The Impacts of Unauthorized Immigration on U.S. Labor and Housing Markets: New Evidence from Administrative Microdata

Daniel J. Wilson and Xiaoqing Zhou

Working Paper 2607 **March 2026**

Research Department

https://doi.org/10.24149/wp2607

The Impacts of Unauthorized Immigration on U.S. Labor and Housing Markets: New Evidence from Administrative Microdata*

Daniel J. Wilson† and Xiaoqing Zhou‡

March 6, 2026

Abstract

From early 2021 to early 2024, the U.S. experienced an unprecedented boom in unauthorized immigration, followed by a rapid slowdown beginning in mid-2024. We provide the first systematic empirical assessment of the labor- and housing-market effects of this episode. Using newly available administrative microdata on individual immigrants, we construct measures of net unauthorized immigration at the national and local levels and exploit plausibly exogenous variation across local markets. We find that unauthorized immigrant worker flows (UIWF) increased local employment approximately one-for-one, without significant declines in local wages. These inflows also raised local house prices and rents without expanding housing supply, consistent with a housing demand shock in the face of short-run inelastic supply. Lastly, we find that UIWF reduced labor income per capita, consistent with downward wage composition of the local workforce, and strongly reduced government transfers. These findings should help inform policy debates surrounding how unauthorized immigrant labor supply impacts local labor and housing markets as well as public finances.

Keywords: Immigration, Labor Market, Housing Market, Unauthorized, Post-pandemic

JEL Codes: E24, H53, J11, J21, J22, J23, J31, J61, R31

---

* The views expressed in this paper are solely those of the authors and do not necessarily reflect the views of the Federal Reserve Bank of San Francisco, the Federal Reserve Bank of Dallas or the Federal Reserve System.

† Daniel J. Wilson, Federal Reserve Bank of San Francisco, daniel.wilson@sf.frb.org.

‡ Xiaoqing Zhou, Federal Reserve Bank of Dallas, Xiaoqing.Zhou@dal.frb.org.

1 Introduction

The large increase in unauthorized immigration into the U.S. from early 2021 to early 2024, and its subsequent rapid decline, have been unprecedented in modern times. According to the U.S. Congressional Budget Office (Congressional Budget Office, 2026), net entry of this category of immigrants added roughly 7 million people to the U.S. population over 2021 to 2024 (1.75 million per year), nearly double that of legal immigration.[1] To put this growth in perspective, net unauthorized immigration—that is, immigration of individuals who entered the country without being formally admitted for purposes of immigration law—averaged only 0.1 million a year from 2000 to 2019 and was slightly negative from 2010 to 2019.

By mid-2024, net unauthorized immigration had begun falling rapidly. Based on the newly compiled measures we construct in this paper from individual-level immigration court data combined with other administrative data, monthly net unauthorized immigration peaked in January 2024 and then fell steeply, becoming negative by February 2025 (see Figure 2). In addition to these large national swings, there was tremendous variation across local economies in their exposure to unauthorized immigration (Wilson and Zhou, 2026).

These developments have prompted considerable economic and public policy debate about the impacts of unauthorized immigration, with particular focus on labor and housing markets. To date, existing studies have (i) largely relied on aggregate data to measure net unauthorized immigration after 2020, and (ii) inferred its economic effects either from pre-pandemic estimates or through model-based simulations. We provide the first systematic empirical assessment of the local labor- and housing-market effects of unauthorized immigration, using newly available administrative microdata and a rigorous cross-sectional research design that exploits exogenous variation across local markets. We find that unauthorized immigrant worker flows increased local employment approximately one-for-one, without generating significant declines in local wages during the early 2021 to early 2024 boom period. At the same time, these inflows raised local house prices and rents, with little evidence of increases in housing supply. Thus, while the influx of these immigrant workers acted as a positive supply shock to local labor markets, it acted simultaneously as a demand shock to local housing markets, boosting rents given relatively inelastic short-run housing supply.

Standard theoretical frameworks yield limited and ambiguous predictions about the impacts of unauthorized immigration on local labor and housing markets. Models focusing on

[1] More generally, net immigration of lawful permanent residents averaged 0.8 million per year since 2000, never exceeding 900,000 (Congressional Budget Office, 2026).

low-skilled immigration are the most relevant, given that unauthorized immigrants tend to have lower education and work in lower-skilled occupations (e.g., Cheremukhin et al. 2025). An influx of low-skilled immigrants typically is modeled as, on net, an increase in low-skilled labor supply, dominating any corresponding increase in demand from these immigrants as consumers.[2] Such an increase in labor supply should theoretically increase immigrant employment, while crowding out employment of low-skilled native workers who are close substitutes. The effect on high-skilled employment, however, is ambiguous and depends on the degree of complementarity between high-skilled labor and low-skilled immigrant labor. As such, the impact on total employment is ambiguous, depending on group-specific employment shares and the degree of substitution or complementarity across groups. Likewise, predictions for wages are ambiguous, reflecting these same factors as well as the labor-demand elasticities faced by each group.

Standard theory also yields ambiguous predictions for the impact of unauthorized immigrants on housing markets. Immigration is typically modeled as a net increase in housing demand, which raises house prices and rents. However, two unique aspects of unauthorized immigration complicate the picture. First, evidence suggests unauthorized immigrants tend to live in higher-occupancy housing units, an intensive margin that can help offset upward pressure on rents (e.g., Brown et al. 2023). Second, as argued by Monras (2020), unauthorized immigrants may disproportionately work in the construction sector, potentially lowering construction costs and, eventually, increasing housing supply and putting downward pressure on rents.

Given these ambiguous theoretical predictions, many economists and policymakers have turned to existing empirical research on the local impacts of immigration for guidance. Unfortunately, while there is a rich literature, with pioneering studies dating back at least to Card (2001), providing careful estimates of the causal effects of immigration on local labor- and housing-markets, the prior literature has focused either explicitly on legal immigration or on total immigration (e.g., Saiz 2007). Such focus was natural, given that unauthorized immigration in the past was typically small compared to legal immigration (as discussed in Section 3). In addition, data used in past studies generally did not allow for credible means to distinguish between unauthorized and legal immigration.

Because the recent rise in total immigration was driven primarily by unauthorized immigration, the economic impacts on local markets may be quite different from those found in prior studies. For instance, it is unclear *a priori* whether unauthorized immigrant workers

[2]See Galaasen et al. (2025) for an analysis of the consumer demand-side impacts of legal immigration in the E.U.

are fully captured in official data measuring employment and wages. Moreover, such workers may have different spillover effects on incumbent workers than authorized immigrant workers, leading to different impacts on local areas' total employment and wages. The impacts of unauthorized immigration on house prices and rents could also differ from those found for authorized immigration given their unique features. Lastly, some local economic outcomes, such as government spending on welfare and other social safety programs, are of particular interest for debates surrounding unauthorized immigration and are largely unaddressed by the literature on legal or total immigration.

In this paper, we provide novel estimates of the economic effects of unauthorized immigration using variation across local labor market areas. We use restricted-use, individual-level immigration court data, combined with other administrative data, to construct monthly estimates of entries, exits, and net entry of working-age unauthorized immigrants by county, which we aggregate to commuting zones (CZs) and metropolitan statistical areas (MSAs). We also adjust these values using the employment rates of recent immigrants from the same origin countries in the American Community Survey (ACS) to measure unauthorized immigrant worker flows (UIWF)—the main regressor of interest in our analysis.

Because local economic activity and immigration flows may respond simultaneously to unobserved demand shocks, the standard approach in the literature is to construct a shift-share instrument that interacts national origin-specific inflows ("the shift") with the pre-existing distribution of immigrants from that origin across U.S. destinations ("the share"), aggregated across origins (Card 2001). This approach, however, is subject to concerns that persistent local demand shocks may induce sustained inflows from specific countries, creating a spurious correlation between current employment growth and past immigration used to predict current immigration inflows (Burchardi et al. 2019).

To address this concern, we implement a two-way leave-out shift-share instrument design building on Card (2001) and Burchardi et al. (2019). Our instrument interacts a leave-out shift—national inflow from origin o excluding destination d—with a leave-out share—the pre-existing distribution of immigrants from origins other than o across U.S. destinations. The first leave-out mitigates concerns that contemporaneous local economic conditions generate endogenous pull factors large enough to influence national inflows from any single origin. The second leave-out limits the influence of origin-specific settlement patterns that could themselves reflect persistent local economic conditions. We show that this empirical design passes validation tests recommended in the recent microeconometrics literature (e.g., Goldsmith-Pinkham et al., 2020) and delivers substantially stronger first-stage relevance than alternative identification strategies.

Using this instrumental-variable (IV) approach, we estimate the causal effects of unauthorized immigrant worker flows on a range of local economic outcomes, exploiting cross-sectional variation. We focus primarily on the immigration "boom" period from March 2021 to March 2024, with observations at the CZ or MSA level, depending on the outcomes. When data allows, we also estimate effects separately for the subsequent "slowdown" period from June 2024 to June 2025.

We start by examining the impacts of UIWF on local labor market outcomes, specifically employment and wages. We utilize the comprehensive, high-quality data on monthly employment and quarterly average weekly wages from the Quarterly Census of Employment and Wages (QCEW). These data are based on state unemployment insurance administrative records and provide the benchmarks for the national monthly payroll employment statistics from the Bureau of Labor Statistics (BLS).

We find that an increase in UIWF equal to 1% of initial employment increases local employment by 0.96%—a nearly one-for-one effect and highly statistically significant. The first-stage regression underlying this result yields an F-statistic near 30, well above standard critical values associated with weak instrument bias. Following the recommendations of Goldsmith-Pinkham et al. (2020), we compute the Rotemberg weights and the just-identified IV estimates associated with each country's immigration share component. We find that no single country has a dominant contribution to our baseline IV estimate, that the just-identified estimates are all close to the baseline IV estimate of approximately one, and that over-identification tests fail to reject the null of instrument exogeneity. Furthermore, placebo tests do not point to any notable pre-trend concerns. Using a back-of-the-envelope calculation, we show that UIWF explains roughly 30% of total employment growth over March 2021 to March 2024 in the average CZ.

The estimated effect of UIWF on average weekly wages is roughly -1, but is imprecisely estimated. Thus, we cannot reject the hypothesis that local wages were unaffected by UIWF. Examining industry-specific effects, we find that UIWF had a particularly large impact on employment in the Leisure & Hospitality industry during the boom period. We find no significant effects on industry-specific wages. We also extend the labor market analysis to the immigration slowdown period of mid-2024 to mid-2025. While we find an IV point estimate for the employment effect around one, estimates are statistically insignificant. Industry-specific estimates for this period point to positive and weakly significant employment effects in Education & Health and in Manufacturing.

To supplement our employment and wage results for the boom period, we additionally analyze ACS data, which are commonly used in prior studies of immigration effects. While noting several concerns with ACS data for studying unauthorized immigration, especially in recent years, we find that the results are broadly consistent with those based on the QCEW data.

We then turn to the effects of unauthorized immigration on the broader local economy, focusing in particular on the housing market. We start by estimating the effects on house prices, rents, and new housing supply. First, we find that during the boom period an increase in unauthorized immigrant worker flows equal to 1% of a local area's initial employment increased local house prices by 2.2% and increased local rents by 1.4%. The impact on rents is slightly smaller for single-family units and slightly larger for multi-family units. These magnitudes are similar to those found by Saiz (2007) based on legal immigration over the 1985-1998 period. A back-of-the-envelope calculation suggests that UIWF can explain about 30% of the total growth in house prices and 20% of total growth in rents over the boom period for the average local market.

On the quantity side, our IV estimates of the housing supply effect, measured by new permits issued relative to existing housing units, point to small and statistically insignificant effects across all housing segments (i.e., single-family, multi-family and total). These results, together with the muted construction-sector employment effects we document with both QCEW and ACS data, suggest that during the boom period, UIWF acted primarily as a housing demand shock in the presence of relatively fixed short-run housing supply.

Lastly, we investigate the effects of UIWF on personal income and select subcomponents. Based on the IV estimates, we find a positive but insignificant effect on overall personal income. For the labor income component, we find a negative and significant effect of UIWF on per capita labor income growth, which is consistent with the expected negative effect on average wages from a labor supply shock and a compositional effect due to the fact that unauthorized immigrants typically earn lower wages than native-born workers.

Looking at the subcomponent of government transfers, the results point to significant negative effects of UIWF on both total local area government transfers and government transfers per capita. Specifically, we find that an increase in unauthorized immigrant worker flows equal to 1% of initial local employment leads to a reduction in government transfers of -4.5% and in government transfer per capita of -5%. These transfer-reducing effects of unauthorized immigration are consistent with the positive employment effects we find, which reduce eligibility and need for government safety net programs such as unemployment insurance, food assistance, and Medicaid. However, they stand in some contrast with survey results reported by Camarota and Zeigler (2026) pointing to higher participation in government welfare programs in 2024 by non-citizens compared with U.S.-born citizens. These survey data, however, only report on the extensive margin of program utilization, not the dollar amount of the government transfers. In addition, the data do not distinguish between legal and unauthorized immigrants, nor between immigrants heading households containing U.S. citizen children or others.

2 Literature

As noted above, our paper differs importantly from much of the prior literature in that we focus on *unauthorized* immigration rather than legal or total immigration. There are, however, important exceptions, and, more generally, it is useful to understand the methods, data, and findings of past studies examining legal and total immigration in order to place our results in context.

The modern literature on the causal impacts of immigration largely traces its roots to the seminal papers of Card (1990), Altonji and Card (1991), and Card (2001). These papers pioneered the idea of identifying exogenous local immigration supply shocks by exploiting the fact that new immigrants tend to settle in the same local labor markets as past immigrants from the same country. A few recent papers have refined this approach to mitigate remaining endogeneity concerns.[3] A related strand of the literature studies the mechanisms through which immigration affects labor markets, emphasizing imperfect substitution between immigrant and native workers and the role of task specialization (e.g., Peri and Sparber 2009; Ottaviano and Peri 2012). These studies typically focus on the impacts of immigration on native-born workers. This emphasis partly reflects public policy debates about whether immigrants crowd out native employment and partly reflects data considerations: unlike unauthorized immigrants, legal immigrants are generally well captured in standard data sources, making it reasonable to presume a one-for-one relationship between legal immigrant worker inflows and measured employment.

Theoretically, an exogenous increase in local labor supply due to immigration has ambiguous effects on total and native-born employment and wages. If immigrant workers are substitutable for native workers, the effects on native worker employment generally are negative, with the degree to which the negative impacts show up more on employment versus wages depending on the elasticity of native labor supply. For instance, Cravino et al. (2026) develop a dynamic spatial model with immigration, calibrated with estimates on unauthorized immigration derived from ACS data. Their model predicts that mass deportations, or decreases in the number of unauthorized immigrants, lead to modestly higher real wages

[3] See, for example, Jaeger et al. (2019), Burchardi et al. (2019), and Terry et al. (2026).

of native workers in the short-run, with substantial variation across regions and occupations. Over the medium to longer run, however, lower (higher) immigration leads to declines (increases) in investment and capital accumulation, reducing (increasing) capital-labor ratios and productivity and ultimately depressing (boosting) employment and wages (see also Burchardi et al. 2019).

The empirical literature on these impacts is mixed. Altonji and Card (1991) and Card (2001) used the immigration network IV approach and found small negative effects from immigration of low-skilled workers on wages of low-skilled natives and insignificant effects on employment. Dustmann et al. (2017) examined the short-run effects of an exogenous policy-induced immigration shock involving low-skilled Czech workers commuting to German localities along the Czech-German border in 1991. They found that this shock led to a modest decline in wages and a substantial decline in employment for native German workers. In contrast, Clemens et al. (2018) study the termination of the Bracero program in the U.S. and find little evidence that the resulting reduction in migrant labor supply increased wages or employment of native farm workers.

