native labor market and local public finance outcomes) and Z the instrument matrix (contemporaneous and lagged SSIVs). Without loss of generality, we will not deal with covariates or fixed effects in drawing this framework. What we have done so far in the paper is to estimate a model - that we call model A - where T is a function of Z and a stochastic error term, so that $T = f_T(Z, \epsilon_T)$ and Y is a function of T and another stochastic error term, like $Y = g_Y(T, \eta_Y)$, given the usual exclusion restriction for the instrument, that is, $Z \perp\!\!\!\perp (\epsilon_T, \eta_Y)$.

What we aim to do is to estimate a different model - that we call model B - in which the treatment-instrument relation is the same, but we add M as a function of T, that

[33] The method was initially proposed by Baron and Kenny (1986) and was later developed by Imai et al. (2010), Imai et al. (2011), MacKinnon (2012), Pearl (2014) and Heckman and Pinto (2015), to cite some examples.

[34] Those are respectively the 2SLS estimates of β_1 and β_2 in equation 8.

is, $M = f_M(T, \epsilon_M)$. Now Y is a function of T and M, like $Y = f_Y(T, M, \epsilon_Y)$. Consequently, there is a direct effect of T on Y, and an indirect one, mediated by M. This framework assumes that T has a causal effect on M that can be identified using the same instrument Z, meaning that $Z \perp\!\!\!\perp (\epsilon_T, \epsilon_M)$.

Under linearity, we can estimate the parameters of interest using standard econometric techniques such as 2SLS. Dippel, Gold, et al. (2020, 2022) show that identification of causal parameters from a linear system of equations based on model B implies the estimation of two systems of equations from which we can recover β_Y^T, the direct effect, and $\beta_M^T \cdot \beta_Y^M$, the indirect (mediated) effect. In the first mediation system, we obtain β_M^T (the effect of the treatment on the mediator) in its second stage, using equation 10 below with the predicted $\hat{T}$ from equation 9.

$$\text{First Stage: } T = \beta_T^Z \cdot Z + \epsilon_T \tag{9}$$

$$\text{Second Stage: } M = \beta_M^T \cdot \hat{T} + \epsilon_M \tag{10}$$

In order to estimate β_Y^M (the effect of the mediator on the outcome) and β_Y^T (the direct effect), we use the second mediation system composed by equations 11 and 12.

$$\text{First Stage: } M = \gamma_M^Z \cdot Z + \gamma_M^T \cdot T + \epsilon_T, \tag{11}$$

$$\text{Second Stage: } Y = \beta_Y^M \cdot \hat{M} + \beta_Y^T \cdot T + \epsilon_Y \tag{12}$$

Considering the equations discussed above and, in particular, the error terms, this kind of model requires some assumptions for the identification of causal parameters. The first one is the usual exclusion restriction for the instrument, not only for the outcome but also for the mediator ($Z \perp\!\!\!\perp (\epsilon_T, \epsilon_M, \epsilon_Y)$). The second is the independence between the error terms of equations 11 and 12: $\epsilon_T \perp\!\!\!\perp \epsilon_Y$. This hypothesis means that unobservables affecting immigration do not correlate with unobservables affecting voting behavior unless through the mediator. For example, when we are looking at the labor market channel, we allow unobserved shocks that impact immigration (T) to also affect voter preferences

(Y) through labor markets (M), but not through unrelated channels. In other words, T is endogenous in a regression of Y on T only because of confounders that affect T and M, and Z is a valid instrument for M only conditional on T. The other error terms are allowed to correlate ($\epsilon_T \not\perp\!\!\!\perp \epsilon_M$, $\epsilon_M \not\perp\!\!\!\perp \epsilon_Y$ and $\epsilon_T \not\perp\!\!\!\perp \epsilon_Y|\epsilon_M$).

Importantly, the direct effect is a residual from the indirect effect estimation. We interpret it as the proportion of the total effect that is not explained by the chosen mediator. It could, however, be mediated by other variables that are not in the model. This points to a limitation: when we look at the mediated effects by each channel separately, we are implicitly imposing a *coeteris paribus* assumption for all other channels. If we have evidence rejecting mediation through a given variable, we may be interested in looking for evidence for other variables mediating the effect of immigration on voting, and so on. In our case, we proceed by looking at mediators individually[35] and, after that, we create a measure to evaluate the joint mediated effect of many mediators to get more credible and interpretable estimates of the direct component.

In the just-identified case, - when the number of treatment, instrument, and mediator variables is the same - the sum over direct and indirect components is numerically equivalent to the total effect. In our setting, we address each possible mediator separately, so we have an over-identified estimate, given we have two treatments and two instruments (contemporaneous and lagged). This will allow us to understand how mediators operate in the short run and in the dynamic adjustment component. We can still extract some information from the results by looking at the significance of direct and indirect estimates and by expressing the indirect effect as a percentage of the total effect. As we show later, the sum of the direct and the indirect components is quite close to the total effects we have shown earlier.

[35]Results using more than one mediator simultaneously are available upon request. In general, they are much noisier and the magnitudes are hard to interpret.

5.4.2 Mediation results

We report the results from applying this mediation framework in Table 6. We focused on the unemployment rate among low-skilled natives (Panel A) and the local welfare expenditure per capita (Panel B) as mediators because they seem to be more related to the effect of low-skilled immigration on voting[36]. For the sake of simplicity, we only report the direct effect, the relation between low-skilled immigration and the mediator (β_M^T), and the relation between the mediator and Republican voting (β_Y^M)[37], the indirect effect - which is the product of these two - and its percentage over the total effect estimated in section 4.2. We report those estimates considering the contemporaneous and the two-period lagged low-skilled immigration shares to capture short-run and dynamic adjustment mediation components. We report first stage F-statistics relative to the standard IV first stage (equation 9) but also for the first stage of the second mediation model in equation 11. Dippel, Ferrara, et al. (2020) suggest that a conservative approach to draw conclusions over all three effects (total, direct and indirect) would consider this F-statistic to be above 30. But if interest lies only on the indirect effect, the commonly used cutoff of 10 seems reasonable enough. We also report the p-value for the Anderson-Rubin test for each mediation model (in brackets).

[36] We ran the same models using the other labor market and public finance outcomes shown earlier and we find no evidence for mediation. Results are reported in Appendix Tables A9 and A10. Importantly, although we find positive labor market effects of immigration for high-skilled natives, we do not find that those effects mediate voting shifts.

[37] This will be the same for both contemporaneous and lagged immigration since all of them are used as regressors together in equation 12. This coefficient captures the relationship between mediator and outcome after conditioning on the instrumented treatment.

Table 6: Mediation analysis results for the main mediators

	Coefficients						First Stage F-stat	
	DE (β_Y^T)	β_M^T	β_Y^M	IE ($\beta_M^T \cdot \beta_Y^M$)	TE	IE(% of TE)	Eq. 9	Eq. 11
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Low-skilled native unemployment rate as mediator								
Share of low-skilled immigrants at t	1.052**	1.252***	1.518***	1.901***	3.224***	59.0	66.72	10.55
	(0.512)	(0.340)	(0.399)	(0.718)	(0.669)			
Share of low-skilled immigrants at $t-2$	-0.033	-0.881*	1.518***	-1.337	-0.426	314.0	77.91	10.55
	(0.430)	(0.524)	(0.399)	(0.869)	(0.588)			
Anderson-Rubin Test P-value		[0.004]	[0.000]					
Panel B: Welfare expenditure per capita as mediator								
Share of low-skilled immigrants at t	0.635	1.860**	0.900**	1.673	3.224***	51.9	66.72	5.99
	(0.439)	(0.834)	(0.434)	(1.102)	(0.669)			
Share of low-skilled immigrants at $t-2$	0.089	-1.788***	0.900**	-1.608*	-0.426	377.6	77.91	5.99
	(0.416)	(0.650)	(0.434)	(0.972)	(0.588)			
Anderson-Rubin Test P-value		[0.085]	[0.000]					
Observations	3610							

The table reports a summary of the causal mediation estimates. The coefficients reported are based on equations 9 to 12. "DE" stands for "Direct Effect" (non-mediated), "IE" stands for "Indirect Effect" (mediated) and "TE" is the "Total Effect", the ones that we estimated in section 4.2. Since the model with the dynamic shift-share is over-identified for one mediator, the sum of the "DE" and the "IE" is not exactly the "TE". In columns (7) and (8), we report F-statistics for the two first stages presented in equations 9 and 11 respectively. All regression specifications have baseline controls interacted with time dummies, commuting zone and election year fixed effects and are weighted by the voting-age population. Clustered standard errors at the commuting zone level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Mediation results for the unemployment rate among low-skilled natives reinforce our previous findings. They suggest that this variable explains 60% of the short-run effect of low-skilled immigration on voting, as shown in Panel A of Table 6. Not only does immigration affect unemployment positively, but also unemployment is positively associated with voting for the Republicans. The resulting indirect coefficient in column 4 is significant at 1%. In the medium run, the indirect coefficient is not significant. We interpret this as saying that employment adjustment dynamics derived from past immigration shocks do not play a role in explaining dynamics in voting patterns. However, if this dynamic indirect coefficient was significant, it would account for three times the adjustment observed in voting patterns. This implies that some other factors explain why we do not observe a more relevant attenuation of the voting shift in the medium run. We will come back to this point later.

We do not find evidence that local welfare expenditure per capita works as a mediator for the effect of low-skilled immigration on voting in the short run, as shown in Panel B of Table 6. It would account for 52% of the total effect, but it is not significant. Nevertheless, in the medium run, the indirect component is not only significant but also higher in magnitude compared to the total effect (378%). The interpretation is that if there is no other channel beyond welfare expenditure at play, then the attenuation effect on voting over time would be higher. As in the unemployment case, this suggests the existence of other channels operating in the inverse direction (increasing the voting shift dynamically). The result is that the Republican shift observed in the short run is not attenuated over time.

Those single variable mediation exercises implicitly suppose independence from each other and from other channels that we did not test for, as explained earlier. However, it is odd to say, for example, that each mediator separately explains a major part of the total effects. It is natural to argue that multiple mediators operate simultaneously to explain the effect of immigration on electoral behavior. When we do not care about the identification of causal effects, the inclusion of multiple mediators can be straightforward. In our case, though, we estimate causal relations using instrumental variables.

To run a model with more mediators would require more instruments to identify direct and indirect effects that sum up to the total effect of interest. This would create a need to search for other sources of exogenous variation for low-skilled immigration, which is not a straightforward enterprise. Also, the real number of mediators can be arbitrarily large, implying an arbitrarily large number of instruments to identify effects.

Having said that, we could still be interested in measuring the whole economic-related mediated effect of low-skilled immigration on voting. If we can do that, the residual from that estimation (the direct effect) would have a more explicit interpretation: the part of the immigration-on-voting effect not explained by labor market or public finance outcomes. To answer that without departing from our framework, and acknowledging the potential correlation between mediators, we followed a strategy similar to Dippel et al. (2022)'s one. We created a latent variable using Principal Component Analysis (PCA) from the low-skilled native unemployment rate and welfare expenditure per capita[38]. It can be interpreted as a measure of the labor market and public finance factors that influence the relation between low-skilled immigration and voting for Republicans. We then applied the mediation model with this variable playing the mediator role. Although we are limited to interpreting the magnitude of the coefficients in these regressions, this approach allows us to have a more precise measurement of the degree of economic mediation underlying the relation between low-skilled immigration and voting for the Republicans. This can be done both for the short-run effect and the dynamic adjustment component. Since both mediators operate in the same way - immigration increases (decreases) mediator and mediator increases votes for Republicans in the short (longer) run -, the latent variable will show the same pattern.