More recently, East et al. (2023) analyzed the local labor market effects of immigration enforcement actions over 2008 to 2014, finding that they reduced employment of unauthorized immigrants due both to the direct effect of out-migration and the indirect effect of lower labor force participation of immigrants that did not emigrate. In contrast to the findings of Dustmann et al. (2017), which found negative short-run effects on native employment and wages from a *positive* immigration shock, East et al. (2023) find negative short-run effects on native employment and wages from a *negative* immigration shock. They argue that these effects are explained by higher labor costs that decrease job creation and a reduction in local consumption.

It is important to note that the local economic effects estimated in these types of cross-geographical analyses may well be different from the national economic effects of immigration. There may be important general equilibrium effects that are absorbed by the intercept or time fixed effects of such analyses. In addition, as argued by Borjas (2003), native workers may move to other local labor markets in response to a local immigration shock. Indeed, Monras (2020) found evidence consistent with such domestic migration responses. Domestic migration responses to immigration can also lead individual-based outcomes to differ importantly from place-based outcomes, a point emphasized in Dustmann et al. (2025). Echoing the earlier concerns by Borjas (2003) regarding both geographic and occupational mobility, they demonstrate the usefulness of individual longitudinal data for tracking the medium- to longer-run employment and wage effects of an immigration shock on the workers living in the area when the shock occurred.

In the study by Borjas (2003), he used national labor market variation across skill (education-by-experience) cells, instead of geographic areas, and assumed that immigration into these cells is exogenous. He found large negative effects of immigration on the wages of "competing" workers, especially within low-skilled cells. Caiumi and Peri (2024) followed a similar national-level approach but introduced a skill-based shift-share IV to address endogeneity concerns. Specifically, they used a skill-based shift-share IV approach implemented on national U.S. data over 2000 to 2019, a period in which immigration was primarily of higher-skilled individuals. They found evidence of strong complementarity between immigrant and native workers, especially those with lower skills, such that higher immigration led to higher wages and employment for native workers. These results are also consistent with earlier work by Ottaviano and Peri (2012).

Many studies also have utilized the immigration network IV approach to estimate immigration's effects on local housing markets, especially on rents and house prices. The findings from this literature have received renewed attention of late as policymakers seek to assess how much of recent inflation and disinflation in shelter prices can be explained by the recent immigration swings. The seminal paper by Saiz (2007) used the immigration network IV approach and estimated an elasticity of local housing rents with respect to an exogenous increase in local (legal) immigrant population of approximately one, meaning that an immigration inflow equaling 1% of population increases rents by 1%. The effect on house prices was even larger at a little over 2%.[4]

Monras (2020), studying the spike in low-skilled immigration to the U.S. induced by the 1995 Mexican Peso crisis, found similar positive short-run effects of immigration on local housing prices. However, in the longer-run (5+ years), he found this immigration reduced rents due to reduced construction costs from an increased supply of low-wage immigrant construction workers combined with some out-migration of native-born households. Consistent with these findings, Howard et al. (2024) analyzed the housing market effects of immigrant deportations and found modest short-run declines in house prices that quickly reversed as housing supply contracted. Lastly, Chen et al. (2025) used state-level estimates of unauthorized immigrant populations from the Pew Research Center to study their effect on house prices over the 2005 to 2022 period. They found no significant effect of the stock

[4] Cabral and Steingress (2026) conducted a similar analysis recently using Burchardi et al. (2019) et al. ancestry-based IV approach and using county level data over 1985-2019. They found even larger positive effects of immigration on local housing prices on average, though they also found the effects were heterogeneous across counties, with local education levels and housing permits explaining much of the heterogeneity.

of unauthorized immigrants in a given year on the growth of house prices in that year.

3 Background, Data, and Facts on Recent Unauthorized Immigration

In this section, we provide background on trends in U.S. immigration since the 1990s, with particular emphasis on the unprecedented boom and subsequent slowdown in unauthorized immigration beginning in early 2021. We then describe the data and the construction of unauthorized immigration measures at both the national and disaggregated geographic levels, which form the basis of our cross-sectional analysis. Finally, we highlight the demographic features of recent unauthorized immigrants and the geographic variation revealed by our adminstrative microdata.

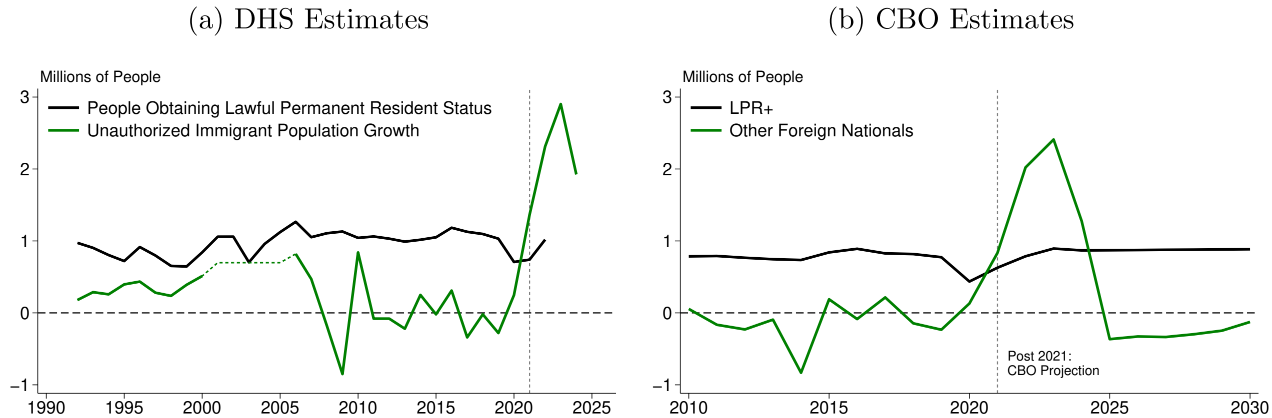
3.1 Background

Since the last major statutory overhaul of immigrant admissions under the Immigration Act of 1990, legal permanent immigration to the U.S. has been remarkably stable. By contrast, unauthorized immigration—people who entered the U.S. without being formally admitted under immigration law—has been much more variable. This contrast can be seen in Figure 1 (left panel), in which we combine multiple Department of Homeland Security (DHS) data sources to construct annual estimates of net unauthorized immigration over the past three decades. The figure shows that this category of immigration rose during the 1990s, peaked in the mid-2000s, and contributed to an increase in the total unauthorized population from approximately 3.5 million in 1990 to about 12 million by 2007 (see also, Baker and Warren 2024; Krogstad et al. 2019; Department of Homeland Security 2003). Around the Great Recession, net inflows declined sharply and remained near zero throughout the 2010s. Thus, on the eve of the pandemic, U.S. immigration was characterized by stable legal admissions and a decade of subdued unauthorized immigration, making the sharp increase in unauthorized entries beginning in 2021 a dramatic departure from the preceding regime.

Between early 2021 and early 2024, unauthorized immigration rose sharply. DHS reports that nationwide total encounters exceeded two million annually during 2021-2024.[5] Although encounters do not map one-for-one into net unauthorized inflows, they reflect a substantial rise in entry attempts and asylum claims. As shown in Figure 1 (right panel), the Congressional Budget Office (CBO) and a number of recent studies similarly estimate a

[5] See https://www.cbp.gov/newsroom/stats/nationwide-encounters.

Figure 1: U.S. Immigration Trends



Sources: U.S. Department of Homeland Security (DHS); Congressional Budget Office (CBO).
Notes: Panel (a) shows people obtaining lawful permanent residents status (LPRs) from DHS *Yearbook of Immigration Statistics, 2023*. Unauthorized immigrant population growth combines two sources. For 1991–2019, it is computed from DHS annual estimates of the unauthorized resident population. For 2020–2024, net inflows are measured by annual encounters plus estimated got-aways minus deportations (the sum of removals, returns and expulsions), using DHS administrative data. Because DHS does not publish annual unauthorized population estimates for 2001–2005, values for those years are linearly interpolated using the 2000 and 2005 unauthorized population estimates. Panel (b) reports projections from the CBO's *The Demographic Outlook: 2026 to 2056*. LPR+ and Other Foreign Nationals denote the CBO's categories for legal permanent immigration and unauthorized immigration, respectively.

large increase in net unauthorized immigration beginning in 2021 (e.g., Edelberg et al. 2025; Duzhak and New-Schmidt 2025; Orrenius et al. 2025). The influx peaked in early 2024 and subsequently retrenched. Unauthorized inflows declined following changes in enforcement practices and processing rules implemented in June 2024 (Aramayo et al. 2024). By early 2025, encounters had fallen markedly, and net unauthorized immigration turned negative as interior removals rose to levels not observed since the mid-2010s (Wilson and Zhou 2026).

Several factors plausibly contributed to the early 2021 to early 2024 boom in unauthorized immigration. First, deteriorating economic and political conditions in countries such as Venezuela, Cuba, Haiti, Nicaragua, and Ukraine generated significant emigration pressures, with many individuals seeking asylum or humanitarian parole in the U.S.[6] Second, U.S. administrative adjustments, including expanded use of parole programs and the issuance of "Notices to Appear" (NTA), allowed substantial numbers of migrants from multiple countries

[6]By 2024, more than 7 million Venezuelans had left their country, making it one of the largest displacement crises worldwide (see United Nations High Commissioner for Refugees, Venezuela Situation, 2024). See also Federal Register FR. 63507 (October 19, 2022) "Implementation of a Parole Process for Venezuelans"; Federal Register FR. 1266 (January 9, 2023) "Implementation of a Parole Process for Cubans"; Federal Register FR. 1243 (January 9, 2023) "Implementation of a Parole Process for Haitians"; Federal Register FR. 1255 (January 9, 2023) "Implementation of a Parole Process for Nicaraguans".

to enter and remain temporarily (Congressional Budget Office 2024). Third, the unwinding of pandemic-era restrictions—including the lifting of travel constraints and the termination of Title 42 public-health expulsions—altered border processing and may have increased short-run crossing attempts. Finally, the recovery of the U.S. labor market following the COVID-19 contraction, particularly in low-wage service sectors, may have increased pull incentives for migration.

The timing and changing country composition of arrivals suggest that exogenous push factors, interacting with shifts in U.S. immigration policy, played an important role in the rapid post-pandemic increase in unauthorized immigration.

3.2 Data Sources

A key component of our empirical analysis is the construction of monthly unauthorized immigrant worker flows at the local level. Our administrative microdata allow us to construct these flows at the county level, from which we aggregate to CZs and MSAs. When possible, we conduct analyses at the CZ level, because CZs span the entirety of the U.S. and correspond closely to the concept of a local labor market. For some outcomes, such as housing prices and supply, we conduct analyses at the MSA level due to unavailability of data at the CZ level.

We rely on newly available, individual-level administrative data on unauthorized immigrants obtained through Freedom of Information Act (FOIA) requests by the Transactional Records Access Clearinghouse (TRAC) at Syracuse University.[7] We first describe these administrative datasets, and then discuss labor market data, housing market outcomes, and broader economic indicators used in the analysis. The methodology for constructing national and local UIWF, as well as their recent aggregate and geographic trends, is presented in Sections 3.3 and 3.4.

Unauthorized Immigration. As noted earlier, we use *unauthorized immigrants* to refer to individuals who enter the U.S. without formal admission under immigration law. A large share of these individuals are encountered by federal authorities at ports of entry, along the border, or in the interior and are subsequently issued an NTA in immigration court, allowing them to seek asylum or otherwise challenge removal. Historically, many such individuals have been permitted to remain in the U.S. while their cases are pending, and NTA-based entries constitute the primary source of unauthorized inflows in recent years. In addition, the expanded use of parole programs over 2021 to 2024 permitted substantial numbers of

[7] https://tracreports.org/

otherwise inadmissible individuals to enter and remain temporarily in the U.S., typically for 18 to 24 months with possible renewal.

We utilize two individual-level, restricted-use datasets covering the most important sources of unauthorized entries. The first consists of immigration court records from the Executive Office for Immigration Review at the Department of Justice. These data include information on nationality, age, reported U.S. residence location (zip code), reported date of entry into the U.S., date of NTA issuance, court decisions, and custody status. The residence information enables us to measure unauthorized NTA-based entries at detailed geographic levels. The second dataset, collected by U.S. Customs and Border Protection within DHS, contains individual-level records of paroles granted at and between ports of entry. These data report nationality, age, entry date, and the specific parole program, but do not include subsequent U.S. residence location. We access both datasets through TRAC.

We supplement these microdata with DHS estimates of nationwide "got-aways", referred to as individuals who entered without being apprehended by border authorities.[8] For external validation of our estimates of unauthorized immigrant net inflows, we also collect DHS aggregate statistics on encounters and deportations (the sum of removals, returns, and expulsions). The aggregate measure—encounters plus got-aways minus deportations—is commonly used by the CBO and other recent studies to estimate post-pandemic net unauthorized immigration. These series, however, are available only at the national level and were discontinued after November 2024 following the suspension of official deportation reporting.

Employment and Wages. We measure local employment and wages using the BLS' Quarterly Census of Employment and Wages (QCEW). QCEW is constructed from administrative establishment-level records based on state unemployment insurance filings, covering nearly the universe of private nonfarm employment. Employment is available monthly and reflects the number of individuals on employer payrolls during the pay period that includes the 12th of the month, which is the same definition used in the national Employment Situation report. Wages are average weekly wages of employees over a quarter. The QCEW data are also used to annually benchmark the Current Employment Statistics (CES) employment counts. In contrast, the monthly CES data are based on a payroll survey of a sample of employers and is therefore subject to sampling error. It also is not available below the MSA level.

[8] DHS publishes official estimates of nationwide got-aways through 2022. For 2023 onward, we use updated figures reported in DHS press releases (see, e.g., U.S. Department of Homeland Security, April 28, 2025) and information obtained through FOIA requests filed with U.S. Customs and Border Protection (see *New data reveals illegal immigrants eluding Border Patrol spiked under Biden, surpassing predecessors*, Shaw and Melugin, May 15, 2024).

Housing Market and Broader Economic Conditions. We measure house price growth using the MSA-level Zillow Home Value Index, which is a repeat-sales–style, model-based index that captures the typical home value within the 35th to 65th percentile range, weighted to reflect the owner-occupied housing stock in a given MSA. For market rents, we use the MSA-level Zillow Observed Rent Index, a monthly repeat-rent index that measures the typical market rent, again in the 35th to 65th percentile range and weighted to reflect the rental housing stock. The rent index is available for all homes, single-family residences and multi-family residences.

To assess robustness, we also examine alternative repeat-sales–based price measures, including the Freddie Mac House Price Index, the CoreLogic Home Price Index, and the CoreLogic Single-Family Rent Index.[9] For measuring new housing supply, we collect monthly MSA-level building permit data from the Census Bureau and annual housing unit counts from ACS. Finally, we obtain county-level annual personal income and its components from the Bureau of Economic Analysis' Regional Economic Accounts and aggregate these measures to the CZ level for the empirical analysis.

Household Survey Data. We use ACS, a large annual household survey providing detailed demographic, labor market, housing, and migration information, for two purposes. First, we use the 2015–2019 five-year ACS to measure the geographic distribution of immigrants across CZs and MSAs by country of origin, which is central to our empirical design. Second, we use the 2021–2023 one-year ACS to construct local labor- and housing-market outcomes that complement our analyses based on QCEW and Zillow data. Specifically, we construct employment and average hourly wages by demographic group and industry, as well as self-reported home values and contract rents, consistent with survey-based measures commonly used in the previous literature (e.g., East et al. 2023; Cabral and Steingress 2026; Saiz 2007).