Our conclusion from this exercise is that 59% of the short-run effect on voting can be explained by a mixture of increases in low-skilled native unemployment and welfare expenditure per capita, as shown in Table 7. The other 41% (the direct effect) is left unexplained by these variables. This component can be seen as a measure of the short-run

[38]The resulting variable is the first component, the one which accounts for the majority of the variation that is common to the two variables (53%). The first component is also the only one with an eigenvalue larger than 1, following the Kaiser rule.

impact of immigration on voting not related to the local economy. It could be related to sociotropic views of the economic impacts of immigration or other factors such as cultural distance, fear of diversity, or prejudice[39].

Results for the medium-run go in the same direction as the individual mediator exercises, but now the indirect coefficient is statistically significant and again higher than the total effect (361%). Our interpretation is that if the joint contribution of unemployment of low-skilled natives and local welfare spending is the only relevant channel at play, we should expect a higher attenuation of the effect of low-skilled immigration on voting after eight years. Then, there should be some other unobserved channel generating persistence on - or even amplifying - the short-run Republican shift over time. In other words, voters are still shifted towards the right-wing two election years after a low-skilled immigrant shock, even after some of the negative short-run consequences in living conditions have been compensated by general equilibrium adjustments. We argue that the negative consequences in the short run create a negative memory that persists over time.

To test whether results are not dependent on our choice of aggregation method (PCA), we used two other techniques. The first one is simply a composite index: we rescale variables to vary between 0 and 1 by subtracting each observation by its minimum and dividing by the difference between the maximum and the minimum, then we just take the average over them. The second one follows Kling et al. (2007): it is an average of the variables' z-scores, obtained by subtracting each observation from its mean and dividing by its standard deviation. We find the same results, as shown in Appendix Table A11. Our preferred approach, though, is to use the principal component because it does not pre-impose weights as the other methods.

[39] It could even be related to other economic factors that we do not study here, such as changes in real-estate prices, for example.

Table 7: Mediation analysis results for the PCA index

	Coefficients						First Stage F-stat	
	DE (β_Y^T)	β_M^T	β_Y^M	IE ($\beta_M^T \cdot \beta_Y^M$)	TE	IE(% of TE)	Eq. 9	Eq. 11
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PCA index as mediator								
Share of low-skilled immigrants at t	0.839**	26.370***	0.072***	1.888***	3.224***	58.6	66.72	19.10
	(0.419)	(6.438)	(0.020)	(0.710)	(0.669)			
Share of low-skilled immigrants at $t-2$	0.038	-21.492***	0.072***	-1.539**	-0.426	361.3	77.91	19.10
	(0.261)	(7.878)	(0.020)	(0.716)	(0.588)			
Anderson-Rubin Test P-value		[0.003]	[0.000]					
Observations	3610							

The table reports a summary of the causal mediation estimates. The coefficients reported are based on equations 9 to 12. "DE" stands for "Direct Effect" (non-mediated), "IE" stands for "Indirect Effect" (mediated) and "TE" is the "Total Effect", the ones that we estimated in section 4.2. Since the model with the dynamic shift-share is over-identified for one mediator, the sum of the "DE" and the "IE" is not exactly the "TE". In columns (7) and (8), we report F-statistics for the two first stages presented in equations 9 and 11 respectively. The mediator is the first principal component from the low-skilled native unemployment rate and the local welfare expenditure per capita from the full sample (1992-2016). All regression specifications have baseline controls interacted with time dummies, commuting zone and election year fixed effects and are weighted by the voting-age population. Clustered standard errors at the commuting zone level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

5.5 Survey analysis

So far, we have shown evidence that economic channels are relevant in explaining why voters shifted towards the right as a result of the increasing immigration of low-skilled workers in the U.S. This shift is coming from contemporaneous shocks, and it is persistent even after economic shocks fade away. We have shown that the higher presence of particular groups explains these patterns, mainly low-skilled white natives.

Those results suggest that natives might have changed political preferences more steadily after observing a negative shock in the short run. This is in line with the so-called *impressionable years hypothesis* (Krosnick & Alwin, 1989; Giuliano & Spilimbergo, 2014). In our context, this conjecture reads as follows: if people perceive a negative shock from low-skilled immigration when they are young (mostly when they enter the labor market), they might establish political preferences for anti-immigration policies in a very persistent and stable way. Based on this hypothesis, older people would show more stable preferences and would be less affected by those shocks, while younger cohorts would explain the ideological shift.

Besides that, changing preferences of immigrant descendants might at least partly explain the pattern we found. On average, we expect immigrants to be more liberal towards immigration policy than their native counterparts, but that difference might decrease as generations pass by. Children of immigrant parents will have a more similar background to children of native parents compared to their parents, which might imply more similar political preferences. That trend might intensify with each new generation. As stated before, most of the low-skilled immigrant population in the U.S. has a Hispanic origin, coming mostly from Central America and the Caribbean. That is why we call this the *Americanization of the Latin voter* hypothesis[40].

In this section, we shed light on those hypotheses by looking at individual-level survey data. Even though we do not provide a causal identification strategy, descriptive

[40]The two hypotheses are not necessarily mutually exclusive. For example, immigrant descendants might compete more directly with recently settled immigrants, so they can also be affected by shocks taking place in impressionable years.

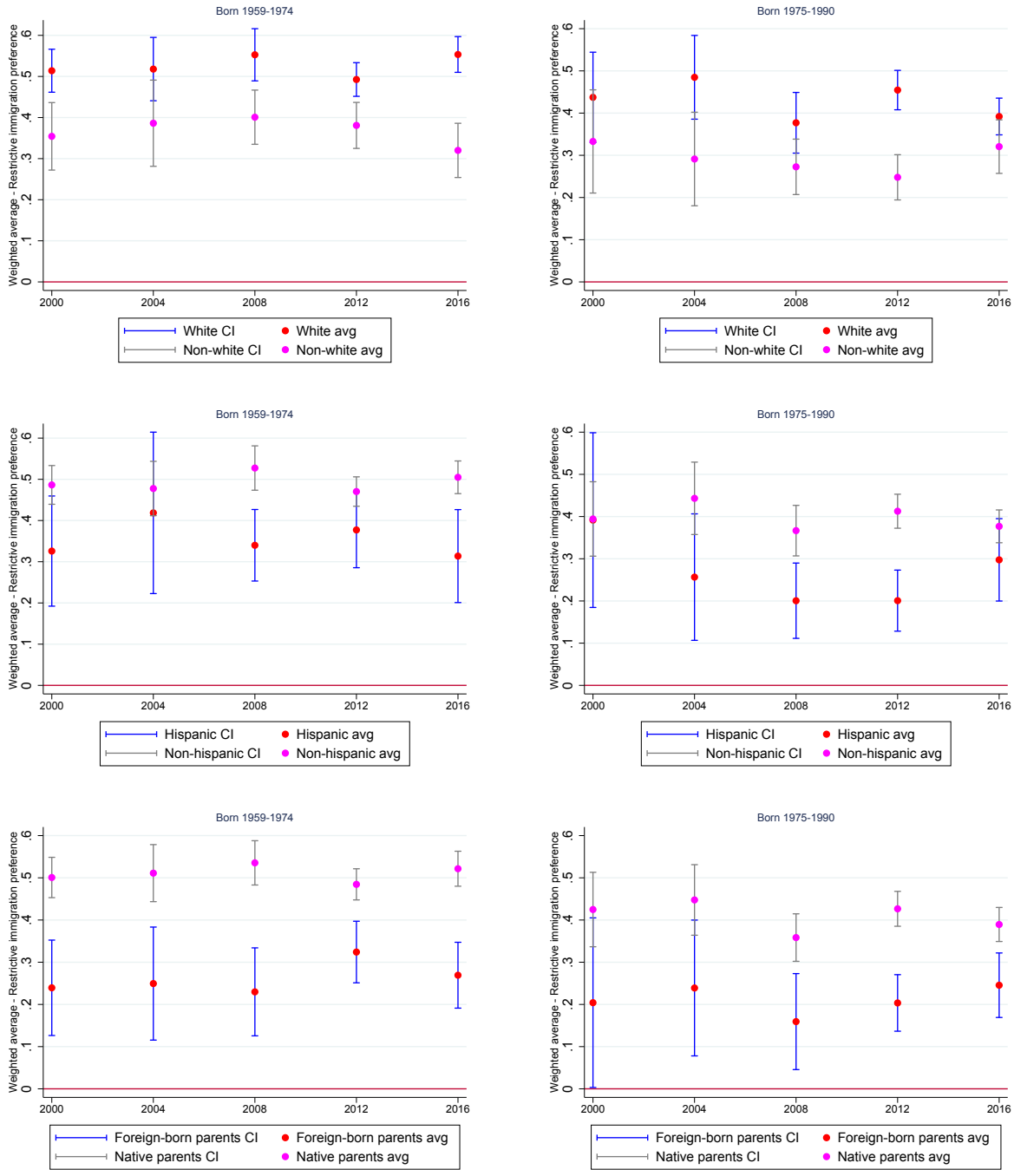
evidence at this level might suggest if those hypotheses are likely underlying the mechanisms of the phenomenon we study. To do that, we analyzed data from the American National Election Studies (ANES, 2021) in a pooled cross-section individual-level dataset, representative of the U.S. as a whole. We focused on presidential election years between 2000 and 2016 to match our main period of analysis, for which we have a total of 15,525 observations. We fix a cohort based on respondents' year of birth to avoid composition effects over time. We use the cohort definition provided by ANES' codebook. Here we focus on the cohort born between 1975 and 1990 (younger, more likely to be affected by events in impressionable years during our period of study) and the cohort born between 1959 and 1974 (older). In Figure 1, we plot the average of respondents[41] eliciting a preference for more restrictive immigration policies[42] in each year, breaking by race, parents' citizenship, and cohort.

Overall, we observe clear differences between Hispanics and whites, and also between first-generation and other generations. As expected, whites and children of native parents are considerably more likely against flexible immigration policies. But there are some remarkable trends in this period. The first one is the consolidation of an anti-immigration view among the older white population. For people born between 1959 and 1974 (between 42 and 57 years old in 2016), the distance between whites and other ethnicities considerably increases in more recent years. This is not the case for people born between 1975 and 1990 (between 26 and 41 years old in 2016).

[41] This is weighted by each year survey weights, so this is representative at the national level. Confidence intervals are calculated based on the survey design. In general, confidence intervals in the last three years tend to be smaller than in the first two because of differences in sample size across years.

[42] Anti-immigration preference is based on respondents answering that the number of admitted immigrants in the U.S. should decrease.

Figure 1: Preferences for restrictive immigration policies by race, parents' citizenship and cohort: ANES data from 2000 to 2016, presidential election years



Notes: Each figure plots the sample-weighted average (and confidence intervals) of respondents at the American National Election Studies (ANES) in a given year answering that the number of admitted immigrants in the U.S. should decrease. The top figures break this average between whites and non-whites, the middle figures between Hispanics and non-Hispanics, and the bottom figures between children of foreign-born parents and children of native parents. Figures on the left are composed of individuals born between 1959 and 1974, while on the right are composed of individuals born between 1975 and 1990.

The second one is a change in migration policy preferences among younger individuals. After 2008, the year of the global financial crisis, we observe a slightly increasing trend in anti-immigration preferences for younger Hispanics, which is a sign of the Americanization of the Latin voter hypothesis. In line with that, preferences for restrictive immigration policies also increased in the same period for children born between 1975 and 1990 to foreign-born parents (first-generation) and for Hispanics. We consider this as evidence for the Americanization of the Latin voter hypothesis. On the other hand, this trend is not as evident for older people (those born between 1959 and 1974). We interpret this as evidence for both the impressionable years hypothesis. So this implies that both phenomena are relevant in more recent years.

Finally, we argue that preferences for immigration policies became an increasingly important driver of party identification in the U.S. during our study period. In Appendix Table A12, we use the same ANES data in an OLS regression[43] where the dependent variable is the identification as Republican, while the main regressor is the same anti-immigration policy preference indicator. We interact the regressor with year dummies to capture changes over time, and we regress separately for cohorts born in 1975-1990 and 1959-1974. We find positive coefficients for all groups but significant only for the older cohort. The only significant interaction term coefficient shows up in 2016, meaning that Trump's election happened in a year where the relation between being Republican and anti-immigration was particularly strong. Including additional controls (Hispanic and first-generation) does not significantly change the results. However, on the cross-sectional variation, this inclusion points out that: 1) Hispanics tend to identify less as Republicans, but this is more evident among older people compared to younger ones, and 2) children of foreign-born parents also tend to identify less as Republicans, but this is more evident among younger people. These facts reinforce evidence of a more recent Americanization of the Latin voter phenomenon.

[43]Probit regressions yield similar results, available upon request.

6 Discussion

There are two main takeaways from our econometric analysis. The first one is that the shift towards the right in the U.S. derived from low-skilled immigration is produced immediately after the immigrant population increases and does not attenuate over time. The second one is that this pattern effect can be partially explained by changes in the economic conditions of specific groups of natives, through a temporary negative shock on employment for the low-skilled ones and a temporary rise in local welfare spending. We conclude that specific groups of voters might respond permanently to actual temporary shocks in economic conditions when voting for immigration policies.

Let us first discuss short-run effects. The increase in unemployment among low-skilled natives derived from low-skilled immigration can be interpreted as an impressionable years shock. We indeed find survey evidence that especially younger cohorts changed their attitudes towards immigration in our study period, especially after 2008. Besides that, the welfare expenditure effect is in line with evidence that voters prefer less redistribution if they feel there is competition with immigrants for public resources (Razin et al., 2011; Alesina & Stantcheva, 2020; Alesina et al., 2022). Misperception views towards immigrants may even amplify this effect, making them support less redistribution.

The literature on the political effects of immigration attributes less relevance to economic factors because the evidence so far does not seem to point in that way (Alesina & Tabellini, 2021). Some studies find positive economic effects of immigration and interpret this as evidence that economic channels are not at play (Tabellini, 2020). In our paper, we show that, when using SSIVs, failing to take account of the dynamic nature of those processes might result in misleading conclusions. When we properly disentangle between a more reliable estimate of the short-run effect of immigration and a dynamic adjustment component, we observe negative consequences for natives in the short term. In that sense, our results converge with other work documenting negative labor market effects of immigration and their possible association with voting patterns, such as Rozo and Vargas (2021).

We also find that those economic effects fade away within a decade, meaning that negative consequences for natives are short-lived. This is possibly rationalized by general equilibrium effects that take time to kick in. It is intuitive to think that if those effects are fading, this should play a role in attenuating the Republican shift. Indeed, our mediation results suggest that if the adjustment in economic conditions of natives were the only channel explaining the political shift towards the right, then we would expect this shift to also fade, at least by half in 8 years. However, the electoral effect is persistent. Trying to reconcile our findings with the literature (Alesina & Tabellini, 2021), we interpret this as evidence that other (possibly cultural) aspects could be at play in the medium run. In that sense, our results do not reject the cultural hypothesis, but place it as a medium-run mechanism, while effects on the local economy are relevant in the short term. More investigation on possible cultural channels could be an agenda for future research[44].

In terms of policy implications, we argue that information treatments or other interventions targeted at changing perceptions of natives towards immigrants may not be sufficient to change factors that influence voting through the economic conditions of natives. Our study concludes that particular groups of natives might have been affected by temporary economic shocks and responded by changing their political preferences in a rather permanent way. If the policy-maker wishes to avoid this kind of backlash, it should also mitigate the temporary negative effects of immigration on more vulnerable groups of natives, without necessarily changing immigration policies.

7 Final Remarks

This paper contributes to the evidence on the political effects of immigration by studying its short and medium-run dynamics and the possible economic channels through which they happen. We applied a dynamic shift-share approach to avoid conflating the short-run effect of immigration with the equilibrium adjustment component from earlier waves. We also employed a causal mediation technique to measure the con-

[44]We analyzed language distance as a proxy for cultural distance in the heterogeneity exercises (Adsera & Pytlikova, 2015). We expect that immigrants from more linguistic distant places bring different cultural aspects to the U.S., which might be threatening to some natives. Results are noisy and do not point to a clear conclusion. They are available upon request.

tribution of native labor market outcomes and local provision of public goods to explain voting effects. In a setting of U.S. commuting zones and presidential elections between 1992 and 2016, we analyzed the effect of low-skilled immigration on voting for Republican presidents, which are typically associated with an immigration-restrictive platform.

We found evidence that low-skilled immigration triggers a backlash in the short run, by increasing the Republican vote share. This right-wing shift is likely related to changes in economic conditions affecting particular groups of natives. In the same period, unemployment among low-skilled natives and local welfare expenditure per capita also increased as a result of contemporaneous immigration. However, in an 8-year window, the political effect is not attenuated, while most of the economic effects fade away. Although economic factors are relevant in explaining voting behavior changes due to immigration in the short run, we should expect the voting effects to attenuate if this is only an economically-rooted phenomenon. This suggests the interplay of unobserved - possibly non-economic - factors explaining this political preference shift persistence. Our mediation results reinforce this conclusion: around 60% of the short-run effect can be explained by a summary of the relevant economic variables, while in the medium run, it would account for a larger reduction in the Republican shift over time, if only those economic factors were relevant.

There is an open research agenda on better investigating non-economic channels and the reasons for this persistent backlash. We tried to provide indirect evidence on that by looking at survey data. Our analysis suggests that during our period of study, not only older white Americans increased their preference for restrictive immigration policies, but also younger, mostly first-generation Hispanics took the same direction. Taken together with our heterogeneity exercises pointing to effects being more pronounced in places with more low-skilled natives and whites, we consider two hypotheses possibly underlying our findings. The first is that people receive negative shocks in impressionable years and change their political preferences permanently, and the second that immigrant descendants converge to the preferences of native descendants.

In a sense, this paper tried to answer the question by Alesina and Tabellini (2021):

are the political effects of immigration explained by Economics or Culture? For the recent U.S. context, after we take proper account of the adjustment dynamics of immigration, we find that Economics is certainly relevant in the short run. But in the medium run, changes in the economic conditions of natives are not enough to explain why voting behavior shifts are persistent. We observe an hysteresis effect: after a negative shock, voters remember that and keep voting differently from before the shock, even if they do not feel the negative consequences of that shock anymore. Whether this is explained by culture is still an open question. The policy implication is that we should be interested in mitigating short-run effects for natives and not only focus on information or misperception correction treatments, even though they are also relevant. Immigration might generate economic winners and losers that respond differently to their political preferences.

References

Adão, R., Kolesár, M., & Morales, E. (2019). Shift-share designs: Theory and inference. *The Quarterly Journal of Economics*, *134*(4), 1949–2010.

Adsera, A., & Pytlikova, M. (2015). The role of language in shaping international migration. *Economic Journal*, *125*(586), F49–F81.

Alesina, A., Miano, A., & Stantcheva, S. (2022). Immigration and redistribution. *Review of Economic Studies*, *Forthcoming*.

Alesina, A., & Stantcheva, S. (2020). Diversity, immigration, and redistribution. *AEA Papers and Proceedings*, *110*, 329–34.

Alesina, A., & Tabellini, M. (2021). The political effects of immigration: Culture or economics? *Working paper*.

Allport, G. (1954). *The nature of prejudice*. Addison-Wesley Reading, MA.

Altonji, J. G., & Card, D. (1991). The effects of immigration on the labor market outcomes of less-skilled natives. *Immigration, trade, and the labor market* (pp. 201–234). University of Chicago Press.

ANES. (2021). American National Election Studies (ANES) Time Series Cumulative Data File [dataset and documentation]. https://www.electionstudies.org

Ansolabehere, S., Rodden, J., & Snyder, J. (2008). The strength of issues: Using multiple measures to gauge preference stability, ideological constraint, and issue voting. *American Political Science Review*, *102*(2), 215–232.

Autor, D., & Dorn, D. (2013). The growth of low-skill service jobs and the polarization of the US labor market. *American Economic Review*, *103*(5), 1553–97.

Autor, D., Dorn, D., & Hanson, G. (2013). The China syndrome: local labor market effects of import competition in the United States. *American Economic Review*, *103*(6), 2121–68.

Autor, D., Dorn, D., & Hanson, G. (2018). When work disappears: Manufacturing decline and the falling marriage-market value of young men. *American Economic Review: Insights*, *1*(2).

Autor, D., Dorn, D., Hanson, G., & Majlesi, K. (2020). Importing political polarization? the electoral consequences of rising trade exposure. *American Economic Review*, *110*(10), 3139–83.

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

Baron, R. M., & Kenny, D. A. (1986). The moderator–mediator variable distinction in social psychological research: Conceptual, strategic, and statistical considerations. *Journal of personality and social psychology*, *51*(6), 1173.

Barone, G., D'Ignazio, A., de Blasio, G., & 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.

Bartik, T. J. (1991). *Who benefits from state and local economic development policies?* WE Upjohn Institute for Employment Research.

Battisti, M., Felbermayr, G., Peri, G., & Poutvaara, P. (2018). Immigration, search and redistribution: A quantitative assessment of native welfare. *Journal of the European Economic Association*, *16*(4), 1137–1188.

Becker, S. O., & Ferrara, A. (2019). Consequences of forced migration: A survey of recent findings. *Labour Economics*, *59*, 1–16.

Blau, F. D., & Mackie, C. (2017). *The economic and fiscal consequences of immigration*. The National Academies Press.

Borjas, G. J. (2003). The labor demand curve is downward sloping: Reexamining the impact of immigration on the labor market. *The Quarterly Journal of Economics*, *118*(4), 1335–1374.

Borjas, G. J. (2017). The wage impact of the marielitos: A reappraisal. *Industrial and Labor Relations Review*, *70*(5), 1077–1110.

Borusyak, K., Hull, P., & Jaravel, X. (2022). Quasi-experimental shift-share research designs. *Review of Economic Studies*, *89*(1).

Campante, F. R., Depetris-Chauvin, E., & Durante, R. (2021). The virus of fear: The political impact of Ebola in the US. *Working Paper*.

Campello, D., & Zucco Jr, C. (2016). Presidential success and the World economy. *The Journal of Politics*, *78*(2), 589–602.

Card, D. (1990). The impact of the Mariel Boatlift on the Miami labor market. *Industrial and Labor Relations Review*, *43*(2), 245–257.

Card, D. (2001). Immigrant inflows, native outflows, and the local labor market impacts of higher immigration. *Journal of Labor Economics*, *19*(1), 22–64.

Chandler, C. R., & Tsai, Y. (2001). Social factors influencing immigration attitudes: An analysis of data from the general social survey. *The Social Science Journal*, *38*(2), 177–188.

Chassamboulli, A., & Palivos, T. (2014). A search-equilibrium approach to the effects of immigration on labor market outcomes. *International Economic Review*, *55*(1), 111–129.

Che, Y., Lu, Y., Pierce, J. R., Schott, P. K., & Tao, Z. (2022). Does trade liberalization with china influence U.S. elections? *Working Paper*.

Citrin, J., Green, D. P., Muste, C., & Wong, C. (1997). Public opinion toward immigration reform: The role of economic motivations. *The Journal of Politics*, *59*(3), 858–881.