3.3 Measuring Monthly Unauthorized Immigration Flows

Monthly *net unauthorized immigration flows*, or *net entries*, are the difference between entries and exits. We measure entries as the sum of NTA-based inflows, parole-based en-

[9] Compared with the Zillow indexes, the Freddie Mac index is constructed from Freddie Mac's mortgage portfolio and therefore covers only homes financed with conforming, securitized loans. By contrast, the CoreLogic indexes are based on transaction data that include homes not financed with mortgages, but they do not provide coverage for all MSAs.

tries and got-aways.[10,11] We measure exits using several sources. First, based on the NTA microdata, we classify individuals as exits if they receive a removal or voluntary departure order from an immigration court, or if they are newly booked into detention while awaiting court proceedings. Second, for 2025 we estimate voluntary exits among unauthorized immigrants who entered during 2021–2024 but were not issued an NTA—primarily parolees and got-aways—using official information on parole duration and the observed monthly exit rate among NTA-based entrants (see Appendix A for construction details). Appendix Figure A1 presents all entry and exit categories incorporated in our estimates.[12] We use these sources and methods to first measure the monthly entries, exits, and net entries of unauthorized immigrants at the national level.

Panel (a) of Figure 2 shows our estimates of monthly unauthorized entries, exits, and net entries at the national level from October 2013 through July 2025. The vertical lines indicate start and end months of the "boom" period (March 2021–March 2024) and the subsequent "slowdown" period (June 2024–June 2025). Unauthorized entries increased sharply beginning in early 2021, peaked in early 2024, and declined thereafter, consistent with the patterns seen in the DHS and CBO annual data discussed above. Starting in early 2025, following the termination of major parole programs and more stringent immigration enforcement policies, unauthorized entries fell to their lowest levels since the pandemic. As exits rose drastically in 2025, net entry turned negative in February 2025 and reached -89,000 by the end of our sample in July 2025.

Panel (b) of Figure 2 compares our microdata-based estimates of net unauthorized inflows with those constructed from DHS aggregate statistics—encounters plus got-aways minus deportations—available through November 2024. The two series track each other closely over the overlapping period, providing external validation of our measurement approach.

Although the country composition of unauthorized immigrants expanded during 2021-

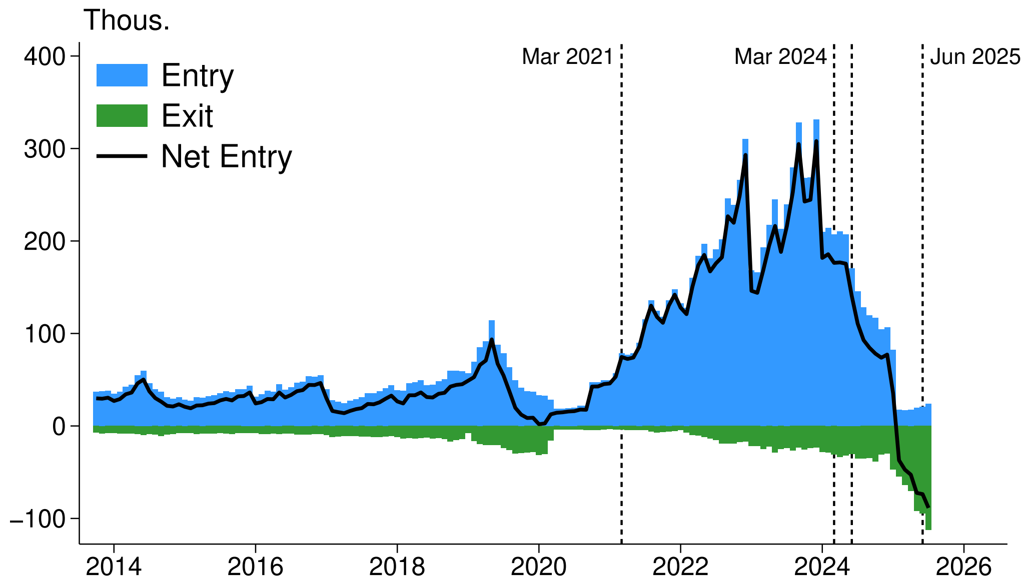
[10] In our NTA microdata, the recorded date of entry is missing for approximately 30% of individuals. For these cases, we use the date of initial NTA issuance—available for all individuals—as a proxy for the entry date. This is supported by the fact that in cases where both dates are observed, roughly 75% differ by no more than one month.

[11] Using our NTA microdata, we classify individuals as "likely got-aways" if the gap between the recorded entry date and the initial NTA issuance date exceeds 90 days. We subtract these cases from the DHS aggregate got-away estimates to mitigate potential double-counting, as such individuals may already be captured in the NTA-based inflow measure.

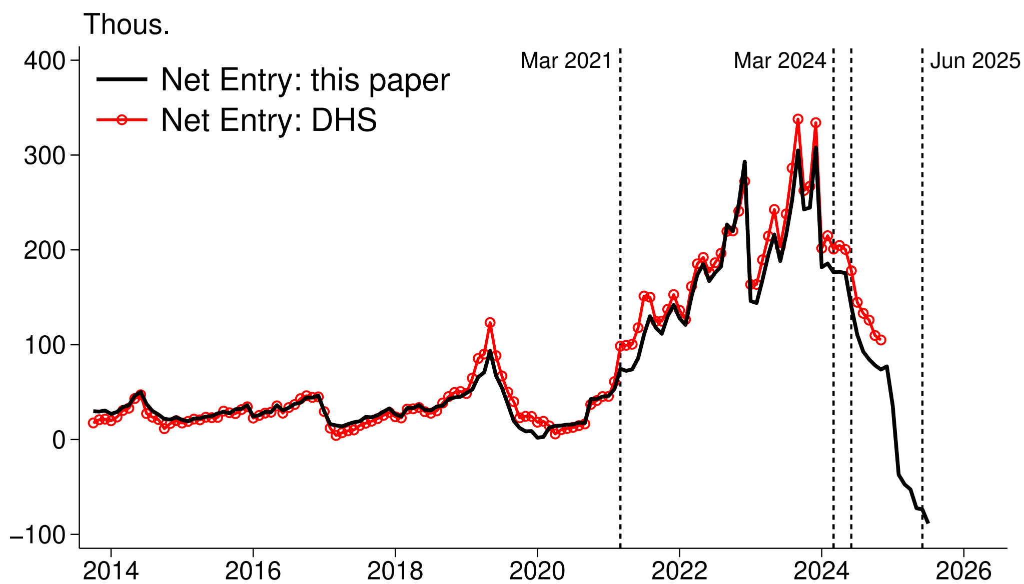
[12] The entry and exit categories incorporated in our estimates are broadly consistent with those used by the CBO in its annual projections of "Other Foreign Nationals." We exclude two additional categories—temporary visa overstays (which contribute to entries) and adjustments to lawful permanent resident status (which contribute to exits)—for two reasons. First, reliable estimation of these flows is challenging given limited administrative data. Second, recent CBO projections suggest that the two categories are of similar magnitude and largely offset each other.

Figure 2: Monthly Net Unauthorized Immigration

(a) Entry, Exit, and Net Entry



(b) External Validation



Notes: Panel (a) reports monthly unauthorized entries, exits, and net entries constructed using the data described in Section 3.2. Entries are the sum of NTA-based inflows, parole-based entries, and estimated got-aways. Exits include removals or voluntary departures ordered by immigration courts, newly detained individuals awaiting court proceedings, and estimated voluntary departures of unauthorized immigrants. Net entry is defined as entries minus exits. Panel (b) compares our estimated net entry with a measure implied by DHS aggregate statistics, constructed as encounters plus got-aways minus deportations (the sum of removals, returns and expulsions).

2024, a relatively small set of origin countries in Central and South America accounted for the majority of unauthorized entries. In Appendix Figure A2, we present monthly net inflows from eleven countries that, based on DHS encounter data, had the highest numbers of encounters between early 2021 to early 2024. These countries are Mexico, Guatemala, Honduras, El Salvador, Venezuela, Colombia, Cuba, Ecuador, Nicaragua, Haiti, and Peru. Following Cheremukhin et al. (2025), we refer to these countries as high-encounter countries.[13]

Panel (a) of Figure 3 shows that high-encounter countries accounted for nearly all net unauthorized entries prior to the pandemic. Their share declined modestly to roughly 80% during 2021–2024, before rising to 93% in 2025. These countries therefore account for the vast majority of post-pandemic immigration inflows and drive the aggregate dynamics during the boom and subsequent retrenchment.

So far, we have focused on measuring total unauthorized immigration. Since our empirical analysis examines the impact of UIWF on local labor markets, we next assess the working-age composition and employment of unauthorized immigrants. Because the individual-level NTA and parole data record each person's age, we can measure working-age immigration from these sources. For the (smaller) got-aways category, we assume their working-age share is the same as that of the other two categories. As shown in Panel (b) of Figure 3, approximately 75%-80% of unauthorized immigrants arriving after 2021 are of working age (orange line). This is considerably higher than the 62% working-age share for U.S.-born individuals over the same period based on ACS data. Among working-age unauthorized immigrants, we estimate that approximately 70% are employed (blue line). This estimate is based on the employment rate of working-age immigrants who arrived in the U.S. within the previous three years from high-encounter countries, using 2021–2024 ACS one-year microdata. Taken together, these figures imply that between 50 and 60% of unauthorized immigrants entering the U.S. in recent years are likely workers.

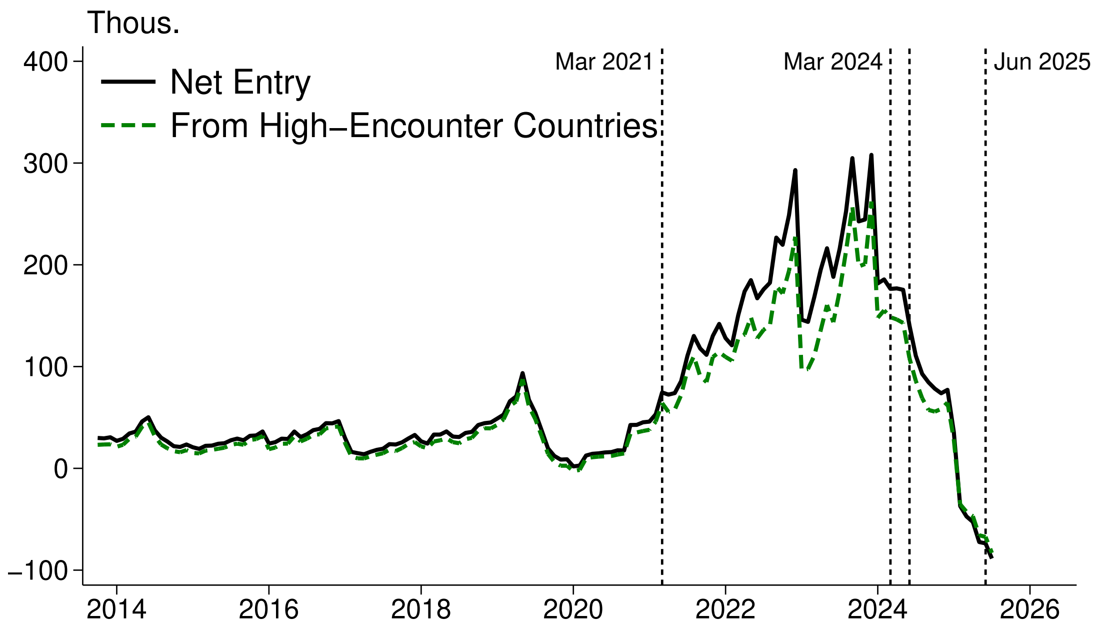
3.4 The Geography of Unauthorized Immigrant Worker Flows

The administrative data for NTAs, which accounted for the vast majority of unauthorized immigration in recent years (see Appendix Figure A1), includes individuals' current county of residence. Because residential location is unobserved in our data for parolees and got-

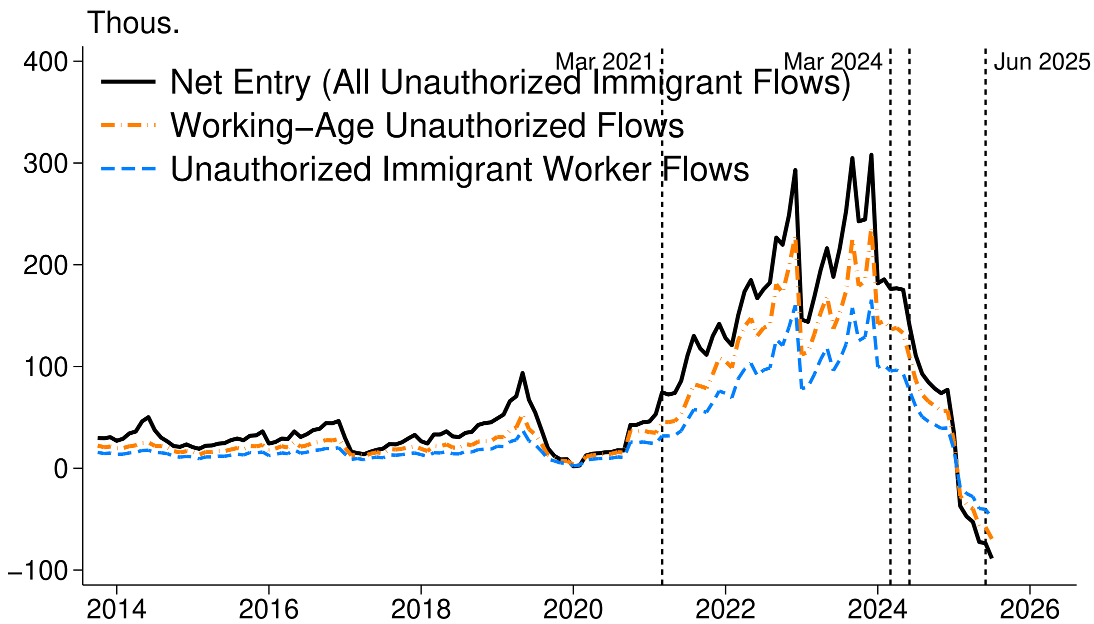
[13] We construct country-specific net unauthorized inflows using an approach similar to the aggregate net entry measure. The main difference is that we distribute aggregate got-away estimates to individual countries according to their shares of national NTA-based entries and parole-based entries (in between ports of entry), given that these two entry categories are most similar to got-aways.

Figure 3: Country and Age Composition of Net Unauthorized Immigration

(a) By Country



(b) By Age

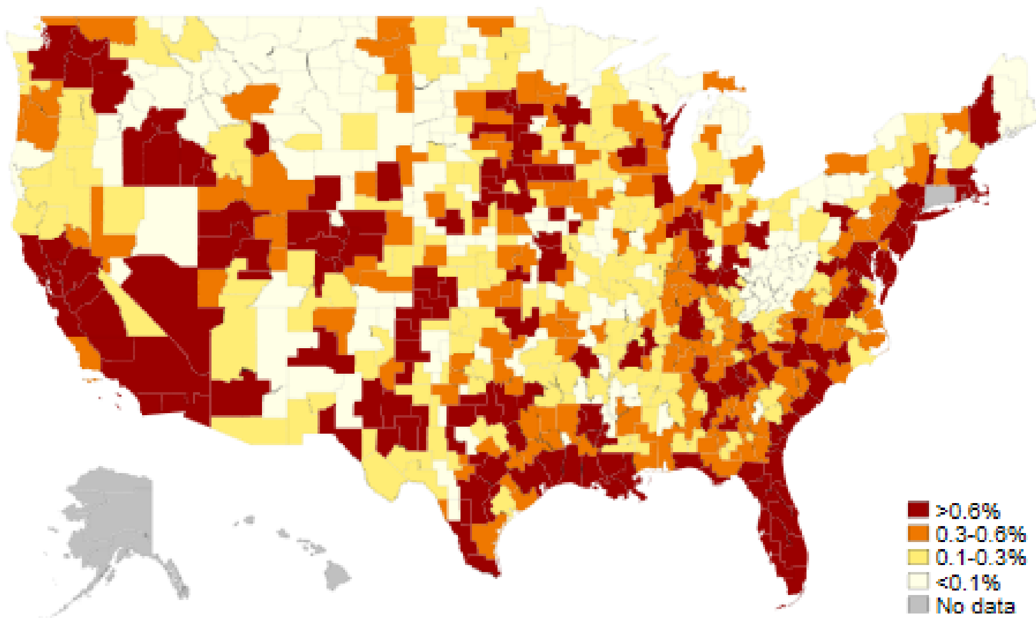


Notes: High-Encounter countries include Mexico, Guatemala, Honduras, El Salvador, Venezuela, Colombia, Cuba, Ecuador, Nicaragua, Haiti, and Peru. Working age refers to age 16-65. Unauthorized immigrant worker flows refers to working-age unauthorized immigrants who are employed.

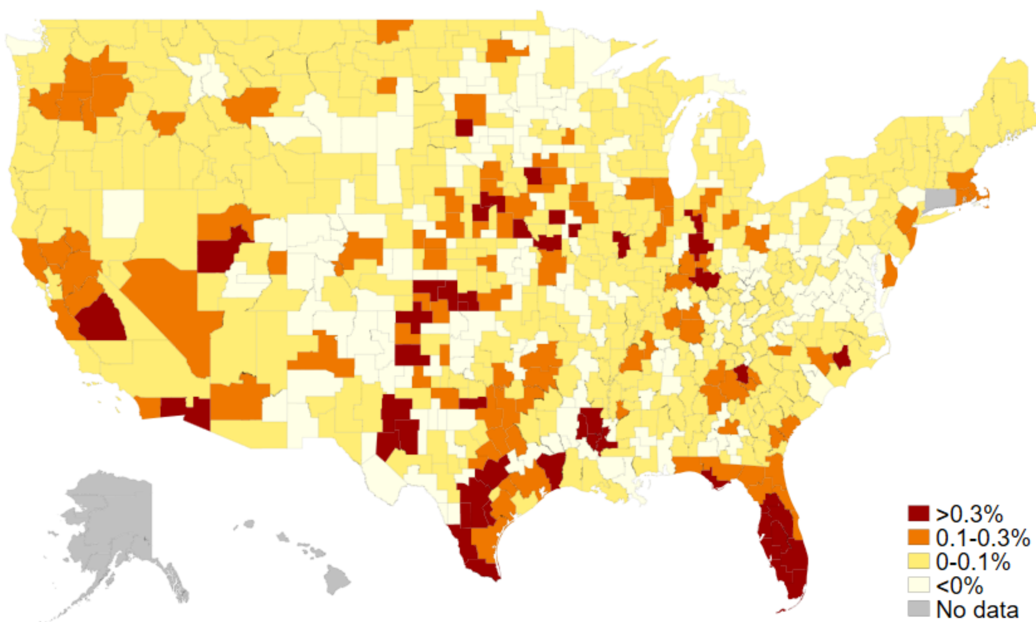
aways, we use this NTA information on county, as well as country of origin, to apportion our aggregate measures of national net unauthorized immigration across counties.

Figure 4: Unauthorized Immigrant Worker Flows By Commuting Zone

(a) March 2021-March 2024, Annual Rate



(b) June 2024-June 2025



Notes: Unauthorized immigrant worker flows are annualized and scaled by beginning-of-period employment for each commuting zone in each episode.

Specifically, we implement a scaling procedure that adjusts local NTA-based entries and exits according to their origin-specific shares of corresponding national flows. This approach ensures that the sum of net unauthorized immigration across locations matches both the national and origin-specific totals (see Appendix A for construction details). We then construct county-level UIWF analogously to the national measures, and obtain net entries for all individuals, working-age individuals, and likely workers (i.e., UIWF). For our empirical analyses, we aggregate these county-level measures to both CZs and MSAs.[14]

Figure 4 shows the resulting measure of UIWF, as a percent of beginning-of-period employment, for each commuting zone in two periods: the boom period (March 2021–March 2024) and the subsequent slowdown (June 2024–June 2025). During the boom, areas in the West (including California and much of the mountain west), the southern belt of states (Arizona, Texas and Florida) and the Northeast (particularly around the New York City metropolitan area) experienced the largest immigrant worker inflows.

In the slowdown period, most commuting zones experienced near zero net inflows, and roughly one-quarter experienced net outflows. Many parts of the Eastern seaboard, in particular, saw a stark slowdown relative to the boom period. In a companion piece (Wilson and Zhou 2026), we identify a period of net aggregate outflows (February 2025–July 2025) and document that places with the largest unauthorized worker inflows during the boom also experienced the largest subsequent outflows of unauthorized workers, with the correlation across counties between boom-period and slowdown-period UIWF equaling -0.96.

4 Empirical Strategy

Our goal is to estimate the causal effects of unauthorized immigration, and in particular UIWF, on local employment and wage growth, housing market outcomes, and broader economic conditions. This section discusses our main identification strategy and alternative empirical approaches used to assess robustness.

We focus primarily on cross-sectional effects during the national boom period (March 2021–March 2024). While we also offer some results on the subsequent slowdown period,

[14]The scaling procedure we use to construct local UIWF assumes that, conditional on the country of origin, parolees and got-aways exhibit settlement patterns similar to those of individuals who entered through NTAs. This assumption is consistent with a large literature documenting the importance of immigration enclaves and network-driven settlement patterns. That said, little is known about how the settlement patterns may differ across these categories of unauthorized immigrants, especially since the pandemic. However, any resulting measurement error in UIWF is unlikely to meaningfully affect the validity of our cross-sectional IV estimates in Sections 5 and 6 for two reasons. First, as noted above and shown in Appendix Figure A1, the categories for which we lack geographic information are small relative to the NTA category. Second, our instruments are constructed primarily from aggregate origin-specific inflows and pre-existing immigrant shares from ACS data. As a result, the IV estimates remain unbiased even if the endogenous variable—local UIWF—is measured with error, although such error could attenuate the OLS estimates.

we treat that as an extension, subject to particular caution in interpreting results, for three reasons. First, although the effects of net outflows, such as those associated with intensified interior deportations, are of independent interest, our data cover only a limited period of net aggregate outflows (February–July 2025), which precludes a comprehensive evaluation of deportation effects. Second, measures of unauthorized outflows are subject to greater uncertainty, particularly from 2025 onward, given limited information on voluntary exits. Third, for the slowdown period, lagged effects from the earlier boom in UIWF may potentially be important. While estimating such lagged effects, and accounting for their possible endogeneity, is potentially feasible, it introduces considerable multicollinearity and imprecise estimates as a result. By contrast, meaningful effects of previous UIWF on boom period economic outcomes is unlikely, because UIWF was generally close to zero over 2020–2021. Indeed, as we show in Section 5.2, we find little or no evidence of pre-trends in our boom period employment and wage results.

The OLS estimate of the effect of UIWF on a local outcome of interest is obtained from the following cross-sectional regression:

$$\frac{\Delta Y_{d,T}}{Y_{d,t_0}} = \beta_0^T + \beta_1^T \frac{I_{d,T}}{E_{d,t_0}} + \mathbf{x}_d \boldsymbol{\beta}_2^T + \varepsilon_d^T, \tag{1}$$

where d indexes geographic destinations (commuting zones or MSAs). T denotes the period of interest, and t_0 is the initial month of T. The dependent variable represents the percentage change in the outcome of interest over period T, $I_{d,T}$ denotes UIWF over T (constructed as described in Section 3.4), and E_{d,t_0} is employment at the beginning of the period. The vector $\mathbf{x}_d$ includes predetermined controls to mitigate concerns about unobserved heterogeneity correlated with UIWF. Specifically, we include NAICS 2-digit industry employment shares from 2015-2019 QCEW data, as well as demographic characteristics measured using 2015–2019 ACS five-year data, including age-group shares (16–24 and 25–54), educational attainment shares (high school or below; bachelor's degree or above), and the non-white population share. Standard errors are heteroskedasticity-robust and clustered at the state level to account for spatial correlation across CZs or MSAs within states. We use weighted least squares, weighting by initial employment, due to the likelihood of measurement error inversely related to CZ or MSA size.

We consider a number of alternative economic outcomes for $Y_{d,t}$ in the dependent variable. In addition to employment, we examine wages, house prices, market rents, total personal income, labor income, and income from government transfers. For the latter three, we report results both in absolute and per capita terms.[15] To estimate the extent to which immigration flows affect housing supply, we use as the dependent variable the cumulative number of new building permits issued over period T, normalized by the initial housing stock, $\frac{\text{Permits}_{d,T}}{\text{HousingUnits}_{d,t_0}}$. This is analogous to an investment-to-capital ratio.

4.1 Identification

The primary concern with the OLS estimates is that unobserved factors may jointly affect unauthorized immigration and local economic outcomes, even though equation (1) is estimated in differences and includes a rich set of controls. As emphasized in the literature, two identification challenges are particularly salient.

First, reverse causality may arise if areas experiencing positive local demand shocks attract immigrant workers contemporaneously, generating a positive correlation between immigration inflows and employment growth that does not reflect a causal labor supply effect.[16] Second, origin–destination-specific confounding factors may lead to endogenous sorting of immigrants over time (Burchardi et al. 2019; Terry et al. 2026). For example, persistent local demand shocks can induce sustained inflows from specific origin countries, creating a spurious relationship between current employment growth and past immigration inflows from those origins—flows that are often used to predict current immigration.[17]

A conventional approach in the literature is to construct a shift-share instrument that, for each destination locality, interacts a national origin-specific inflow ("the shift") with the pre-existing geographic distribution of immigrants from origin o to that destination ("the share"), and then aggregates across origins (Card 2001; Card 2009). The identifying assumption is that newly arriving immigrants disproportionately settle in locations with established co-ethnic networks or "enclaves." However, as discussed above, this approach may be subject to persistent origin–destination confounding. If local demand shocks are persistent, locations that experienced earlier growth may attract both past immigrants and subsequent immigrants from the same specific origins. In that case, the instrument assigns large pre-

[15]For all outcomes except employment, we use the log change to approximate the growth rate. For employment, we use the actual percentage change over the period, though using the log change yields very similar results.

[16]As an example of such reverse causality, Caballero et al. (2023) show that adverse local labor market shocks in the U.S. during the Great Recession led to reduced immigration to the U.S. from the areas of Mexico with the strongest historical ties to those U.S. labor markets.

[17]Terry et al. (2026) describe an example in which persistent productivity shocks to the technology sector in Silicon Valley attract successive waves of high-skilled workers from India. An analogous example in our context is that persistent demand shocks in Miami may have attracted Cuban immigrants with industry-specific skills (e.g., in hospitality or food services). Subsequent demand shocks in these sectors—such as tourism booms—could then generate renewed inflows from the same origin group, confounding causal inference.

dicted inflows precisely to areas with favorable demand conditions, potentially conflating immigration effects with persistent local growth trends.

To address this concern, we implement a two-way leave-out shift-share instrument, building on Card (2001) and Terry et al. (2026). Departing from the conventional approach, we construct the instrument by interacting (i) a leave-out shift, i.e., current national inflows from origin o excluding the flows to destination d, with (ii) a leave-out share, i.e., the distribution across destinations of early immigrants from all origins other than o. The corresponding first stage is:

$$\frac{I_{d,T}}{E_{d,t_0}} = \alpha_0^T + \alpha_1^T \sum_o \left(I_{o,-d,T} \times \frac{\bar{M}_{-o,d}}{\bar{M}_{-o,.}} \times \frac{1}{\bar{E}_d} \right) + \mathbf{x}_d \boldsymbol{\alpha}_2^T + u_d^T, \tag{2}$$

where $I_{o,-d,T}$ denotes unauthorized immigrant worker flows from origin o over period T, excluding those entering d. The term $\frac{\bar{M}_{-o,d}}{\bar{M}_{-o,.}}$ measures the pre-boom share of immigrant workers from origins other than o residing in destination d. We scale the shift-share interaction by the location's pre-boom employment $\bar{E}_d$ (measured using 2021 ACS data) to match the normalization used in the endogenous variable.[18] The shares, $\frac{\bar{M}_{-o,d}}{\bar{M}_{-o,.}}$, are computed using the 2015-2019 ACS five-year data.

Our identification strategy exploits two sources of variation: cross-origin variation in national unauthorized inflows and cross-destination variation in the pre-existing distribution of immigrant workers from other origins. Intuitively, when national inflows from origin o increase due to origin-specific push factors, destinations that are generally attractive to immigrants from many origins—rather than uniquely attractive to immigrants from origin o—receive larger predicted inflows.

To illustrate how this mitigates origin–destination confounding, consider Cuban immigrants in Miami. Suppose persistent demand shocks in Miami—such as sustained growth in specific industries—were the main reason that historically attracted Cuban immigrants with industry-specific skills. Under a conventional shift-share instrument, Miami's large Cuban enclave would generate high predicted Cuban inflows whenever Cuban immigration to the U.S. rises, even if that immigration boost was driven by persistent local demand conditions. In contrast, our two-way leave-out instrument first removes the Cuban inflows into Miami from the national shift, such that the predicted Cuban migration to the U.S. is given by the inflows into all destinations other than Miami. Second, it removes the Cuban share from Miami's pre-existing immigrant distribution, thus predicting Miami's share of new Cuban

[18]We use ACS employment data in scaling the instrument to be consistent with the use of ACS data for measuring the shares component of the instrument. Using instead March 2021 QCEW employment data for this scaling yields very similar results.

immigrants based on Miami's share of new immigrants from all origins other than Cuba. The predicted inflow of Cubans to Miami, then, depends on national Cuban inflows to other destinations and on Miami's attractiveness to non-Cuban immigrant groups. As a result, persistent Cuban-specific sorting into Miami does not mechanically inflate the predicted inflow, mitigating concerns that the instrument captures origin–destination-specific demand shocks rather than exogenous immigrant worker supply shocks.

4.2 Alternative Empirical Strategies

In recent work, Burchardi et al. (2019) and Terry et al. (2026) develop a new instrument to isolate plausibly exogenous variation in the pre-existing ancestry of current U.S. residents. The instrument is constructed in two steps. First, the number of residents in destination d at time T, say 2010, with ancestry from origin o, denoted as $A_{o,d}^{2010}$, is predicted using historical U.S. decennial census data:

$$A_{o,d}^{2010} = \delta_o + \delta_d + \sum_{t=1880}^{2000} \alpha_t I_{o,-r(d),t} \times \frac{I_{-c(o),d,t}}{I_{-c(o),.,t}} + \mathbf{x}_{o,d}\boldsymbol{\gamma} + \epsilon_{o,d}, \tag{3}$$

where $I_{o,-r(d),t}$ denotes the number of immigrants from origin o entering the U.S. at time t, excluding destination d's census region. $\frac{I_{-c(o),d,t}}{I_{-c(o),.,t}}$ is the share of immigrants entering in time t, who settled in destination d, excluding those from countries in origin o's continent. $\mathbf{x}_{o,d}$ is a vector of controls, including the geographic and latitude distances between o and d, as well as higher-order terms of $I_{o,-r(d),t}$ and $\frac{I_{-c(o),d,t}}{I_{-c(o),.,t}}$. The predicted ancestry is given by

$$\hat{A}_{o,d}^{2010} = \sum_{t=1880}^{2000} \hat{\alpha}_t \left(I_{o,-r(d),t} \times \frac{I_{-c(o),d,t}}{I_{-c(o),.,t}} \right)^{\perp},$$

where $\perp$ indicates that the interaction term has been residualized with respect to all controls in equation (3).