Conconi, P., Facchini, G., Steinhardt, M. F., & Zanardi, M. (2020). The political economy of trade and migration: Evidence from the U.S. congress. *Economics & Politics*, *32*(2), 250–278.

Dahlberg, S. (2013). Does context matter – The impact of electoral systems, political parties and individual characteristics on voters' perceptions of party positions. *Electoral Studies*, *32*(4), 670–683.

DellaVigna, S., & Kaplan, E. (2007). The Fox News Effect: Media Bias and Voting. *The Quarterly Journal of Economics*, *122*(3), 1187–1234.

Dippel, C., Ferrara, A., & Heblich, S. (2020). Causal mediation analysis in instrumental-variables regressions. *The Stata Journal*, *20*(3), 613–626.

Dippel, C., Gold, R., Heblich, S., & Pinto, R. (2020). Mediation analysis in IV settings with a single instrument. *Working Paper*.

Dippel, C., Gold, R., Heblich, S., & Pinto, R. (2022). The effect of trade on workers and voters. *Economic Journal*, *132*(641), 199–217.

Dustmann, C., & Frattini, T. (2014). The fiscal effects of immigration to the UK. *Economic Journal*, *124*(580), F593–F643.

Dustmann, C., & Preston, I. (2006). Is immigration good or bad for the economy? analysis of attitudinal responses. *The economics of immigration and social diversity*. Emerald Group Publishing Limited.

Dustmann, C., Schönberg, U., & Stuhler, J. (2016). The impact of immigration: Why do studies reach such different results? *Journal of Economic Perspectives*, *30*(4), 31–56.

Dustmann, C., Vasiljeva, K., & Damm, A. P. (2019). Refugee migration and electoral outcomes. *Review of Economic Studies*, *86*(5), 2035–2091.

Facchini, G., & Mayda, A. M. (2009). Does the welfare state affect individual attitudes toward immigrants? Evidence across countries. *The Review of Economics and Statistics*, *91*(2), 295–314.

Facchini, G., & Steinhardt, M. F. (2011). What drives U.S. immigration policy? Evidence from congressional roll call votes. *Journal of Public Economics*, *95*(7), 734–743.

Feler, L., & Senses, M. (2017). Trade shocks and the provision of local public goods. *American Economic Journal: Economic Policy*, *9*(4), 101–43.

Friedberg, R. M., & Hunt, J. (1995). The Impact of Immigrants on Host Country Wages, Employment and Growth. *Journal of Economic Perspectives*, *9*(2), 23–44.

Giuliano, P., & Spilimbergo, A. (2014). Growing up in a recession. *The Review of Economic Studies*, *81*(2), 787–817.

Giuliano, P., & Tabellini, M. (2021). The Seeds of Ideology: Historical Immigration and Political Preferences in the United States. *Harvard Business School Working Paper 20-118*.

Goldsmith-Pinkham, P., Sorkin, I., & Swift, H. (2020). Bartik instruments: What, when, why, and how. *American Economic Review*, *110*(8), 2586–2624.

Hainmueller, J., & Hiscox, M. J. (2010). Attitudes toward highly skilled and low-skilled immigration: Evidence from a survey experiment. *American Political Science Review*, *104*(1), 61–84.

Hainmueller, J., Hiscox, M. J., & Margalit, Y. (2015). Do concerns about labor market competition shape attitudes toward immigration? New evidence. *Journal of International Economics*, *97*(1), 193–207.

Hainmueller, J., & Hopkins, D. J. (2014). Public attitudes toward immigration. *Annual Review of Political Science*, *17*, 225–249.

Halla, M., Wagner, A. F., & Zweimüller, J. (2017). Immigration and voting for the far right. *Journal of the European Economic Association*, *15*(6), 1341–1385.

Hawley, G. (2013). Issue voting and immigration: Do restrictionist policies cost congressional republicans votes? *Social Science Quarterly*, *94*(5), 1185–1206.

Healy, A. J., Persson, M., & Snowberg, E. (2017). Digging into the pocketbook: Evidence on economic voting from income registry data matched to a voter survey. *American Political Science Review*, *111*(4), 771–785.

Heckman, J. J., & Pinto, R. (2015). Econometric mediation analyses: Identifying the sources of treatment effects from experimentally estimated production technologies with unmeasured and mismeasured inputs. *Econometric reviews*, *34*(1-2), 6–31.

Iftikhar, Z., & Zaharieva, A. (2019). General equilibrium effects of immigration in Germany: Search and matching approach. *Review of Economic Dynamics*, *31*, 245–276.

Imai, K., Keele, L., Tingley, D., & Yamamoto, T. (2011). Unpacking the black box of causality: Learning about causal mechanisms from experimental and observational studies. *American Political Science Review*, *105*(4), 765–789.

Imai, K., Keele, L., & Yamamoto, T. (2010). Identification, inference and sensitivity analysis for causal mediation effects. *Statistical science*, *25*(1), 51–71.

Jaeger, D. A., Ruist, J., & Stuhler, J. (2019). Shift-share instruments and dynamic adjustments: The case of immigration. *Working Paper*.

Kling, J. R., Liebman, J. B., & Katz, L. F. (2007). Experimental analysis of neighborhood effects. *Econometrica*, *75*(1), 83–119.

Krosnick, J. A., & Alwin, D. F. (1989). Aging and susceptibility to attitude change. *Journal of Personality and Social Psychology*, *57*(3), 416.

Lewis, E., & Peri, G. (2015). Immigration and the Economy of Cities and Regions. *Handbook of Regional and Urban Economics* (pp. 625–685). Elsevier.

MacKinnon, D. (2012). *Introduction to statistical mediation analysis*. Routledge.

Markus, G. B. (1988). The impact of personal and national economic conditions on the presidential vote: A pooled cross-sectional analysis. *American Journal of Political Science*, *32*(1), 137–154.

Mayda, A. M. (2006). Who is against immigration? a cross-country investigation of individual attitudes toward immigrants. *The Review of Economics and Statistics*, *88*(3), 510–530.

Mayda, A. M., Peri, G., & Steingress, W. (2022). The political impact of immigration: Evidence from the United States. *American Economic Journal: Applied Economics*, *14*(1), 358–89.

Mayda, A. M., Senses, M., & Steingress, W. (2020). The impact of immigration on local tax revenues and public expenditures in the united states. *Working Paper*.

MEDSL. (2018). County Presidential Election Returns 2000-2016, MIT Election Data Science Lab. https://doi.org/10.7910/DVN/VOQCHQ

Moreira, M. J. (2009). Tests with correct size when instruments can be arbitrarily weak. *Journal of Econometrics*, *152*(2), 131–140.

Ottaviano, G. I., & Peri, G. (2012). Rethinking the effect of immigration on wages. *Journal of the European Economic Association*, *10*(1), 152–197.

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

Pearl, J. (2014). Interpretation and identification of causal mediation. *Psychological methods*, *19*(4), 459.

Peri, G., & Yasenov, V. (2019). The labor market effects of a refugee wave: Synthetic control method meets the Mariel Boatlift. *Journal of Human Resources*, *54*(2), 267–309.

Pew Research Center. (2015). On views of immigrants, Americans largely split along party lines. Retrieved March 5, 2021, from https://www.pewresearch.org/fact-tank/2015/09/30/on-views-of-immigrants-americans-largely-split-along-party-lines/

Politico.com. (2017). Full text: 2017 Donald Trump inauguration speech transcript. Retrieved February 26, 2020, from https://www.politico.com/story/2017/01/full-text-donald-trump-inauguration-speech-transcript-233907

Preston, I. (2014). The effect of immigration on public finances. *Economic Journal*, *124*(580), F569–F592.

Razin, A., Sadka, E., & Suwankiri, B. (2011). *Migration and the welfare state: Political-economy policy formation*. MIT Press.

Rozo, S., & Vargas, J. F. (2021). Brother or invaders? how crisis-driven migrants shape voting behavior. *Journal of Development Economics*, *150*.

Ruggles, S., Flood, S., Goeken, R., Grover, J., Meyer, E., Pacas, J., & Sobek, M. (2020). IPUMS USA: Version 10.0 [dataset]. *Minneapolis: University of Minnesota*.

Scheve, K. F., & Slaughter, M. J. (2001). Labor Market Competition and Individual Preferences Over Immigration Policy. *The Review of Economics and Statistics*, *83*(1), 133–145.

Speciale, B. (2012). Does immigration affect public education expenditures? Quasi-experimental evidence. *Journal of Public Economics*, *96*(9-10), 773–783.

Steinmayr, A. (2021). Contact versus exposure: Refugee presence and voting for the far-right. *The Review of Economics and Statistics*, *103*(2), 310–327.

Tabellini, M. (2020). Gifts of the immigrants, woes of the natives: Lessons from the age of mass migration. *Review of Economic Studies*, *87*(1), 454–486.

US Census Bureau. (2020). Annual Survey of State and Local Government Finance Data. https://www2.census.gov/programs-surveys/gov-finances/datasets/historical/_IndFin_1967-2012.zip

Vertier, P., & Viskanic, M. (2018). Dismantling the "jungle": Migrant relocation and extreme voting in France. *SSRN Working Paper 2963641*.

Viskanic, M. (2017). Fear and loathing on the campaign trail: Did immigration cause Brexit? *SSRN Working Paper 2941611*.

Appendix

Tables

Table A1: Weighted commuting zone averages of the main variables over years

	Years						
	1992	1996	2000	2004	2008	2012	2016
Share of immigrants (%)	9.50	11.38	13.31	14.29	15.29	15.88	16.49
	(0.166)	(0.180)	(0.195)	(0.192)	(0.189)	(0.187)	(0.181)
Share of low-skilled immigrants (%)	3.33	3.80	4.30	4.38	4.46	4.26	4.11
	(0.068)	(0.071)	(0.073)	(0.069)	(0.064)	(0.058)	(0.053)
Share of high-skilled immigrants (%)	6.16	7.58	9.01	9.91	10.83	11.62	12.38
	(0.101)	(0.114)	(0.127)	(0.129)	(0.131)	(0.134)	(0.133)
Republican vote share (%)	37.77	41.43	48.32	50.82	45.84	47.66	47.08
	(0.061)	(0.093)	(0.126)	(0.117)	(0.120)	(0.131)	(0.152)
Native unemployment rate (%)	6.13	5.91	5.70	6.11	6.54	9.69	5.97
	(0.011)	(0.011)	(0.014)	(0.013)	(0.015)	(0.025)	(0.012)
Native unemployment rate low-skilled (%)	14.14	14.73	15.32	16.57	17.82	25.20	16.22
	(0.031)	(0.042)	(0.056)	(0.045)	(0.045)	(0.056)	(0.040)
Native hourly average wages (constant US$)	16.19	17.13	18.09	17.21	16.33	16.23	17.02
	(0.402)	(0.414)	(0.437)	(0.415)	(0.400)	(0.388)	(0.397)
Native hourly average wages low-skilled (constant US$)	11.85	12.07	12.31	10.68	9.06	8.88	10.13
	(0.258)	(0.243)	(0.304)	(0.183)	(0.121)	(0.158)	(0.630)
Total revenue per capita (constant US$)	3.21	3.35	3.50	3.85	4.14	3.89	4.00
	(0.175)	(0.162)	(0.143)	(0.169)	(0.208)	(0.178)	(0.203)
Total expenditure per capita (constant US$)	3.23	3.29	3.57	3.89	4.02	3.87	3.95
	(0.166)	(0.150)	(0.158)	(0.173)	(0.182)	(0.186)	(0.206)
Expenditure in welfare per capita (constant US$)	0.159	0.150	0.150	0.152	0.149	0.145	0.123
	(0.037)	(0.034)	(0.032)	(0.031)	(0.031)	(0.033)	(0.029)

The table reports the weighted average of each variable in terms of the commuting zone adult population (18+). Standard deviations in parentheses are clustered at the commuting zone level.

Table A2: OLS results - Control variables coefficients of Table 1

	Republican vote share (Presidential elections)
Controls	(3)
Average per capita income	0.013
	(0.016)
Share of African-American citizens	-0.449***
	(0.170)
Share of urban citizens	-0.012
	(0.013)
Share of male citizens	2.022***
	(0.276)
Share of married citizens	-0.956***
	(0.125)
Share of low-skilled citizens	-1.307***
	(0.118)
Labor market participation rate	-0.104
	(0.115)
Bartik employment instrument (millions)	-0.552*
	(0.325)
ADH instrument for trade exposure (USD billions)	0.093**
	(0.042)

The table reports the coefficients on the covariates used in column (3) of Table 1 for the OLS regression of the low-skilled immigration on the share of Republican votes in presidential elections. It includes commuting zone and election year fixed effects and is weighted by the voting-age population. Clustered standard errors at the commuting zone level are shown in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A3: Effects of low-skilled immigration on voting: lagged immigration on $t-1$ instead of $t-2$

Panel A: First stage		
	(1)	(2)
	Share of low-skilled immigrants at:	
	t	$t-1$
Shift-share instrument at t	1.475***	1.223***
	(0.132)	(0.169)
Shift-share instrument at $t-1$	-0.660***	0.076
	(0.172)	(0.203)
First-stage F-statistic	62.91	61.1
Panel B: OLS and 2SLS		
	(3)	(4)
	Republican vote share (Presidential elections)	
	OLS	2SLS
Share of low-skilled immigrants at t	1.139***	2.980***
	(0.225)	(1.096)
Share of low-skilled immigrants at $t-1$	0.159	-0.111
	(0.225)	(0.965)
Election Year FE	X	X
Commuting Zone FE	X	X
Weighted by voting pop	X	X
Controls (baseline)	X	X
Observations	3610	3610
Anderson-Rubin test P-value		0.000

The table reports first stage (Panel A) OLS and 2SLS (Panel B) regressions of the low-skilled immigration and its lag on $t-1$ (previous election year) on the share of Republican votes in presidential elections. All specifications include commuting zone and election year fixed effects, baseline (1992) values of controls interacted with time dummies, and voting-age population weights fixed at baseline. Clustered standard errors at the commuting zone level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A4: OLS and 2SLS results - effect of high and low-skilled immigration on voting

	Republican vote share (Presidential elections)			
	(1)	(2)	(3)	(4)
Panel A: OLS				
Share of low-skilled immigrants	1.363***	1.188***	1.179***	1.246***
	(0.294)	(0.227)	(0.249)	(0.223)
Share of high-skilled immigrants	-0.998***	-0.523***	-0.764***	-0.537***
	(0.134)	(0.163)	(0.156)	(0.159)
R-squared	0.924	0.953	0.952	0.969
Panel B: 2SLS				
Share of low-skilled immigrants	3.030***	1.292*	2.679***	2.942***
	(0.859)	(0.738)	(0.588)	(0.556)
Share of high-skilled immigrants	-1.087***	-0.786*	-0.720	0.586
	(0.249)	(0.477)	(0.794)	(1.260)
R-squared	0.381	0.649	0.266	0.461
Election Year FE	X	X	X	X
Commuting Zone FE	X	X	X	X
Weighted by voting pop	X	X	X	X
Controls (yearly)	X		X	
Controls (baseline)		X		X
Observations	5,054	5,054	3,610	3,610

The table reports OLS and 2SLS regressions of low-skilled and high-skilled immigration on the share of Republican votes in presidential elections. The instruments are the shift-shares for high and low-skilled immigration. Columns (1) and (3) includes the set of controls with their yearly values for each commuting zone, while columns (2) and (4) uses their baseline (1992) values interacted flexibly with time dummies. We restricted the panel to 2000 to 2016 in columns (3) and (4) in order to have comparable samples with specifications containing lags. Column (1) of Panel A is basically the same specification as Column (2) of Table 2 of Mayda et al. (2022), except that in our case the unit of analysis is the commuting zone. Clustered standard errors at the commuting zone level are shown in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A5: Effects of low-skilled immigration on labor market and public finance outcomes: lagged immigration on $t-1$ instead of $t-2$

Panel A: Labor Market Outcomes						
	Native unemployment			Native wages		
	All	Low-skill	High-sk.	All	Low-skill	High-sk.
	(1)	(2)	(3)	(4)	(5)	(6)
Share of low-skilled immigrants at t	0.106	2.014***	-0.078	-0.166	-2.302	-0.031
	(0.223)	(0.760)	(0.176)	(1.554)	(3.840)	(1.527)
Share of low-skilled immigrants at $t-1$	-0.234	-1.414	-0.146	3.302	5.607	3.214
	(0.270)	(0.974)	(0.214)	(2.169)	(4.620)	(2.100)
Panel B: Local Public Finance Outcomes						
	Revenue			Expenditure		
	Total	Taxes	Transfers	Total	Education	Welfare
	(7)	(8)	(9)	(10)	(11)	(12)
Share of low-skilled immigrants at t	-5.662	0.456	-3.272	-3.641	-0.629	3.173***
	(5.013)	(3.886)	(3.356)	(5.262)	(3.023)	(0.978)
Share of low-skilled immigrants at $t-1$	6.571	-1.776	2.233	3.281	3.648	-2.654***
	(4.724)	(3.516)	(4.464)	(5.007)	(4.205)	(0.962)
Observations	3610	3610	3610	3610	3610	3610

The table reports 2SLS regressions of the low-skilled immigration and its lag on $t-1$ (previous election year) on labor market (native unemployment rate and native log average hourly wages, overall and separated by skill level) and public finance outcomes (total revenue, taxes, transfers, total expenditure, education and welfare spending, all in per capita terms). All specifications include commuting zone and election year fixed effects, baseline (1992) values of controls interacted with time dummies, and voting-age population weights fixed at baseline. Clustered standard errors at the commuting zone level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Dynamic heterogeneous effects of low-skilled immigration on voting

	(1)	(2)	(3)	(4)	(5)
	Republican vote share (Pres. elections)				
Sh. low-skilled immig. t	3.107***	1.623**	2.580***	2.251***	0.678
	(0.592)	(0.751)	(0.468)	(0.406)	(1.159)
Sh. low-skilled immig. $t-2$	0.087	0.931	0.157	0.012	0.269
	(0.602)	(0.639)	(0.422)	(0.549)	0.269
Sh. low-skilled immig. t		4.317**			
$\times$ low-to-high-skilled ratio		(1.758)			
Sh. low-skilled immig. $t-2$		-2.963**			
$\times$ low-to-high-skilled ratio		(1.500)			
Sh. low-skilled immig. t			2.029		
$\times$ share of rural citizens			(2.814)		
Sh. low-skilled immig. $t-2$			0.343		
$\times$ share of rural citizens			(1.085)		
Sh. low-skilled immig. t				11.589**	
$\times$ govt. exp. / income ratio				(5.306)	
Sh. low-skilled immig. $t-2$				0.147	
$\times$ govt. exp. / income ratio				(2.095)	
Sh. low-skilled immig. t					7.935**
$\times$ share low-skill whites					(3.443)
Sh. low-skilled immig. $t-2$					-1.260
$\times$ share low-skill whites					(5.372)
Observations	3,610	3,610	3,610	3,610	3,610

The table reports dynamic shift-share 2SLS estimates of the Republican vote share in Presidential elections on the low-skilled immigration and its lag (2 election years before), together with interaction terms with heterogeneity variables (all measured at 1980 levels), all instrumented by the predicted low-skilled immigration, its lag and the respective interactions. All specifications have commuting zone and election year fixed effects, baseline controls interacted with time dummies and are weighted by the voting-age population. Clustered standard errors at the commuting zone level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A7: Dynamic heterogeneous effects of low-skilled immigration on unemployment

	(1)	(2)	(3)	(4)	(5)
	Unemployment rate (low-skilled natives)				
Sh. low-skilled immig. t	1.231***	0.054	0.438	0.756**	-1.942*
	(0.310)	(0.564)	(0.660)	(0.383)	(1.118)
Sh. low-skilled immig. $t-2$	-0.938*	-0.590	-1.104***	-0.739	0.668
	(0.483)	(0.552)	(0.424)	(0.494)	(1.637)
Sh. low-skilled immig. t		3.347**			
$\times$ low-to-high-skilled ratio		(1.445)			
Sh. low-skilled immig. $t-2$		-1.275			
$\times$ low-to-high-skilled ratio		(1.207)			
Sh. low-skilled immig. t			2.782		
$\times$ share of rural citizens			(1.923)		
Sh. low-skilled immig. $t-2$			1.613		
$\times$ share of rural citizens			(1.060)		
Sh. low-skilled immig. t				8.485**	
$\times$ govt. exp. / income ratio				(3.771)	
Sh. low-skilled immig. $t-2$				-2.520	
$\times$ govt. exp. / income ratio				(2.297)	
Sh. low-skilled immig. t					10.636***
$\times$ share low-skill whites					(3.861)
Sh. low-skilled immig. $t-2$					-6.624
$\times$ share low-skill whites					(5.656)
Observations	3,610	3,610	3,610	3,610	3,610

The table reports dynamic shift-share 2SLS estimates of unemployment rate among low-skilled natives on the low-skilled immigration and its lag (2 election years before), together with interaction terms with heterogeneity variables (all measured at 1980 levels), all instrumented by the predicted low-skilled immigration, its lag and the respective interactions. All specifications have commuting zone and election year fixed effects, baseline controls interacted with time dummies and are weighted by the voting-age population. Clustered standard errors at the commuting zone level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A8: Dynamic heterogeneous effects of low-skilled immigration on welfare

	(1)	(2)	(3)	(4)	(5)
	Local welfare expenditure (per capita)				
Sh. low-skilled immig t	1.762**	1.836	0.222	2.539***	3.812**
	(0.793)	(1.202)	(0.716)	(0.730)	(1.665)
Sh. low-skilled immig $t-2$	-1.756***	-1.104	-1.177*	-1.804**	-0.939
	(0.571)	(0.925)	(0.633)	(0.733)	(1.660)
Sh. low-skilled immig t		-0.052			
$\times$ low-to-high-skilled ratio		(2.548)			
Sh. low-skilled immig $t-2$		-2.169			
$\times$ low-to-high-skilled ratio		(2.063)			
Sh. low-skilled immig t			6.291**		
$\times$ share of rural citizens			(3.071)		
Sh. low-skilled immig $t-2$			-0.512		
$\times$ share of rural citizens			(1.548)		
Sh. low-skilled immig t				-11.511	
$\times$ ratio of govt. exp. to income				(7.070)	
Sh. low-skilled immig $t-2$				1.124	
$\times$ ratio of govt. exp. to income				(2.435)	
Sh. low-skilled immig. t					-6.502
$\times$ share low-skill whites					(4.263)
Sh. low-skilled immig. $t-2$					-2.469
$\times$ share low-skill whites					(4.766)
Observations	3,610	3,610	3,610	3,610	3,610

The table reports dynamic shift-share 2SLS estimates of the local welfare expenditure per capita on the low-skilled immigration and its lag (2 election years before), together with interaction terms with heterogeneity variables (all measured at 1980 levels), all instrumented by the predicted low-skilled immigration, its lag and the respective interactions. All specifications have commuting zone and election year fixed effects, baseline controls interacted with time dummies and are weighted by the voting-age population. Clustered standard errors at the commuting zone level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A9: Mediation analysis results for labor market mediators