The second step applies a shift-share design similar to our preferred strategy in Section 4.1, interacting the predicted ancestry with origin-specific aggregate immigration flows with leave-out adjustments. Adapting this step to our cross-sectional setting yields the following first stage:

$$\frac{I_{d,T}}{E_{d,t_0}} = \alpha_0^T + \alpha_1^T \sum_o I_{o,-d,T} \times \hat{A}_{o,d}^{2010} \times \frac{1}{\bar{E}_d} + \mathbf{x}_d \boldsymbol{\alpha}_2^T + u_d^T,$$

where $I_{o,-d,T}$ and $\bar{E}_d$ are defined as in equation (2).[19] This approach isolates variation in

[19] We obtain $A_{o,d}^{2010}$ and the historical pull-push interaction terms in equation (3) from the authors' website:

local ancestry composition that arises from historically staggered origin-specific migration shocks interacting with settlement patterns attractive to the average migrant at the time. By construction, the predicted ancestry measure is purged of persistent origin–destination matching patterns and contemporaneous local demand shocks, providing plausibly exogenous variation in exposure to current immigration flows.

As we will show in Section 5, our baseline results are robust to using the ancestry-based instrument. Nevertheless, we prefer the two-way leave-out instrument for two main reasons. First, when adapted to our cross-sectional setting over the post-pandemic period, the ancestry instrument exhibits weaker first-stage relevance. Second, applying the Goldsmith-Pinkham et al. (2020) Rotemberg-weight decomposition reveals that the ancestry instrument places disproportionate weight on a single origin—Mexico—which accounts for roughly two-thirds of the identifying variation. This suggests that the IV estimates may be sensitive to potential misspecification or endogeneity specific to the Mexico ancestry component. By contrast, the Rotemberg-weight decomposition for our preferred two-way leave-out instrument is more evenly distributed across origins, with no single country accounting for more than 20% of the total weight.

We also implement the conventional shift-share design without leave-out adjustments. The IV estimates obtained from this approach are similar to those from our preferred specification. Moreover, the conventional instrument passes standard specification tests, suggesting that origin–destination-specific confounding is not strongly detected in the data.

5 Impacts on the Labor Market

In this section, we present results on how UIWF affects the local labor market, specifically employment and wages. We start, in subsection 5.1, with our main results during the boom period, March 2021 to March 2024, on employment and wages for total private nonfarm sector. In subsection 5.2, we assess the validity of our identification strategy by investigating pre-trends, rotemberg weights, and over-identification tests associated with our leave-out shift-share design. In subsection 5.3, we consider a number of extensions: (1) estimating impacts of UIWF by industry, (2) estimating impacts during the immigration slowdown period (mid-2024 to mid-2025), (3) assessing whether and how estimated effects change as one expands the sample period, one month at a time, from ending in March 2024 to ending in June

https://www.immigrationshock.com/ancestry-instruments. The data are available at the county-origin level. We therefore first estimate $\hat{A}^{2010}_{o,d}$ at the county-origin level using the number (rather than the log number) of residents with ancestry o as the dependent variable, and then aggregate the predicted ancestry measure to the CZ-origin level using the 1990 county-to-CZ crosswalk.

2025, (4) estimating effects on annual labor market outcomes from the American Community Survey, and (5) conducting back-of-the-envelope calculations of the implied boom-period effects for both a "typical" local area and the average local area.

5.1 Impacts on Total Private Nonfarm Employment & Wages

We start with our main results on employment and wages, using data at the CZ level from QCEW as described in Section 3.2. Before presenting the regression results, we first visualize the correlation between UIWF and employment growth across CZs for the main period of interest, March 2021 through March 2024, which corresponds to the national immigration boom. Figure 5 provides a scatter plot of these two variables along with an OLS linear fit line. Each circle represents a single CZ and its size is proportional to the CZ's initial employment level. There is a clear positive relationship between UIWF and employment growth.

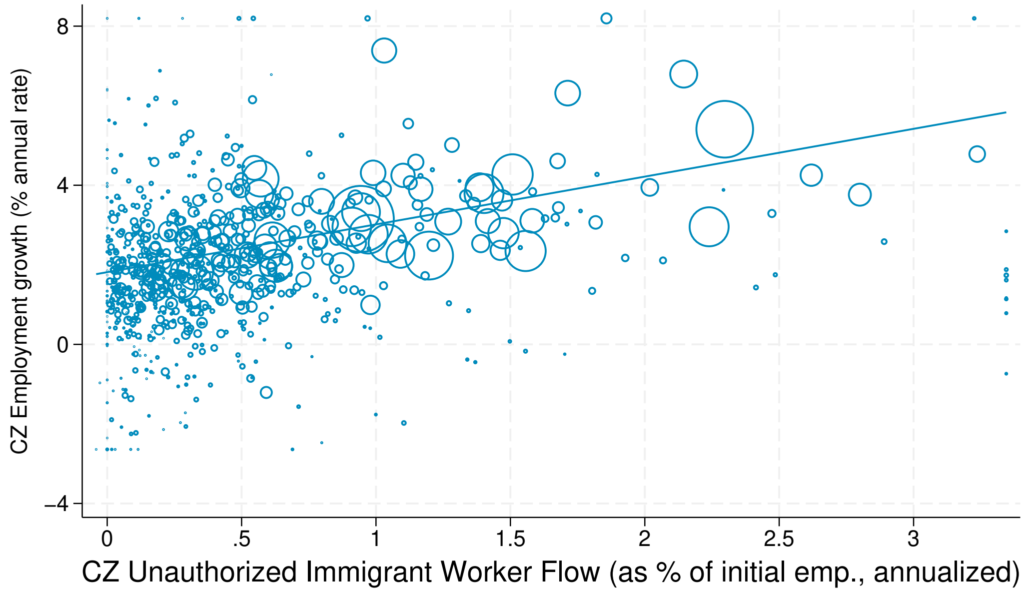
The regression results from estimating equation (1), for both employment growth and wages, are shown in Table 1. The OLS estimate of the effect of UIWF on employment growth (column 1) is 0.84. One cannot reject the hypothesis that this coefficient equals 1; one can strongly reject the hypothesis that it equals 0. The IV estimate is 0.96 (column 2), implying that an increase in UIWF equal to 1% of initial employment increases local employment by 0.96%—a nearly one-for-one effect. The effect is highly statistically significant. Similar results are obtained using MSA level data (see Appendix Table B1). It is also important to note that the first-stage regression underlying this result—that is, the regression of UIWF on predicted immigration flows—yields an F-statistic near 30, well above standard critical values associated with weak instrument bias.

The OLS and IV results for average weekly wages are shown in columns (3) and (4), respectively. The OLS slope coefficient is small and statistically insignificant. The IV coefficient is larger, approaching -1, but is imprecisely estimated and statistically insignificant. Thus, we cannot reject the hypothesis that local wages were unaffected by UIWF. That said, one should keep in mind that average weekly wages are the product of average hours per week and the average hourly wage, which are not available in QCEW data. If UIWF push up hours—consistent with the positive effect we find for employment—while pushing down hourly wages, as one might expect from a labor supply shock, these countervailing effects could offset each other to leave average weekly wages unchanged.

The key regressor in these regressions is our measure of unauthorized immigrant worker flows as a share of initial employment. For comparison, we also report results in Appendix Table B2 for the effects of all unauthorized immigrant flows (all ages and unadjusted for em-

Figure 5: Employment Growth vs. Immigrant Worker Flows Across CZs

Boom Period: 2021m3–2024m3



Notes: Each circle represents a commuting zone (CZ). The size of the circles are proportional to CZ beginning-of-period employment.

Table 1: Effect of Unauthorized Immigrant Worker Flows on Employment & Wages

Boom Period: 2021m3–2024m3

	(1) OLS, Emp.	(2) IV, Emp.	(3) OLS, Wages	(4) IV, Wages
UIWF	0.840***	0.961***	-0.140	-0.933
	(0.143)	(0.274)	(0.197)	(0.608)
Constant	-0.130	-0.105	0.0877	-0.0802
	(0.113)	(0.123)	(0.121)	(0.193)
Observations	721	721	720	720
R^2	0.581	0.580	0.636	0.579
First-Stage Results:				
Coefficient Estimate		0.812***		0.812***
Coefficient Std. Error		(0.152)		(0.152)
F Statistic		28.50		28.50

Notes: UIWF (as share of initial employment) is winsorized at 99th percentile due to outliers. Observations are weighted by beginning-of-period employment. Standard errors (shown in parentheses) are heteroskedasticity-robust and clustered on state. Statistical significance is indicated by: * p<0.1, ** p <0.05, *** p < 0.01.

ployment rates) and all working-age unauthorized immigrant flows. For the IV employment growth regressions, we obtain coefficients of 0.51 and 0.65 when using all and all working-age immigrant flows, respectively. Both are statistically significant at above the 99% level. These values are very close to expectations given that our data indicate around 56% of all unauthorized immigrants and 70% of working-age unauthorized immigrants are workers. The IV wage growth regressions yield negative but insignificant effects from both all and all working-age unauthorized immigrant flows, consistent with the results using UIWF.

5.2 Validity of Identification Strategy

Before proceeding to extensions of this estimation to other outcomes and sample periods, we assess the validity of our identification strategy.

5.2.1 Assessing Parallel Trends Assumption

First, we assess the parallel trends assumptions underlying our research design by looking at pre-trends. To do so, we repeat our estimation of equation (1) for both employment growth and wage growth, keeping the right-hand side variables the same and using our preferred instrument in equation (2), but changing the time period of growth for the dependent variable from 2021m3-2024m3 to earlier 36-month intervals. Specifically, we estimate this specification for 2015m3–2018m3, 2016m3–2019m3, 2017m3–2020m3, and 2018m3–2021m3.

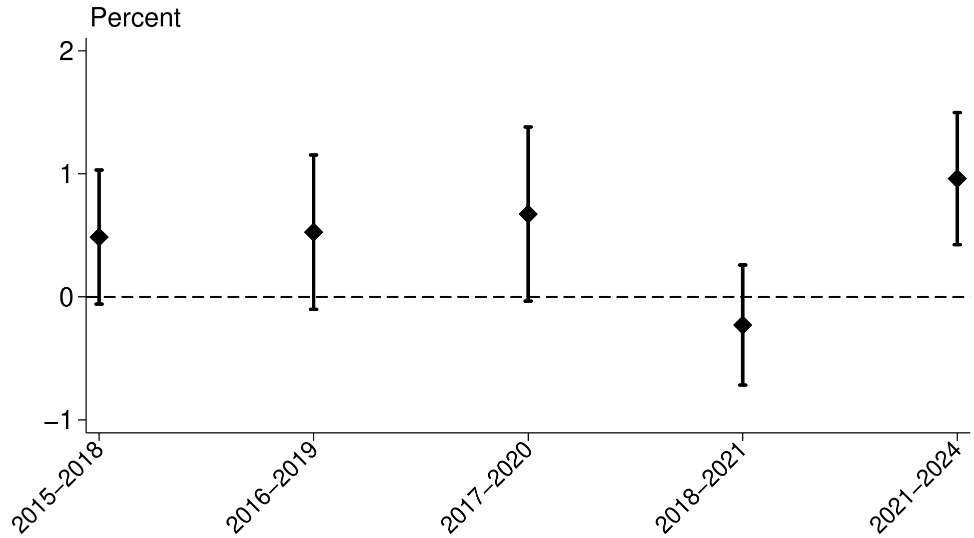
The estimated coefficient on UIWF and its 95% confidence interval from each of these regressions is shown in Figure 6, with the results of employment growth in panel (a) and those for wage growth in panel (b). For comparison, our baseline estimates for 2021m3–2024m3 are shown on the far right of each plot. For employment growth, these placebo estimates are positive, but statistically insignificant, for the first three intervals and negative and insignificant for the most recent pre-boom period interval. For wage growth, the estimated placebo effects are statistically insignificant except for a small positive and significant effect in 2017–2020, which is notably different than the negative but insignificant effect we find for 2021–2024. In sum, these placebo regressions do not point to any notable pre-trend concerns.

5.2.2 Rotemberg Weights, TSLS with Shares as Instruments, Over-ID Tests

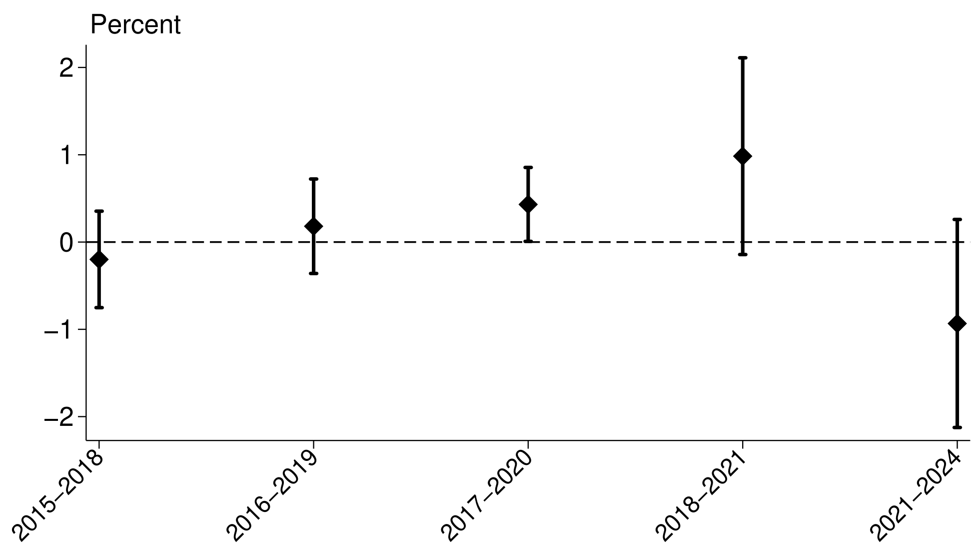
Second, we examine the properties and validity of our leave-out shift-share identification strategy, following recommendations from the recent microeconometrics literature (e.g., Goldsmith-Pinkham et al. 2020; Borusyak et al. 2025). Goldsmith-Pinkham et al. (2020) demonstrate that in a shift-share IV research design such as the one used in this paper as well as that of other papers following the Card (2001) strategy, identification relies on the

Figure 6: Placebo Regressions Using Prior 3-Year Periods

(a) Employment



(b) Wage



Notes: Each figure plots the UIWF coefficient (diamond) and 95% confidence interval (bracket) from a cross-CZ IV regression of either employment growth (panel a) or average weekly wages growth (panel b) on the same set of regressors and instruments as used in our baseline boom-period specifications.

validity of the shares. That is, identification relies on the exogeneity of the historical shares of immigrants from a given origin country that settle in each geographic area. As such, the shares corresponding to each separate origin country can be used as a separate instrument. One can then estimate the IV regression using either all country's shares as instruments, allowing for an overidentifying restrictions test, or separately using each country's shares as a single instrument, allowing one to assess how sensitive the IV coefficient of interest is to the country or countries providing the identification. Lastly, they show that one can decompose the IV estimate of interest coming from the full shift-share IV into the contributions, known as the Rotemberg weights, from each country's shares. If a single country is responsible for much of the identification, the assumption that that country's past immigrants sorted themselves geographically in a way uncorrelated with current local economic outcomes becomes particularly important.

In Table 2 we present the results of this exercise for our preferred, two-way leave-out shift-share IV estimation of the UIWF effect on boom-period employment growth. Recall that our instrument is constructed to (1) leave out the own-CZ inflow from the origin-specific shift and (2) leave out the own-country share from the pre-existing distribution of immigrants in a CZ. As detailed in Section 4.1, these two strategies reduce, but do not necessarily eliminate, the *a priori* concerns the Goldsmith-Pinkham et al. (2020) statistics are meant to assess.

The first column of panel A in Table 2 lists the origin countries used in constructing our instrument, with smaller countries grouped into a single "Other" category. The second column shows each country's Rotemberg weight. The fourth (far right) column in panel A shows the just-identified IV estimate obtained using only the share corresponding to that country as the instrument. The weighted average of these just-identified IV estimates, using the Rotemberg weights, equals the full shift-share IV coefficient of 0.961 reported in column (2) of Table 1, which is re-displayed in panel B.