Mediator	Treatment	Coefficients						First Stage F-stat	
		DE (β_Y^T)	β_M^T	β_Y^M	IE ($\beta_M^T \cdot \beta_Y^M$)	TE	IE(% of TE)	Eq. 9	Eq. 11
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total native unemployment rate	Share of low-skilled immigrants at t	1.708*** (0.326)	0.038 (0.103)	4.040** (1.863)	0.152 (0.424)	3.224*** (0.669)	4.7	66.72	2.97
Total native unemployment rate	Share of low-skilled immigrants at $t-2$	-0.443 (0.338)	-0.213 (0.136)	4.040** (1.863)	-0.860 (0.678)	-0.426 (0.588)	201.9	77.91	2.97
High-skilled native unemployment rate	Share of low-skilled immigrants at t	1.399*** (0.270)	-0.101 (0.083)	1.074 (1.359)	-0.109 (0.164)	3.224*** (0.669)	-3.4	66.72	2.01
High-skilled native unemployment rate	Share of low-skilled immigrants at $t-2$	-0.298 (0.218)	-0.156 (0.104)	1.074 (1.359)	-0.168 (0.240)	-0.426 (0.588)	39.4	77.91	2.01
Total native log hourly wages	Share of low-skilled immigrants at t	0.378 (0.402)	1.912*** (0.656)	0.487 (0.303)	0.932 (0.662)	3.224*** (0.669)	28.9	66.72	2.83
Total native log hourly wages	Share of low-skilled immigrants at $t-2$	-0.406 (0.291)	1.705 (1.345)	0.487 (0.303)	0.831 (0.835)	-0.426 (0.588)	-195.1	77.91	2.83
Low-skilled native log hourly wages	Share of low-skilled immigrants at t	-1.991 (9.450)	2.042 (2.508)	2.560 (6.424)	5.230 (14.608)	3.224*** (0.669)	162.2	66.72	0.10
Low-skilled native log hourly wages	Share of low-skilled immigrants at $t-2$	-5.521 (12.351)	1.938 (2.945)	2.560 (6.424)	4.962 (14.555)	-0.426 (0.588)	-1164.8	77.91	0.10
High-skilled native log hourly wages	Share of low-skilled immigrants at t	0.243 (0.407)	1.975*** (0.652)	0.581* (0.319)	1.148 (0.735)	3.224*** (0.669)	35.6	66.72	3.28
High-skilled native log hourly wages	Share of low-skilled immigrants at $t-2$	-0.406 (0.338)	1.679 (1.291)	0.581* (0.319)	0.976 (0.922)	-0.426 (0.588)	-229.1	77.91	3.28
Observations		3610							

This table is an extension of Table 6 for the other possible labor market mediators shown before. All regressions have controls, commuting zone and election year fixed effects and are weighted by the voting-age population. Clustered standard errors at the commuting zone level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A10: Mediation analysis results for other local public finance mediators

Mediator	Treatment	Coefficients						First Stage F-stat	
		DE (β_Y^T)	β_M^T	β_Y^M	IE ($\beta_M^T \cdot \beta_Y^M$)	TE	IE(% of TE)	Eq. 9	Eq. 11
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total revenue per capita	Share of low-skilled immigrants at t	1.400*** (0.450)	-4.031 (3.548)	-0.199* (0.116)	0.803 (0.847)	3.224*** (0.669)	24.9	66.72	2.18
Total revenue per capita	Share of low-skilled immigrants at $t-2$	-0.229 (0.731)	6.330** (3.171)	-0.199* (0.116)	-1.260 (0.968)	-0.426 (0.588)	295.8	77.91	2.18
Tax revenue per capita	Share of low-skilled immigrants at t	1.950* (1.007)	-0.520 (2.701)	-0.778* (0.397)	0.404 (2.110)	3.224*** (0.669)	12.5	66.72	2.25
Tax revenue per capita	Share of low-skilled immigrants at $t-2$	-0.057 (1.511)	-1.082 (2.252)	-0.778* (0.397)	0.842 (1.803)	-0.426 (0.588)	-197.7	77.91	2.25
Transfers revenue per capita	Share of low-skilled immigrants at t	1.060 (0.980)	-2.645* (1.544)	-0.730 (0.459)	1.930 (1.655)	3.224*** (0.669)	59.9	66.72	1.76
Transfers revenue per capita	Share of low-skilled immigrants at $t-2$	0.172 (1.258)	2.067 (2.640)	-0.730 (0.459)	-1.508 (2.147)	-0.426 (0.588)	354.0	77.91	1.76
Total expenditure per capita	Share of low-skilled immigrants at t	1.620** (0.648)	-2.984 (3.635)	-0.263 (0.172)	0.786 (1.086)	3.224*** (0.669)	24.4	66.72	1.42
Total expenditure per capita	Share of low-skilled immigrants at $t-2$	-0.654 (0.821)	3.344 (3.186)	-0.263 (0.172)	-0.881 (1.017)	-0.426 (0.588)	206.8	77.91	1.42
Education expenditure per capita	Share of low-skilled immigrants at t	2.455** (1.111)	1.193 (1.616)	-0.539 (0.619)	-0.643 (1.142)	3.224*** (0.669)	-19.9	66.72	0.56
Education expenditure per capita	Share of low-skilled immigrants at $t-2$	0.051 (0.941)	2.439 (2.623)	-0.539 (0.619)	-1.315 (2.069)	-0.426 (0.588)	308.7	77.91	0.56
Observations		3610							

This table is an extension of Table 6 for the other possible local public finance mediators shown before. All regressions have controls, commuting zone and election year fixed effects and are weighted by the voting-age population. Clustered standard errors at the commuting zone level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A11: Mediation analysis results for other aggregation methods

	Coefficients						First Stage F-stat	
	DE (β_Y^T)	β_M^T	β_Y^M	IE ($\beta_M^T \cdot \beta_Y^M$)	TE	IE(% of TE)	Eq. 9	Eq. 11
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Composite index as mediator								
Share of low-skilled immigrants at t	0.937**	1.799***	1.060***	1.907***	3.224***	59.2	66.72	15.80
	(0.454)	(0.415)	(0.276)	(0.664)	(0.669)			
Share of low-skilled immigrants at $t-2$	0.006	-1.372**	1.060***	-1.454*	-0.426	341.3	77.91	15.80
	(0.319)	(0.617)	(0.276)	(0.756)	(0.588)			
Panel B: KLK index as mediator								
Share of low-skilled immigrants at t	0.839**	18.647***	0.101***	1.888***	3.224***	58.6	66.72	19.10
	(0.419)	(4.552)	(0.029)	(0.710)	(0.669)			
Share of low-skilled immigrants at $t-2$	0.038	-15.197***	0.101***	-1.539**	-0.426	361.3	77.91	19.10
	(0.261)	(5.570)	(0.029)	(0.716)	(0.588)			
Observations	3610							

The table reports a summary of the causal mediation estimates. The coefficients reported are based on equations 9 to 12. "DE" stands for "Direct Effect" (non-mediated), "IE" stands for "Indirect Effect" (mediated) and "TE" is the "Total Effect", the ones that we estimated in section 4.2. Since the model with the dynamic shift-share is over-identified for one mediator, the sum of the "DE" and the "IE" is not exactly the "TE". In columns (7) and (8), we report F-statistics for the two first stages presented in equations 9 and 11 respectively. The composite index is the average between each variable re-scaled to vary between 0 and 1 (subtracting the minimum and dividing by the maximum minus the minimum). The KLK (Kling et al., 2007) index is the average between variables z-scores, obtained by subtracting the mean and dividing by the standard deviation. All regression specifications have baseline controls interacted with time dummies, commuting zone and election year fixed effects and are weighted by the voting-age population. Clustered standard errors at the commuting zone level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

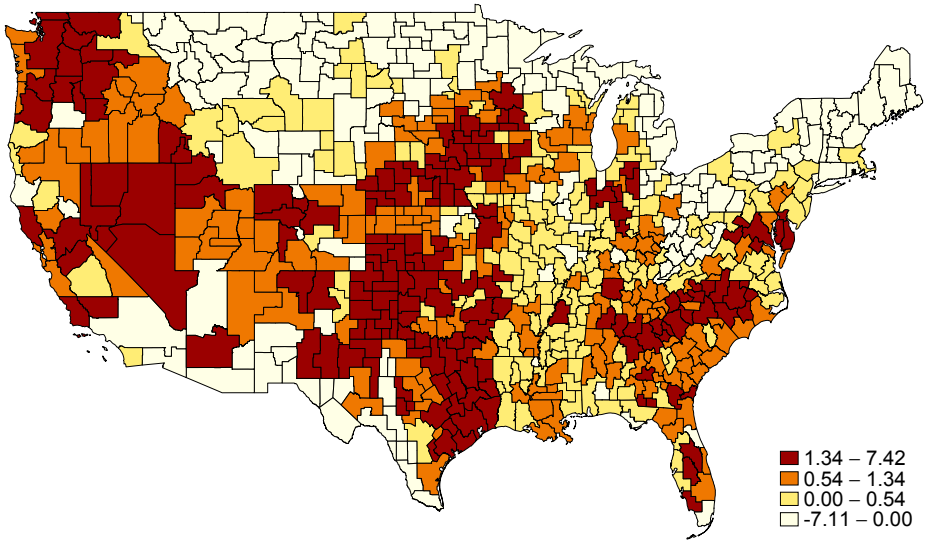
Table A12: Individual-level OLS regressions with ANES data - Identification as Republican and preferences for restrictive immigration policies over time

	(1)	(2)	(3)	(4)
	Party identification: Republican			
Anti-immigration	0.103	0.092	0.125***	0.107**
	(0.078)	(0.082)	(0.045)	(0.045)
Anti-immigration × 2004	0.001	-0.001	-0.097	-0.082
	(0.108)	(0.111)	(0.078)	(0.079)
Anti-immigration × 2008	-0.002	-0.007	-0.089	-0.084
	(0.097)	(0.100)	(0.068)	(0.068)
Anti-immigration × 2012	0.066	0.061	0.009	0.018
	(0.087)	(0.090)	(0.056)	(0.056)
Anti-immigration × 2016	0.151*	0.153*	0.175***	0.183***
	(0.086)	(0.090)	(0.058)	(0.058)
Hispanic		-0.069**		-0.141***
		(0.031)		(0.031)
First-generation		-0.077**		-0.029
		(0.031)		(0.028)
Survey year FE	X	X	X	X
Cohort	1975-1990	1975-1990	1959-1974	1959-1974
Observations	3,125	3,105	3,889	3,862
R-squared	0.040	0.050	0.036	0.046

The table reports OLS regressions at the individual-level using the American National Election Studies (ANES) pooled cross-section data. The dependent variable is an indicator equal to 1 if the respondent identifies as Republican or strong Republican. The main regressor is an indicator if the respondent answered that the number of admitted immigrants in the U.S. should decrease. First generation is defined as those with foreign-born parents. We restricted the sample to presidential election years between 2000 and 2016 and to the cohort born between 1975 and 1990 in columns 1 and 2 and to the cohort born between 1959 and 1974 in columns 3 and 4. Robust standard errors reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