We see that no single country has a Rotemberg weight above 0.16, meaning that our baseline IV estimate is not heavily influenced by the shares associated with any one country. In addition, the just-identified estimates shows that the IV estimates are very close to 1 (bounded between 0.99 and 1.14) with the exception of the estimate corresponding to the Other category. The "Other" category also has a somewhat higher Rotemberg weight of 0.27. These results for the Other category suggest that our baseline IV estimate could potentially be sensitive to the inclusion of that set of countries. Thus, in the third column of the table, we show the Rotemberg weights (panel A) and the leave-out shift-share IV estimate (panel B) obtained from excluding the Other category in constructing the shift-component of the instrument. In this case, the Rotemberg weights on all countries remain fairly low and, more importantly, the shift-share IV estimate is little changed, going from 0.961 to 1.042. Lastly, we report at the bottom of Panel B the p-value of the over-identification test based on using all countries' shares as separate instruments in a single IV estimation. The results easily reject the null hypothesis of instrument endogeneity.

Table 2: Validation of Empirical Design

Panel A. Rotemberg weights	Rotemberg weight ($\hat{\alpha}_k$)		Just-identified estimate ($\hat{\beta}_k$)
	All countries	Excl. Other	
Other	0.270		0.803
Venezuela	0.158	0.216	1.068
Cuba	0.117	0.161	1.129
Nicaragua	0.095	0.130	1.028
Colombia	0.073	0.100	1.032
Haiti	0.070	0.096	1.138
Mexico	0.067	0.092	0.986
Honduras	0.037	0.050	1.014
Ecuador	0.036	0.049	1.014
Guatemala	0.035	0.048	1.013
Peru	0.029	0.040	1.017
El Salvador	0.012	0.017	1.002

Panel B. Over-ID test	All countries	Excl. Other
Baseline IV estimate ($\hat{\beta}$)	0.961	1.042
Over-identified IV estimate ($\hat{\beta}^{OID}$)	0.913	0.946
Over-identification test p-value	0.67	0.61

Notes: Rotemberg weights are computed following Goldsmith-Pinkham et al. (2020). $\hat{\beta}_k$ is the IV estimate using the pre-existing immigrant shares associated with a given country with leave-out adjustments. $\hat{\beta}_k^{OID}$ is the IV estimate using all shares as instruments.

5.2.3 Comparison with Alternative Empirical Designs

We next compare our two-way leave-out shift-share IV estimates with estimates obtained from two alternative empirical strategies: a two-way leave-out ancestry IV and a conventional shift-share IV without leave-out adjustments, as described in Section 4.2.

Panel B of Appendix Table B3 shows that the estimated employment effect is positive and statistically significant under both approaches: approximately 2.5 using the ancestry IV and 0.7 using the conventional shift-share IV. The wage effect (not shown but available upon request) is significantly negative under the ancestry IV, with an estimated magnitude of -1.3, and is insignificant under the conventional shift-share IV. Overall, these estimates are qualitatively consistent with our preferred IV estimates in Table 1.

However, we prefer the two-way leave-out shift-share IV for three reasons. First, it exhibits substantially higher first-stage relevance, with a (Kleibergen-Paap) F-statistic near 30, compared with an F-statistic of 7 for the ancestry IV and 14 for the conventional shift-share

IV (see third row, panel B of Appendix Table B3). Second, specification tests recommended by Goldsmith-Pinkham et al. (2020) favor our preferred design. In particular, the Rotemberg-weight decomposition shown in panel A of Appendix Table B3 indicates that the ancestry instrument places disproportionate weight on a single origin—Mexico—which accounts for roughly two-thirds of the identifying variation. This suggests that the corresponding IV estimates may be sensitive to potential misspecification or endogeneity specific to the Mexico ancestry component. Third, the just-identified employment effect varies widely depending on the country used for the instrument, suggesting that estimates based on the Ancestry IV may be sensitive to the choice of countries used to construct the instrument.

5.3 Extensions

5.3.1 Employment & Wage Impacts by Industry

We next examine how the employment and wage effects of UIWF vary by industry. Specifically, we estimate equation (1) for employment and wages by NAICS major sector. The dependent variable for the employment regressions is the change in that industry's employment level over the period divided by initial total nonfarm employment. We divide by total nonfarm employment, rather than industry-specific initial employment, due to the presence of outliers in CZ industry-specific employment, which can skew industry-specific growth rates.

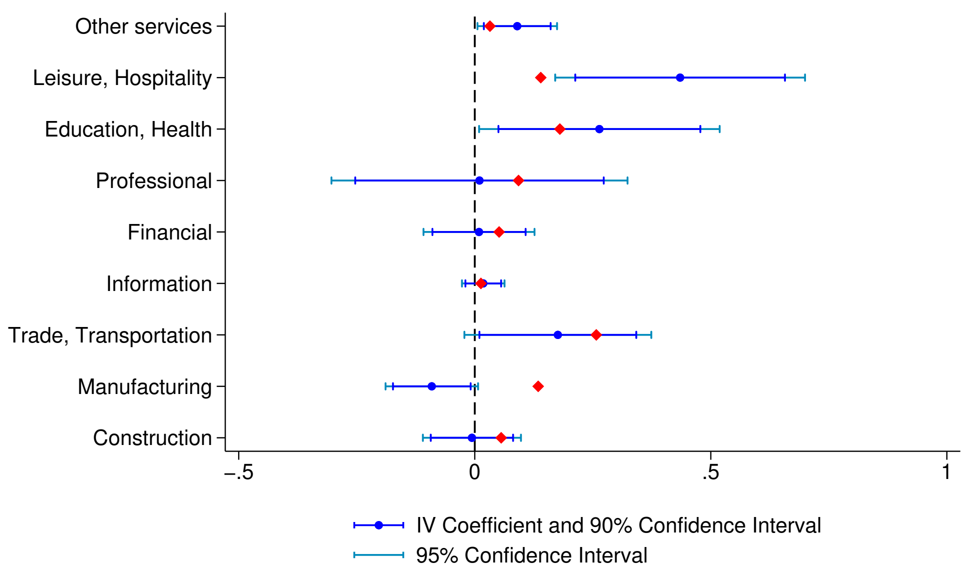
Panel (a) of Figure 7 plots the IV coefficient, along with the 90% and 95% confidence intervals, for industry-specific employment growth. For reference, we also plot each industry's "typical" share of employment, as measured by its 2015-2019 average. If new unauthorized immigrant workers were distributed across industries in the same proportion as all other workers, we would expect the estimated effect of UIWF on industry employment as a share of initial total employment to be approximately equal to this typical share. While the IV estimated effect is statistically significantly positive for three industries (Leisure & Hospitality, Education & Health, and Other Services), the effect is only significantly above the typical share for Leisure & Hospitality. This suggests that, during the boom period, unauthorized immigrant workers disproportionately increased employment in the Leisure & Hospitality sector. We also find a negative effect of UIWF on Manufacturing employment, significantly below the typical manufacturing share.

Panel (b) shows the IV results for industry-specific wage growth. The point estimates are negative for all but one industry, but the effects are statistically insignificant in all cases.

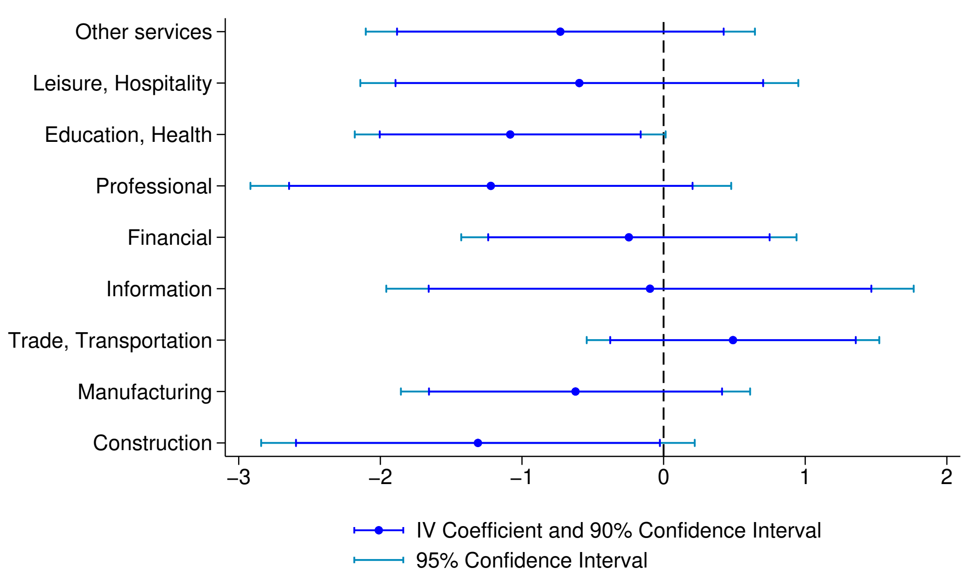
Figure 7: Employment & Wage Effects of UIWF by Industry

Boom Period: 2021m3–2024m3

(a) Employment



(b) Wage



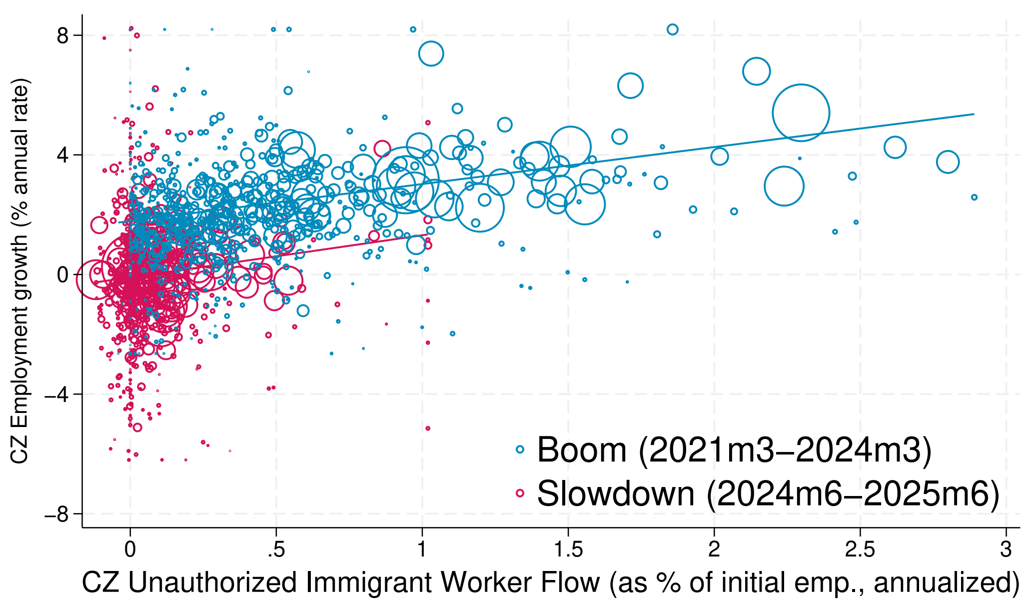
Notes: UIWF (as share of initial employment) is winsorized at 99th percentile due to outliers. Observations are weighted by beginning-of-period total employment. Brackets indicate the 95% confidence intervals based on standard errors that are heteroskedasticity-robust and clustered on state.

5.3.2 Employment & Wage Impacts During the Slowdown Period

We now present results for the more recent period of the national immigration slowdown, which is from June 2024 to June 2025. As shown in Figure 3, this period was characterized by a sharp slowdown in net unauthorized immigration relative to the boom period (March 2021 to March 2024) that we analyzed above.

Figure 8 combines two scatter plots, one for the boom period and one for the slowdown period, of UIWF and employment growth across CZs. The figure reveals two facts. First, UIWF slowed down significantly from the first to the second period, but remained positive on an annual basis for most CZs during the slowdown period. (As shown in Figure 3, national UIWF did not turn negative until around February 2025.) Second, the raw correlations between UIWF and employment growth, as measured by the slope of the OLS linear fit lines shown in the Figure, were very similar across the boom and slowdown periods. Indeed, these slopes are close to one, suggesting a roughly one-to-one relationship between UIWF and employment growth in both periods.

Figure 8: Employment Growth vs. Immigrant Worker Flows Across CZs



Notes: Each circle represents a commuting zone (CZ). The size of the circles are proportional to CZ beginning-of-period employment.

Table 3 shows the OLS and IV estimates of β_1 from estimating equation (1) for the slowdown period for both employment growth and wage growth. Note the OLS estimates differ from the slopes shown in Figure 8 because equation (1) includes control variables. The effects of UIWF turn out to be much more imprecisely estimated for the slowdown period than we found for the boom period, as reflected by the large standard errors in Table 3. The IV point estimate of the effect of UIWF on employment growth is fairly large at 1.6, but it is statistically insignificant. The IV point estimate for wages also is statistically insignificant.

We also estimate the IV effects of UIWF on industry-specific employment and wages during the slowdown period. The results for employment are shown in Figure 9 and those for wages are shown in Appendix Figure B1. The industry-specific effects, although generally imprecisely estimated, point to large positive effects of UIWF on employment growth particularly in Education & Health (significantly different from zero and from its typical employment share at the 10% level), Manufacturing (significant at the 10% level), and Construction. The effects for wages are statistically insignificant for all industries.

Table 3: Effect of Unauthorized Immigrant Worker Flows on Employment & Wages

Slowdown Period: 2024m6-2025m6

	(1) OLS, Emp.	(2) IV, Emp.	(3) OLS, Wages	(4) IV, Wages
UIWF	0.650	1.588	0.195	0.720
	(0.417)	(1.258)	(0.531)	(2.150)
Constant	-0.0686*	-0.0604	0.0887	0.0933*
	(0.0374)	(0.0390)	(0.0553)	(0.0488)
Observations	721	721	720	720
R^2	0.162	0.149	0.096	0.094
First-Stage Results:				
Coefficient Estimate		1.688***		1.688***
Coefficient Std. Error		(0.460)		(0.460)
F Statistic		13.45		13.45

Notes: UIWF (as share of initial employment) is winsorized at 99th percentile due to outliers. Observations are weighted by beginning-of-period employment. Standard errors (shown in parentheses) are heteroskedasticity-robust and clustered on state. Statistical significance is indicated by: * p<0.1, ** p <0.05, *** p < 0.01.

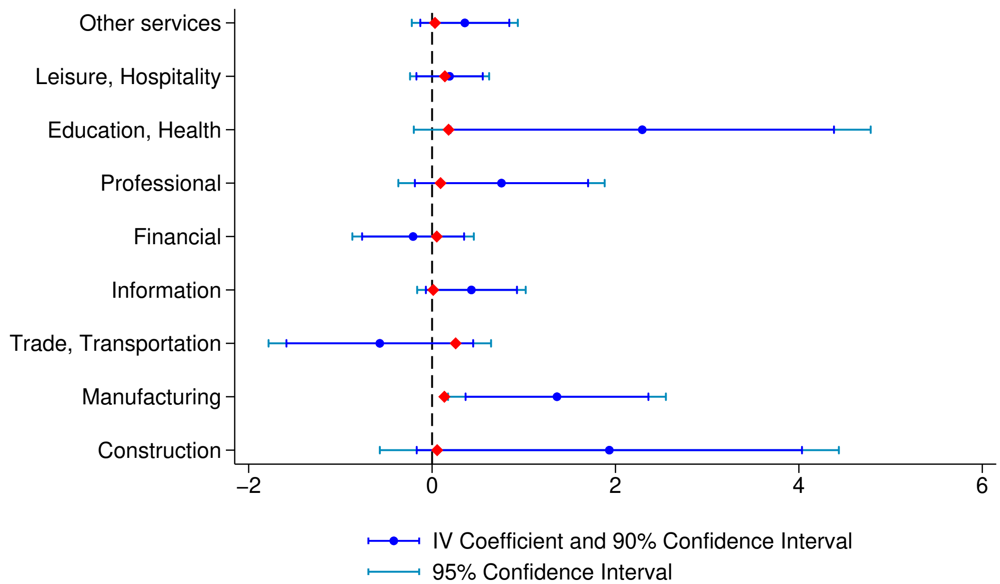
5.3.3 Employment & Wage Impacts Across Expanding-Window Samples

So far we have presented results for two periods, 2021m3–2024m3 and 2024m6–2025m6, characterized by distinctly different national immigration patterns, with the first one a period of large and growing positive net inflows and the second one a period of rapidly slowing but generally positive net inflows.