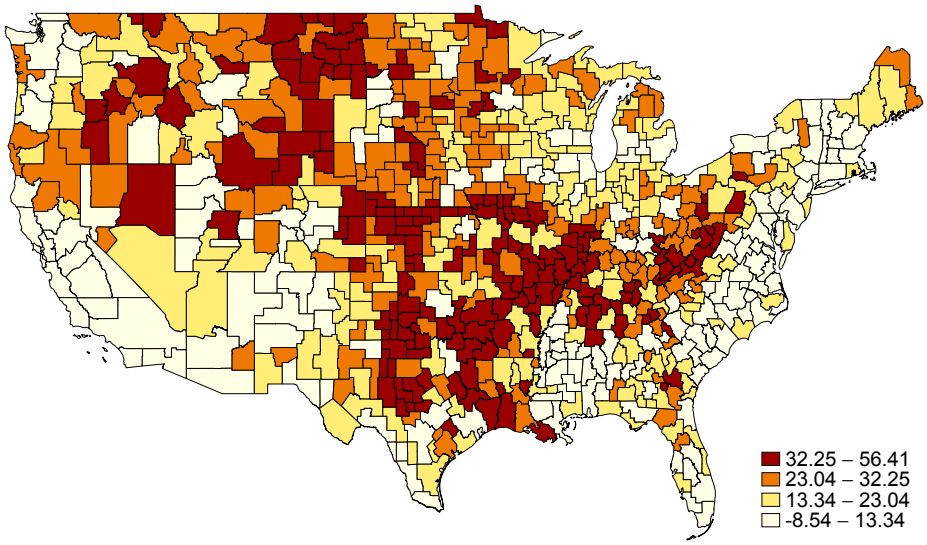
Figures

Figure A1: 1992-2016 change in the low-skilled immigrant share of the population in U.S. commuting zones



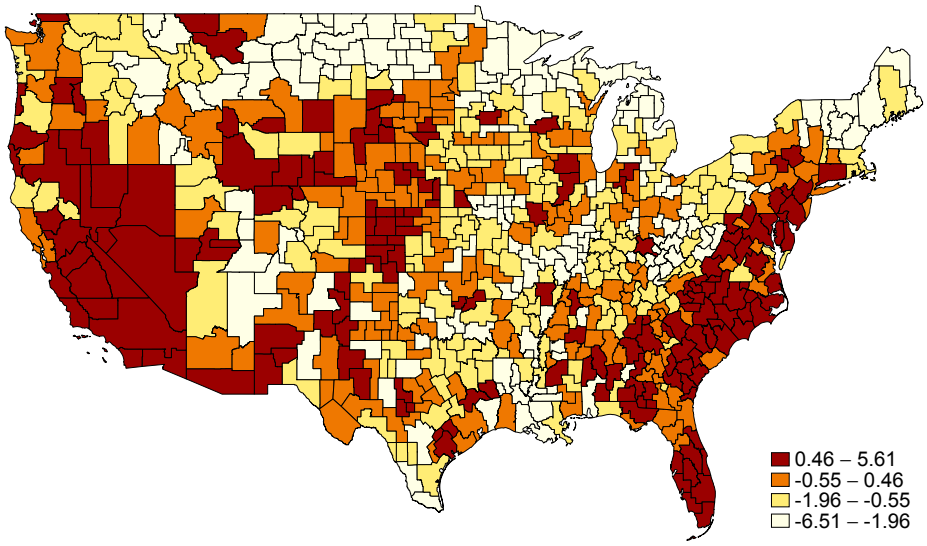
Notes: The legend refers to quartiles of the distribution of the low-skilled immigrant share of the population. We did not plot data for Alaska and Hawaii because these states were not used in our panel.

Figure A2: 1992-2016 change in the presidential elections Republican vote share in U.S. commuting zones



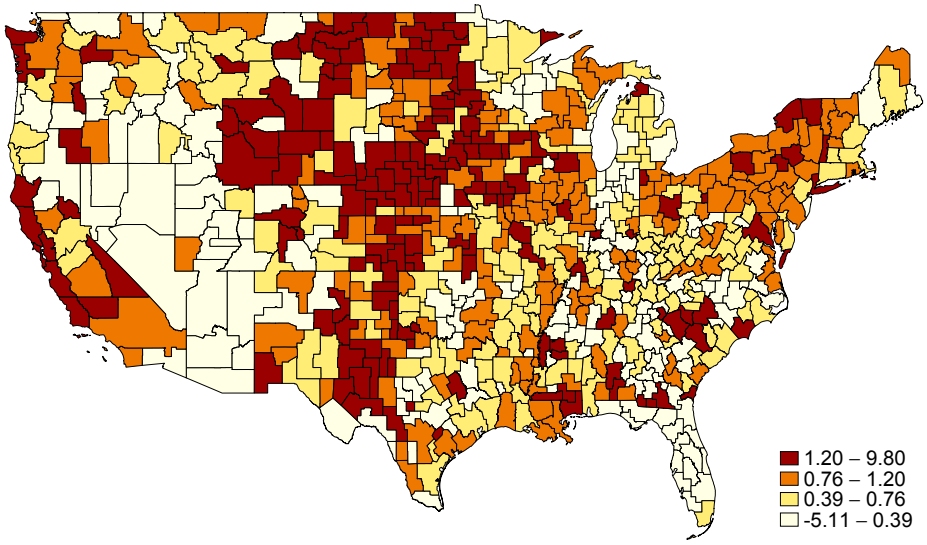
Notes: The legend refers to quartiles of the distribution of the presidential elections Republican vote share. We did not plot data for Alaska and Hawaii because these states were not used in our panel.

Figure A3: 1992-2016 change in the native unemployment rate in U.S. commuting zones



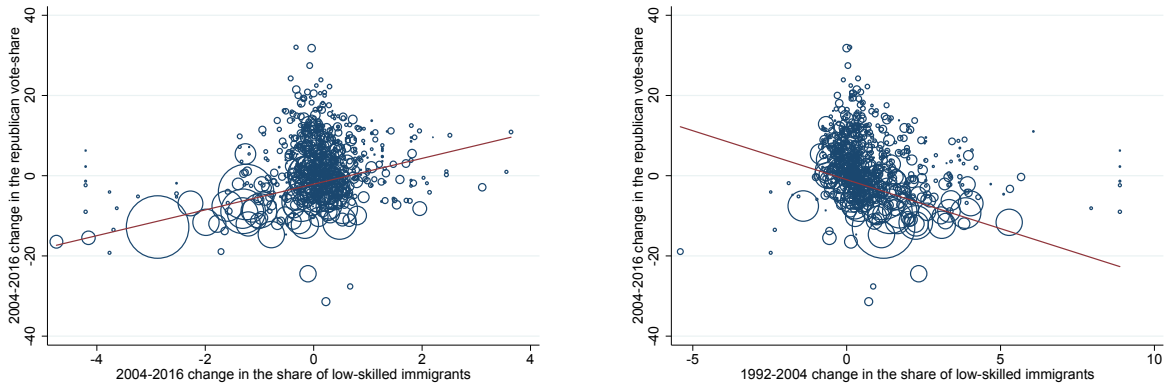
Notes: The legend refers to quartiles of the distribution of the native unemployment rate. We did not plot data for Alaska and Hawaii because these states were not used in our panel.

Figure A4: 1992-2016 change in the total local public expenditure per capita in U.S. commuting zones



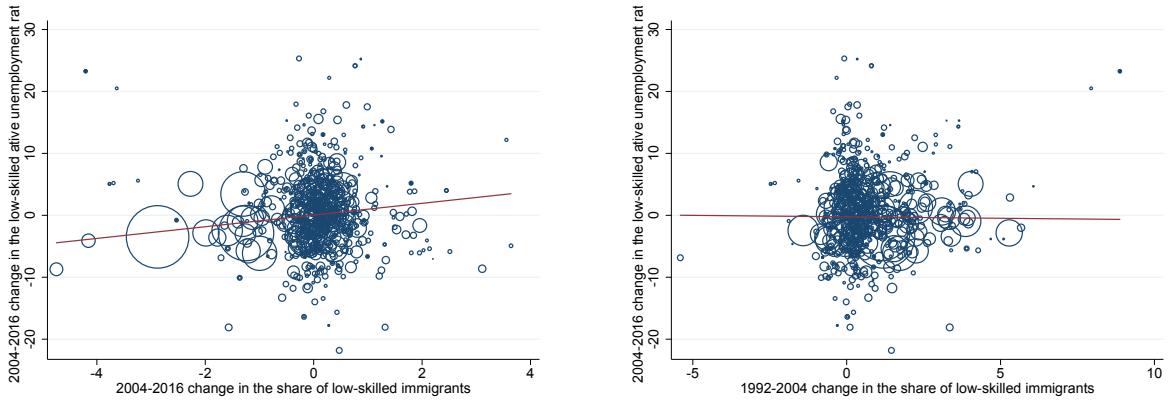
Notes: The legend refers to quartiles of the distribution of the total local public expenditure per capita. We did not plot data for Alaska and Hawaii because these states were not used in our panel.

Figure A5: Population-weighted 2004-2016 changes in the Republican vote share in presidential elections and in the low-skilled immigrant share of the population (2004-2016 and 1992-2014) in U.S. commuting zones



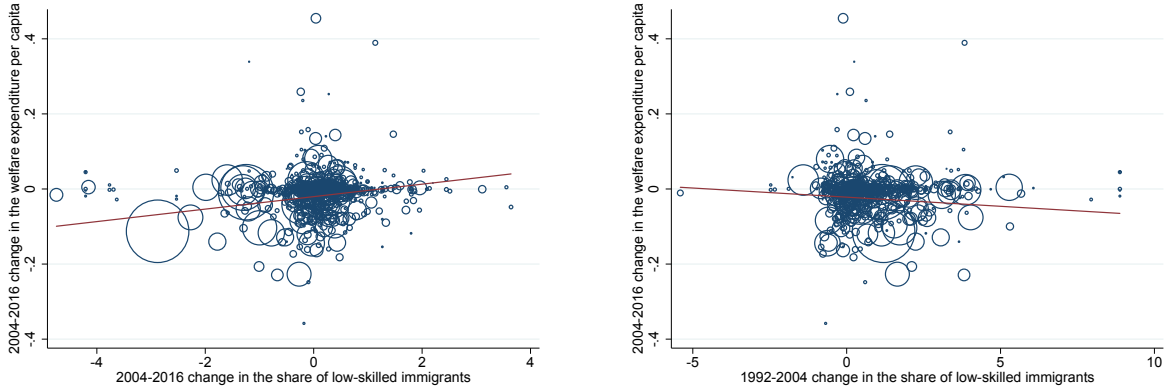
Notes: Each circle reflects the weighted average of the two variables, considering the 1992 voting-age population in each commuting zone as weights. The line refers to the slope coefficient of a simple linear regression between the two variables, also weighted by the voting-age population. The figure in the left capture the contemporaneous relation between the two variables, while the figure in the right shows the relation between the y-axis variable and the change in low-skilled immigration in a previous period, thus reflecting a longer-term relationship.