In Figure 10 we show how the IV estimated employment and wage effects of UIWF change as one expands the initial 2021m3–2024m3 sample period one month (or quarter for wages) at a time, from ending in March 2024 to ending in June 2025. The results for employment are shown in panel (a) while those for wages are shown in panel (b). The estimates shown

Figure 9: IV Estimated Effects of UIWF on Employment by Industry

Slowdown period: 2024m6-2025m6



Notes: UIWF (as share of initial employment) is winsorized at 99th percentile due to outliers. Observations are weighted by beginning-of-period total employment. Brackets indicate the 95% confidence intervals based on standard errors that are heteroskedasticity-robust and clustered on state.

for 2024m3 is the same as those shown in Table 1.

For employment growth, we find that over these expanding window samples, the IV estimated effect of UIWF varies from about 0.5 to nearly 1. For wage growth, the estimates vary from roughly -1 to -2. These results suggest that the employment and wage effects are generally robustly estimated. We note, however, that the QCEW employment and wage data are not seasonally adjusted. National seasonal patterns will be absorbed by the intercept in these regressions, but heterogeneous seasonality across CZs will not and could potentially be correlated with UIWF, especially if unauthorized immigrants disproportionately work in seasonal industries such as construction. Hence, one should view the results for ending months or quarters far away from March or Q1 (the calendar month/quarter of the beginning of our sample in 2021) with caution.

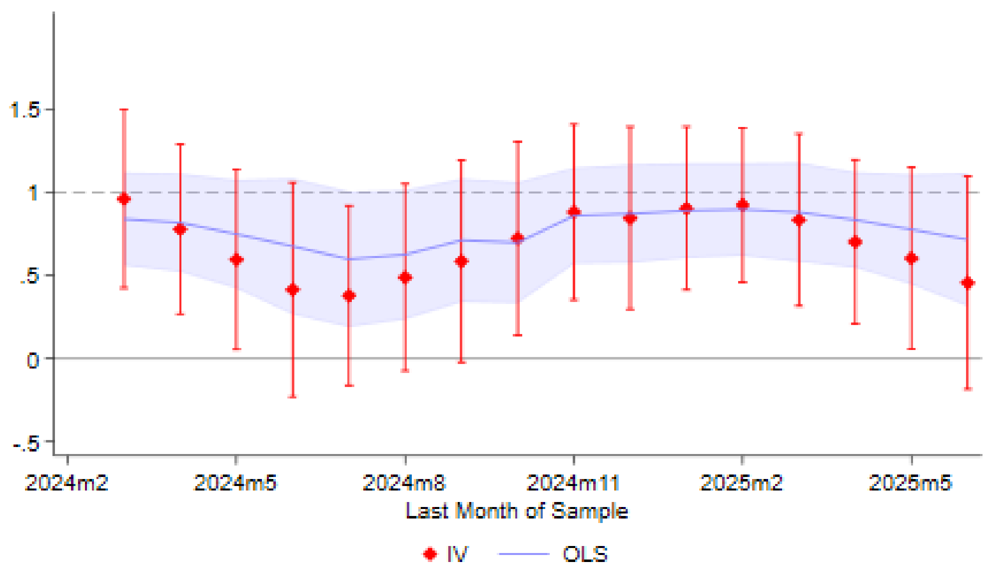
5.3.4 Evidence from the American Community Survey

Many prior studies of immigration's labor market effects have relied on employment and wage data from household surveys such as the ACS. When it comes to studying unauthorized immigration, however, concerns arise that individuals without legal authorization to

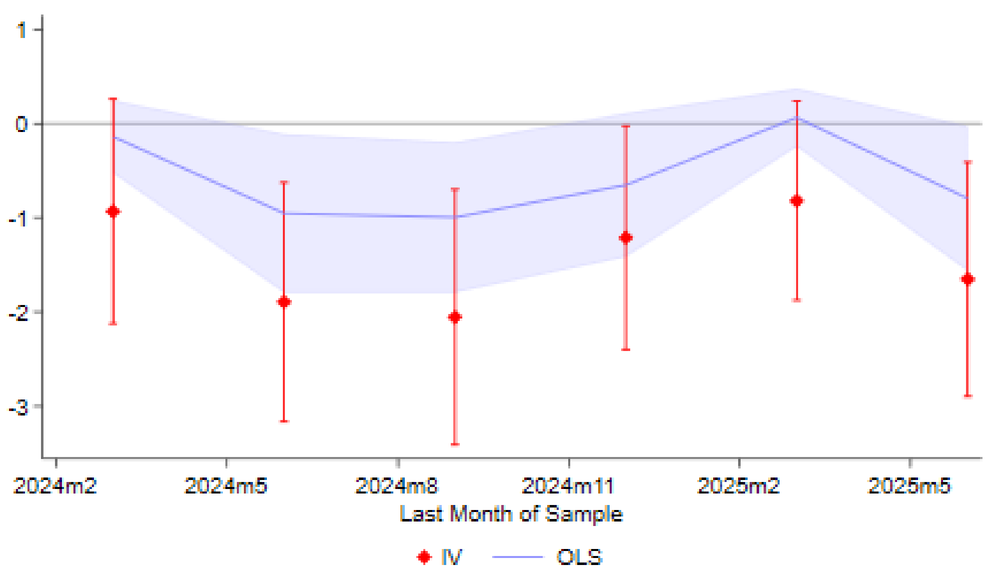
Figure 10: Employment & Wage Impacts Across Expanding-Window Samples

From 2021m3–2024m3 to 2021m3–2025m6

(a) Employment



(b) Wage



Notes: Brackets around point estimates indicate 95% confidence intervals. UIWF (as share of initial employment) is winsorized at 99th percentile due to outliers. Observations are weighted by beginning-of-period employment. Standard errors (shown in parentheses) are heteroskedasticity-robust and clustered on state.

reside and work in the U.S. may be reluctant to participate in government surveys, and that they may not be fully covered by the sampling design of these surveys (Brown et al. 2023). Researchers also have argued that the sample weights for foreign-born vs. native-born individuals in the CPS and ACS since 2021 are likely to be inaccurate, because the boom in unauthorized immigration complicated efforts to measure the overall foreign-born population, especially in real-time (Coglianese et al. 2025). Another limitation of using the ACS data for studying the local effects of recent unauthorized immigration is that the data are only available at an annual frequency, making it impossible to perfectly align ACS outcomes with the boom and slowdown periods identified in our monthly UIWF data.

With these concerns in mind, we nonetheless provide estimates of the effects of UIWF on ACS employment and wages for the 2021-2023 period to facilitate comparison to the prior literature. Specifically, we estimate equation (1) where the dependent variable is employment growth or wage (average hourly earnings) growth from 2021 to 2023.[20] For each of these outcomes, we report results separately for all individuals, foreign-born, foreign-born from high-encounter (HE) countries, native-born, low-education native-born, and high-education native-born. Because the ACS is conducted continuously throughout the year, we use the yearly midpoint for measuring beginning and ending UIWF. That is, we measure UIWF for these regressions as cumulative net entry from June 2021 to June 2023, scaled by initial employment. We estimate these regressions using the same two-way leave-out shift-share IV strategy as used above.

Table 4 presents the results, with the estimated UIWF effects for employment and wages shown in panels (a) and (b), respectively. The IV estimate for total CZ employment is 0.9. Though it is imprecisely estimated and statistically insignificant, the point estimate implies that UIWF equal to 1% of initial employment increases local employment by nearly 1%, in line with our baseline estimate using QCEW data. The effect on foreign-born employment is about 0.75%, and is statistically significant, while the effect on native-born employment is small and insignificant. The result in column (3) shows that the effect on foreign-born employment is driven almost entirely by individuals from high-encounter countries, which are the source of the vast majority of unauthorized immigrant workers during the boom period. *A priori*, we would have expected the impact on foreign-born employment to be closer to one and that on native-born employment to be closer to zero; one cannot statistically reject either hypothesis. Looking at low-education and high-education native-born employment, we find no significant effects.

As shown in panel (b), we find a negative and significant effect on overall CZ wages (av-

[20]Using ACS data, we measure average hourly earnings at the CZ level by dividing total wage income by total hours worked among wage earners in a given year.

Table 4: IV Estimated Effects of Unauthorized Immigrant Worker Flows on ACS Outcomes 2021–2023

(a) Employment

	(1) CZ Total	(2) Foreign-Born	(3) HE Immigrants	(4) Native-Born	(5) Low-Ed Native	(6) High-Ed Native
UIWF	0.924	0.749**	0.698***	0.175	0.168	0.00770
	(0.800)	(0.355)	(0.247)	(0.854)	(0.388)	(0.533)
Constant	-0.0372	-0.0936**	-0.0434*	0.0565	0.170***	-0.114
	(0.166)	(0.0364)	(0.0224)	(0.147)	(0.0562)	(0.119)
Observations	712	712	708	712	712	712
R^2	0.045	0.319	0.112	0.041	0.031	0.049
First-Stage Results:						
Coefficient Estimate	0.488***	0.488***	0.486***	0.488***	0.488***	0.488***
Coefficient Std. Error	(0.086)	(0.086)	(0.085)	(0.086)	(0.086)	(0.086)
F Statistic	32.18	32.18	31.95	32.18	32.18	32.18

(b) Average Hourly Earnings

	(1) CZ Total	(2) Foreign-Born	(3) HE Immigrants	(4) Native-Born	(5) Low-Ed Native	(6) High-Ed Native
UIWF	-0.873**	1.894	0.398	-0.691*	-0.928	-0.757*
	(0.344)	(1.530)	(2.465)	(0.378)	(0.677)	(0.443)
Constant	-0.105	0.824*	0.144	-0.178*	0.111	-0.244**
	(0.0909)	(0.484)	(0.524)	(0.0958)	(0.138)	(0.105)
Observations	712	712	710	712	712	712
R^2	0.096	0.006	0.023	0.114	0.111	0.089
First-Stage Results:						
Coefficient Estimate	0.488***	0.488***	0.488***	0.488***	0.488***	0.488***
Coefficient Std. Error	(0.086)	(0.086)	(0.086)	(0.086)	(0.086)	(0.086)
F Statistic	32.18	32.18	32.11	32.18	32.18	32.18

Notes: UIWF (as share of initial employment) is winsorized at 99th percentile due to outliers. Observations are weighted by beginning-of-period employment. Standard errors (shown in parentheses) are heteroskedasticity-robust and clustered on state. Statistical significance is indicated by: * p<0.1, ** p <0.05, *** p < 0.01.

erage hourly earnings). Specifically, the coefficient of -0.87 implies that an increase of UIWF equal to 1% of initial employment reduces wages by roughly 0.9%. The effect on foreign-born workers' wages is positive but statistically significant, while the effect on native-born workers' wages is negatively and weakly significant, with a coefficient of about -0.69. The estimated effects on low-education and high-education native-born wages are of a similar magnitude. The coefficient on high-education native-born worker wages is weakly statistically significant, while that on low-education native wages is slightly larger but statistically insignificant.

In sum, we find that the roughly one-for-one effect of UIWF on local employment and negative but imprecisely estimated effect on local wages, which we found using the QCEW data, are broadly robust to using the ACS data.[21]

5.3.5 Back-of-the-Envelope Implied UIWF Effects in Boom Period

So far, we have focused on the effects of a one–percentage-point increase in UIWF relative to initial employment. From a policy perspective, however, it is important to quantify the total effect of the UIWF boom and its contribution to local economic outcomes. We consider the implied effects for both a "typical" local area, as given by the median CZ, as well as for the average area, as given by the weighted-mean of CZs. Table 5 presents the results from this exercise, with the results for employment provided in the first row.[22] (We discuss the housing results in Section 6.1.)

Table 5: Contribution of UIWF to Key Outcomes, Boom Period: 2021m3-2024m3

	Median CZ/MSA (unweighted)			Average CZ/MSA (weighted)		
	Implied Effect %, (1)	Total Change %, (2)	Contribution p.p., (1)/(2)	Implied Effect %, (4)	Total Change %, (5)	Contribution p.p., (4)/(5)
Employment	0.8	5.6	14.6	2.6	8.7	30.1
House Prices	2.9	22.3	13.1	6.6	22.4	29.6
Market Rents	1.9	22.3	8.6	4.3	21.6	20.1

Notes: The implied effects are calculated by multiplying the baseline IV estimates (at the CZ level for employment and at the MSA level for house prices and rents) by the corresponding cumulative increase in UIWF (as a share of initial employment) over the boom period, March 2021 to March 2024.

Between March 2021 and March 2024, the median commuting zone experienced cumulative unauthorized immigrant worker flows of 0.8% relative to its March 2021 employment level. Multiplying our baseline IV estimate of 0.961 by 0.8 implies that for the typical CZ, UIWF boosted nonfarm employment by about 0.8%. This UIWF employment effect can account for nearly 15% of the total increase in the median CZ's employment over this period. Thus, for the typical local labor market, because the magnitude of UIWF was relatively modest, its importance for explaining overall employment growth was also modest.

However, as shown in the last three columns, UIWF played a more substantial role in explaining employment growth for the *average* labor market, which is considerably bigger

[21] We also provide industry-specific results (i.e., IV estimates of UIWF effects) for employment growth in Appendix Figure B2. The results are qualitatively consistent with those based on QCEW industry employment (for the boom period). In particular, as with the QCEW results, the only industry with a significant effect above what would be predicted based on employment shares is Leisure & Hospitality.

[22] We do not perform these calculations for wages because our estimated marginal effect of UIWF on wages is statistically insignificant.

than the median. The average labor market experienced a larger boom in UIWF as a share of local employment, because unauthorized immigrant workers disproportionately settled in urban areas. The cross-CZ weighted-mean of UIWF (as a share of initial employment) over this period—which also is equivalent to the national UIWF—was 2.7%. Multiplying this increase by our baseline IV estimate of 0.961 yields an implied UIWF employment effect of 2.6%. Given that total employment change for the weighted-mean CZ (and the nation) was 8.7%, these calculations imply that UIWF can account for roughly 30% of the average CZ's employment growth over this period.[23]

6 Impacts on the Broader Local Economy

In this section, we consider the impacts of unauthorized immigration on the local economy beyond the labor market over the immigration boom period. We first examine the housing market, focusing on the impacts on house prices and rents, as well as the supply of new housing (6.1). We then turn to the impacts on local personal income (6.2). Lastly, motivated by the public policy debates regarding immigration's effects on government welfare program spending, we look within the personal income category specifically at government transfers.

6.1 Housing Market

Here we estimate the impact of UIWF on local shelter prices—both house prices and market rents—and on new housing supply. All data used in this subsection is MSA-level. For shelter price growth, we provide results based on a few different measures. Our preferred measures are Zillow's MSA-level house-price index and rent index. To assess robustness and to facilitate comparison to prior studies, we also present results based on house price indexes from CoreLogic and Freddie Mac, as well as self-reported home values and rents from the ACS. We measure new housing supply using the ratio of new housing permits issued over the boom period of 2021m3-2024m3 to the initial stock of existing housing units (see detailed housing data description in Section 3.2).