Figure A6: Population-weighted 2004-2016 changes in the unemployment rate among low-skilled natives and in the low-skilled immigrant share of the population (2004-2016 and 1992-2014) in U.S. commuting zones



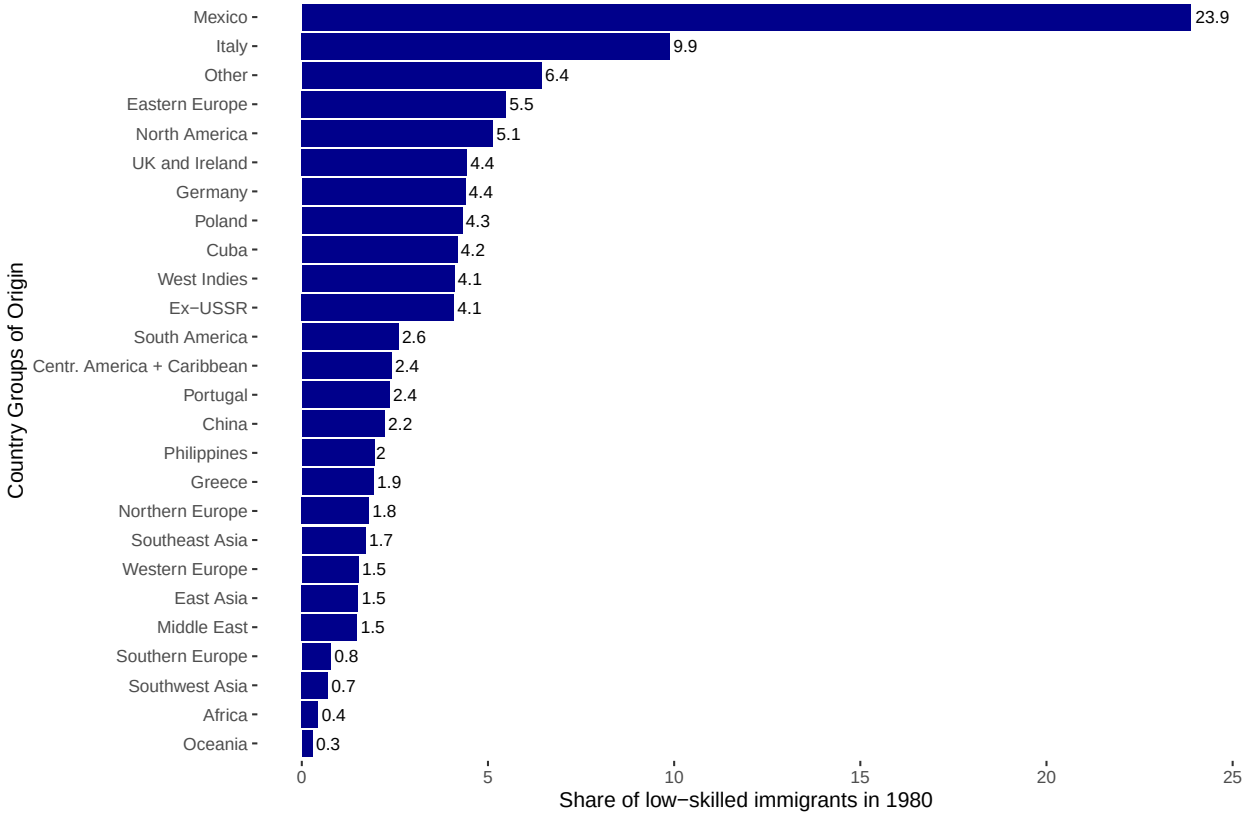
Notes: Each circle reflects the weighted average of the two variables, considering the 1992 voting-age population in each commuting zone as weights. The line refers to the slope coefficient of a simple linear regression between the two variables, also weighted by the voting-age population. The figure in the left capture the contemporaneous relation between the two variables, while the figure in the right shows the relation between the y-axis variable and the change in low-skilled immigration in a previous period, thus reflecting a longer-term relationship.

Figure A7: Population-weighted 2004-2016 changes in the local expenditure in Welfare per capita and in the low-skilled immigrant share of the population (2004-2016 and 1992-2014) in U.S. commuting zones



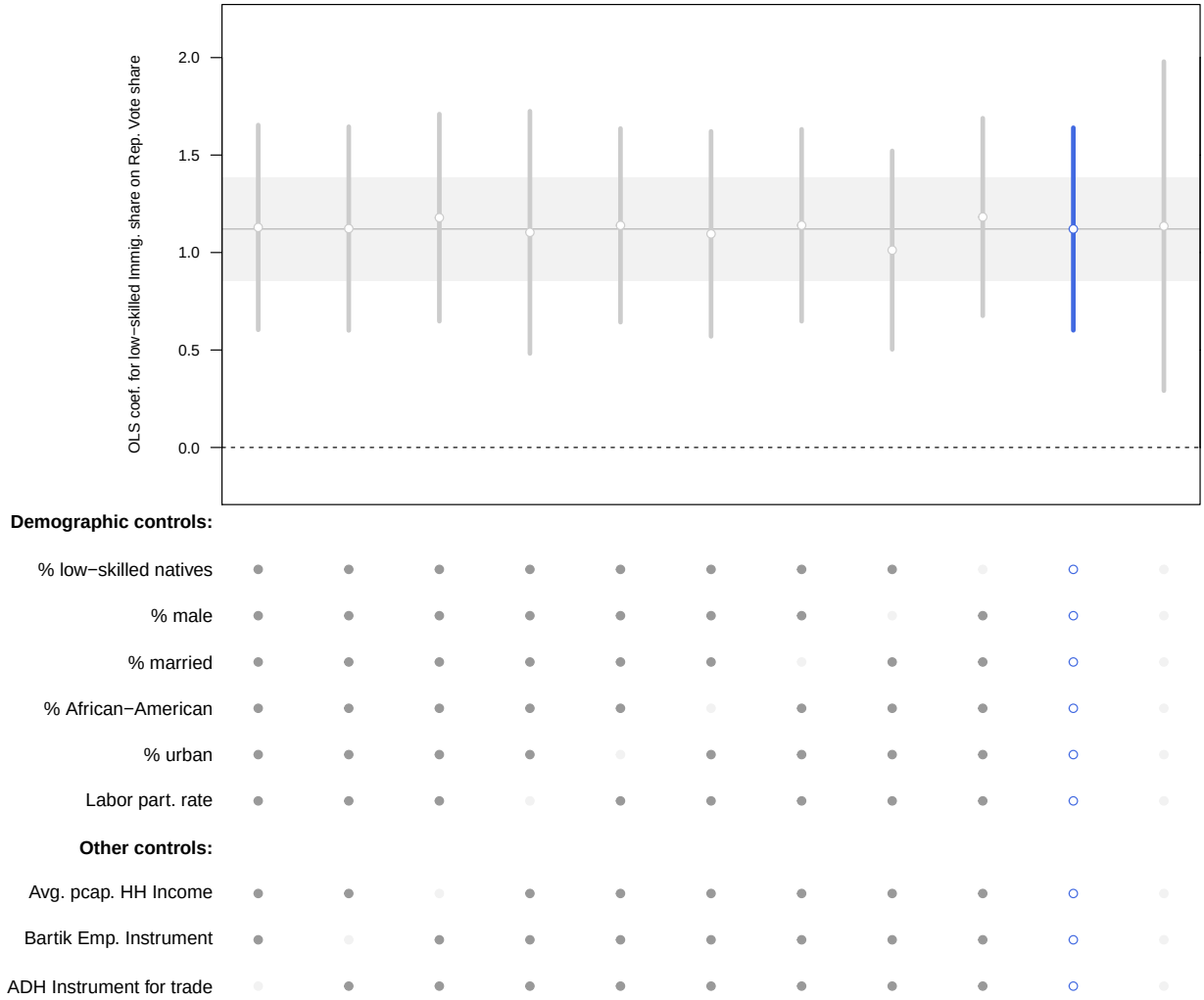
Notes: Each circle reflects the weighted average of the two variables, considering the 1992 voting-age population in each commuting zone as weights. The line refers to the slope coefficient of a simple linear regression between the two variables, also weighted by the voting-age population. The figure in the left capture the contemporaneous relation between the two variables, while the figure in the right shows the relation between the y-axis variable and the change in low-skilled immigration in a previous period, thus reflecting a longer-term relationship. Two commuting zones with outlier values for the change in welfare expenditure per capita were removed for better visualization.

Figure A8: Low-skilled immigrant shares in 1980 by country groups of origin



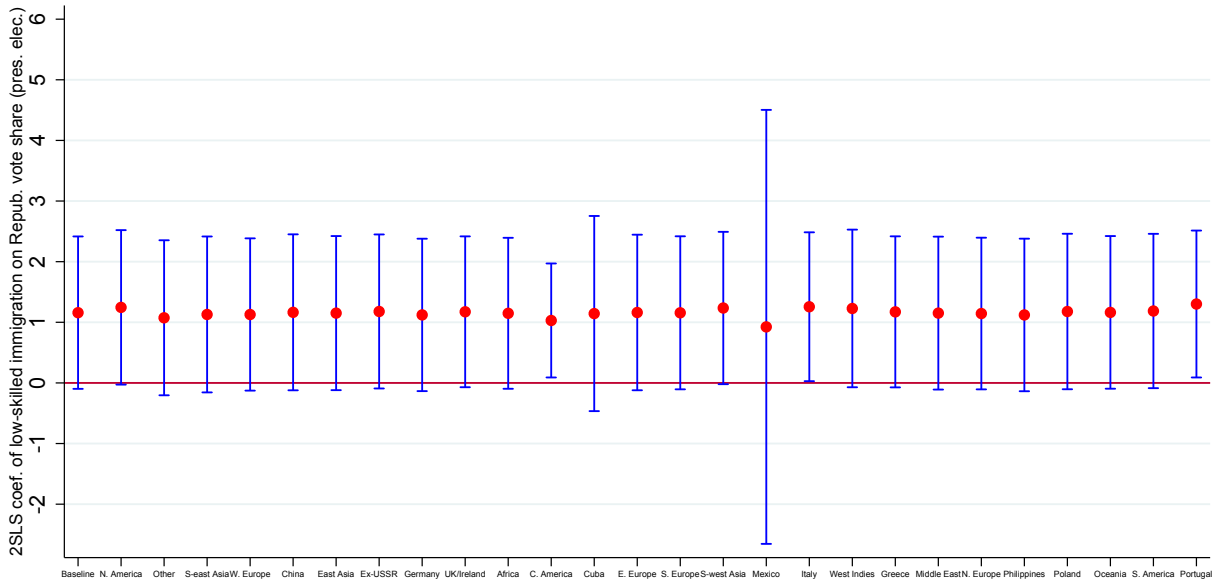
Notes: The groups are composed by: North America (Canada and small islands), South America (all countries), Central America and the Caribbean (all countries except Mexico and the West Indies), United Kingdom and Ireland, Central/Eastern Europe (Austria, Hungary, Czech Republic, Romania, Bulgaria and ex-Yugoslavia countries), Northern Europe (Sweden, Norway, Denmark, Finland and Iceland), Southern Europe (Spain, Malta, Albania, Andorra and Gibraltar), Western Europe (Netherlands, France, Switzerland, Belgium, Luxembourg, Liechtenstein and Monaco), Ex-USSR (Russia, Baltic states and other ex-USSR countries), Southeast Asia (Vietnam, Laos, Thailand, Cambodia, Indonesia, Malaysia, Singapore and Brunei), East Asia (Japan, South and North Korea), Southwest Asia (India, Iran, Afghanistan, Maldives and Nepal), Middle East (Turkey, Lebanon, Israel/Palestine, Iraq, Syria, Jordan, Cyprus, Saudi Arabia, Yemen, Kuwait, Qatar, United Arab Emirates, Bahrain and Oman), Africa (all countries), Oceania (all countries) and Other (non-specified countries of all continents and "unknown or at sea"). Countries with large shares are not grouped.

Figure A9: Specification chart for the OLS estimate of impact of low-skilled immigration on the Republican vote share



Notes: Each estimated coefficient leaves one of the controls out. All specifications have election year and commuting zone fixed effects and are weighted by the voting-age population. Standard errors are clustered at the commuting zone level.

Figure A10: 2SLS estimates of low-skilled immigration on Republican vote shares in presidential elections: leaving each country of origin out of the instrument



Notes: Each estimated coefficient leaves one of the countries of origin out in preparing the instrumental variable. All specifications have election year, commuting zone fixed effects, baseline controls interacted with time dummies and are weighted by the voting-age population. Standard errors are clustered at the commuting zone level.