Before discussing the regression results, we present simple scatter plots to visualize the key correlations. Panel (a) of Figure 11 shows the relationship between UIWF and house price growth across MSAs for the boom period. Each MSA's circle is proportional to the MSA's initial employment size. There is a clear positive relationship. In panel (b), the

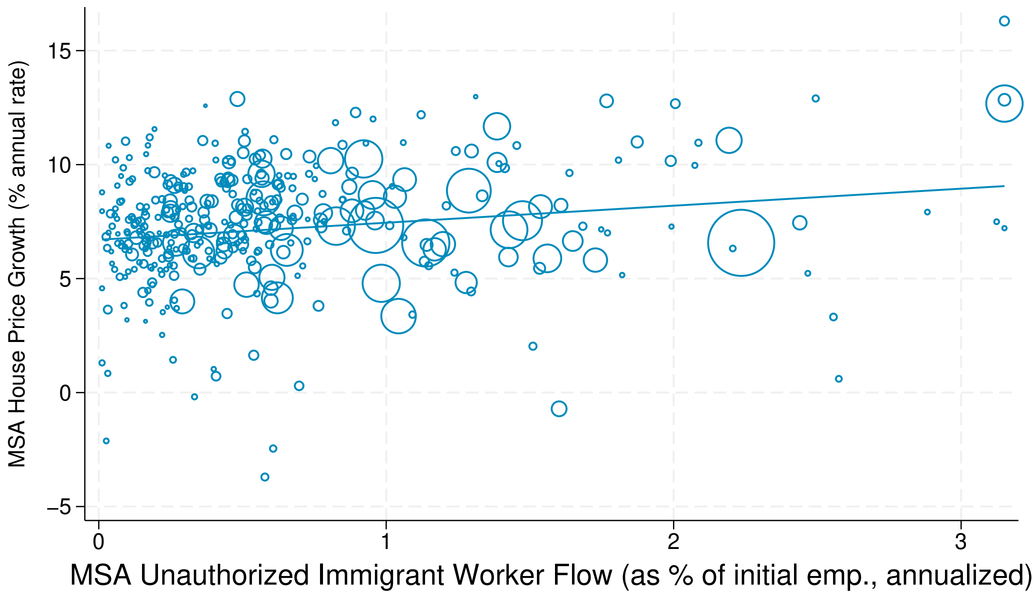
[23] We caution that, although the weighted-mean of UIWF as a share of initial employment is equivalent to the national aggregate of UIWF as a share of initial employment, the employment (and other) effects of UIWF for the weighted-mean CZ do not necessarily equal the national effects. National effects of UIWF will additionally reflect cross-CZ spillovers and general equilibrium effects beyond local markets.

raw relationship between UIWF and new housing supply during the boom period is much cloudier, with no clear positive or negative relationship.

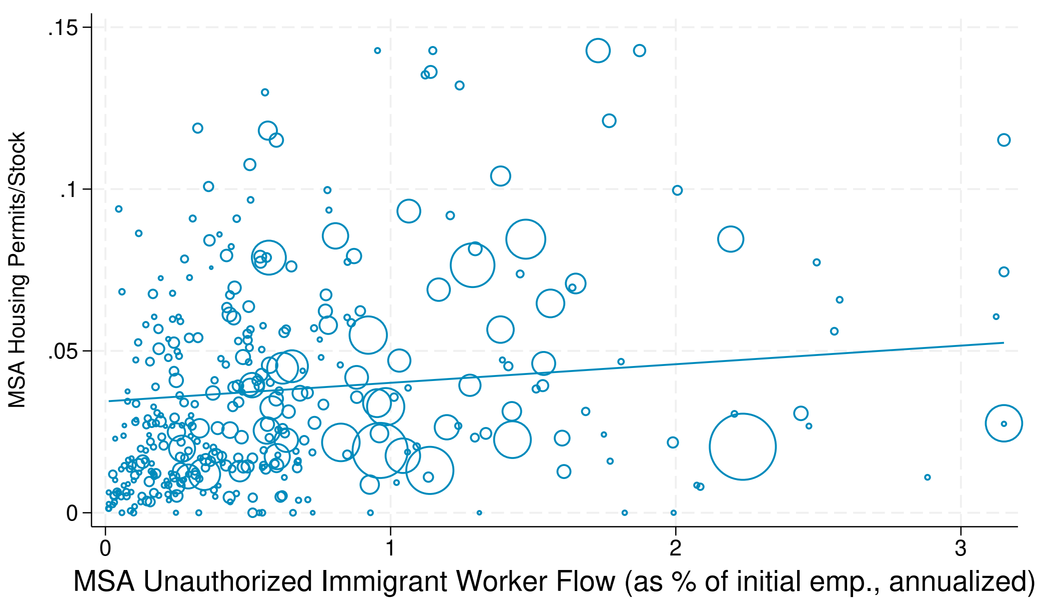
Figure 11: MSA-Level Housing Market Outcomes

Boom Period: 2021m3–2024m3

(a) House Price Growth



(b) Housing Supply



Notes: Each circle represents an MSA. The size of the circles are proportional to MSA beginning-of-period employment.

Turning to the regression results shown in Table 6, we find strong evidence from both OLS and IV estimates that UIWF had a positive and statistically significant effect on house prices and rents. The IV estimates imply that an increase in UIWF equal to 1% of initial employment raises local house prices by 2.2% and market rents by 1.4%. Across rent categories, the effect is somewhat smaller, about 1.2%, for single-family units and slightly larger, about 1.5%, for multi-family units. Results using the CoreLogic and Freddie Mac house price indexes are reported in Appendix Table B4. The evidence is more mixed with these alternative house price measures: both show positive effects of roughly 1%, but with larger standard errors.

As for new housing supply, we find a significant effect from OLS for multi-family units, but the IV regressions point to small and statistically insignificant effects across all three measures of housing supply. Taken together, these results, along with the subdued construction-sector employment effects in both QCEW data (Figure 7) and ACS data (Appendix Figure B2), suggest that during the boom period, UIWF acted primarily as a housing demand shock in an environment of relatively fixed short-run housing supply.

To quantify the contribution of UIWF to the overall increases in local house prices and rents between early 2021 and early 2024, the last two rows of Table 5 apply our baseline IV estimates to the cumulative increase in UIWF (as a percent of initial employment) over this period. As we did for employment in Section 5.3.5, we perform this exercise for both a "typical" local area, as given by the median MSA, and for the average area, as given by the weighted-mean of MSAs. The median MSA's UIWF (as a share of initial employment) during this period was approximately 1.3%. Multiplying this value by our IV estimate for house prices (2.2) from Table 6 and by our IV estimate for rents (1.4) yields implied effects of UIWF on house prices and rents of 2.9% and 1.9%, respectively. These effects amount to relatively modest contributions to the median MSA's total increases in house prices and rents over this period.

Yet, for the *average* MSA, UIWF's contributions to growth in house prices and rents are much larger. The weighted mean of UIWF as a share of initial employment during the boom period was 3.1%. Multiplying this increase by the baseline IV estimates yields an implied effect of UIWF on house prices of 6.6% and on rents of 4.3%. The total weighted-mean increases in house prices and rents over this period were 22.4% and 22.6%, respectively. Putting these together, for the average MSA, UIWF can explain approximately 30% of the total increase in house prices and 20% of the total increase in rents.

Table 6: Effect of Unauthorized Immigrant Worker Flows on Housing

Boom Period: 2021m3–2024m3

(a) Shelter Prices – OLS

	(1) Zillow House Price (HP)	(2) Zillow Rent	(3) Zillow Rent-SF	(4) Zillow Rent-MF
UIWF	1.140**	1.583***	1.133***	1.608***
	(0.487)	(0.176)	(0.294)	(0.186)
Observations	348	280	234	217
R^2	0.368	0.536	0.506	0.534

(b) Shelter Prices – IV

	(1) Zillow House Price	(2) Zillow Rent	(3) Zillow Rent-SF	(4) Zillow Rent-MF
UIWF	2.189***	1.438***	1.229**	1.470***
	(0.741)	(0.344)	(0.529)	(0.287)
Observations	348	280	234	217
R^2	0.312	0.535	0.505	0.533
First-Stage Results:				
Coefficient Estimate	0.793***	0.795***	0.781***	0.784***
Coefficient Std. Error	(0.212)	(0.215)	(0.214)	(0.210)
F Statistic	14.02	13.64	13.25	13.92

(c) New Housing Supply – OLS

	(1) Total	(2) Single-Family	(3) Multi-Family
UIWF	0.152	0.0318	0.126***
	(0.120)	(0.0946)	(0.0379)
Observations	343	343	343
R^2	0.500	0.514	0.519

(d) New Housing Supply – IV

	(1) Total	(2) Single-Family	(3) Multi-Family
UIWF	-0.208	-0.227	0.00998
	(0.213)	(0.170)	(0.0650)
Observations	343	343	343
R^2	0.466	0.479	0.499
First-Stage Results:			
Coefficient Estimate	0.793***	0.793***	0.793***
Coefficient Std. Error	(0.211)	(0.211)	(0.211)
F Statistic	14.05	14.05	14.05

Notes: New housing supply measured as cumulative permits over period divided by initial housing units. UIWF (as share of initial employment) is winsorized at 99th percentile due to outliers. Observations are weighted by beginning-of-period employment. Standard errors (shown in parentheses) are heteroskedasticity-robust and clustered on state. Statistical significance is indicated by: * p<0.1, ** p <0.05, *** p < 0.01.

While our paper is the first to our knowledge to provide results for *unauthorized* immigration, it is interesting to compare our estimates of house price and rent effects to those from prior studies of legal (lawful permanent resident) immigration. The seminal paper by Saiz (2007) uses data from 1985 to 1998 on immigration from the U.S. Immigration and Naturalization Service, on rents constructed by the Department of Housing and Urban Development based on the census, AHS and CPI samples, and on house prices from Freddie Mac. He provided results using both OLS and IV, with a shift-share instrument design similar to Card (2001). With OLS, he found that an inflow of legal immigration equal to 1% of a city's population was associated with increases in average rents of about 1% and in house prices of about 2%. Using IV, the rent elasticity remained about 1 but the house price elasticity increased to a little over 3.

A recent paper by Cabral and Steingress (2026) follows a similar approach to Saiz (2007) but with some key differences. First, they consider a longer period, 1980 to 2019. Second, they measure immigration using foreign-born individuals aged 25 or older who entered the U.S. within the past five years, based on data from the decennial census and ACS. This measure could potentially include some unauthorized immigrants but likely be dominated by lawful permanent residents. Third, they measure house prices (and rents) using median self-reported values from the census and ACS.[24] Fourth, they follow the ancestry-based IV approach of Burchardi et al. (2019) to achieve identification. They find somewhat larger effects on rents relative to Saiz (2007), with an elasticity of around 2, and similar effects on house prices, with elasticities in the range of 2 to 3.

Our estimates using market price-based measures are broadly in line with these findings. Since both prior studies rely on self-reported rents and home prices, to facilitate the comparison, we repeat our OLS and IV regressions using ACS self-reported prices. We do this at the CZ level to allow for a greater number of observations. The results are shown in Appendix Table B5. In contrast to the effects found for legal immigration in these prior studies and to our preferred estimates using market price-based measures, we find that unauthorized immigration generally had no significant effect on self-reported house prices or rents. However, given the potential for measurement error, subjective valuation, and the inability of self-reported prices to control for housing characteristics and compositional changes, we interpret results based on survey data with caution.

[24]These self-reported values may have substantial, and possibly non-classical, measurement error for two reasons. First, they do not control for changes in housing quality/composition. Second, while renters likely can accurately their current rent, many homeowners that have occupied a house for more than 1-2 years may not have a good idea of the current market value of their home.

6.2 Personal Income and Government Transfers

Next, we examine the impacts of UIWF on local personal income and its two key components—labor income and government transfers—over 2021-2023.[25] Columns (1)-(4) of Table 7 present the results for personal income, labor income and their per capita terms. While the OLS coefficient for personal income growth is economically and statistically significant (about a 0.6% increase), the IV coefficient is smaller and insignificant.

For the labor income component, the OLS coefficient is weakly positive indicating a roughly 0.4% increase, while the IV coefficient is negative and insignificant. The IV estimate of per capita labor income growth is negative and significant. A likely explanation for this negative effect is that unauthorized immigrant workers earn lower wages than native-born workers, as they tend to be low-skilled and work in lower-paying occupations and industries, thus bringing down average labor income in the local area.

The lack of a significant boost from UIWF on total local income, despite the one-for-one increase in employment, raises the interesting question of how UIWF can boost local housing prices without boosting local income. One plausible explanation stems from non-homothetic preferences and housing being a necessity good, combined with housing supply being fixed in the short-run.[26] In this case, an increase in UIWF leads to a composition shift toward lower income households who have higher housing expenditure shares. Thus, even with a modest or no increase in total local income, housing demand will increase, pushing up rents in the short-run as housing supply is fixed.

Lastly, we examine the question of whether unauthorized immigrant workers lead to an increase in demand for government services and hence increase government transfers in the local area. Recent research by Camarota and Zeigler (2026) using the U.S. Survey of Income Program Participation (SIPP) data found that in 2024 59% of households headed by a non-citizen received benefits from at least one major government welfare program, compared with 37% for U.S.-born households. However, these survey data measure only the extensive margin of program utilization, not the dollar amount of the government transfers. In addition, the survey data are unable to distinguish between legal and unauthorized immigrants, nor between immigrants heading households containing U.S. citizen children or others.[27]

[25]We use 2023 as the end year for our baseline results for the sake of consistency with the time period used in our analysis of ACS annual outcomes and because end-of-year 2023 is close to the March 2024 end period used in our analyses using QCEW monthly data. However, we find very similar results for the period 2021 to 2024 (see Appendix Table B6)

[26]Non-homothetic preferences and housing as a necessity explain the fact, documented in Albouy et al. (2016) and Dustmann et al. (2022), that expenditures shares on housing fall with income.

[27]Camarota and Zeigler (2026) do provide a probabilistic estimate of welfare program utilization for unau-

Table 7: Effects of Unauthorized Immigrant Worker Flows on Personal Income

Boom Period: 2021–2023

(a) OLS

	(1) Pers. Inc.	(2) Pers Inc p.c.	(3) Labor Inc.	(4) Labor Inc. p.c.	(5) Gov. Transfers	(6) Govt. Transfers p.c.
UIWF	0.595*	0.104	0.448*	-0.0431	-1.018**	-1.509***
	(0.322)	(0.213)	(0.246)	(0.169)	(0.484)	(0.391)
Constant	0.143	0.287***	0.00600	0.151	0.410***	0.555***
	(0.104)	(0.0962)	(0.129)	(0.147)	(0.134)	(0.137)
Observations	712	712	712	712	712	712
R^2	0.510	0.434	0.377	0.235	0.414	0.462

(b) IV

	(1) Pers. Inc.	(2) Pers. Inc. p.c.	(3) Labor Inc.	(4) Labor Inc. p.c.	(5) Gov. Transfers	(6) Govt. Transfers p.c.
UIWF	0.341	-0.141	-0.670	-1.151**	-4.495***	-4.977***
	(0.992)	(0.737)	(0.711)	(0.503)	(1.388)	(1.324)
Constant	0.122	0.267**	-0.0842	0.0612	0.129	0.275*
	(0.153)	(0.124)	(0.173)	(0.162)	(0.171)	(0.162)
Observations	712	712	712	712	712	712
R^2	0.508	0.431	0.322	0.153	0.066	0.114
First-Stage Results:						
Coefficient Estimate	0.548***	0.548***	0.548***	0.548***	0.548***	0.548***
Coefficient Std. Error	(0.083)	(0.083)	(0.083)	(0.083)	(0.083)	(0.083)
F Statistic	43.30	43.30	43.30	43.30	43.30	43.30

Notes: UIWF (as share of initial employment) is winsorized at 99th percentile due to outliers. Observations are weighted by beginning-of-period employment. Standard errors (shown in parentheses) are heteroskedasticity-robust and clustered on state. Statistical significance is indicated by: * p<0.1, ** p <0.05, *** p < 0.01.

Our results on unauthorized immigrant worker flows strongly reject the hypothesis that such immigration leads to higher transfers. Indeed, based on our IV regressions, we find a statistically significant *negative* effect of UIWF on government transfers to local residents, both in total and per capita. For instance, the IV coefficient implies that increased UIWF equal to 1% of initial employment lowers government transfer payments for the average resident in the commuting zone by nearly 5%. This negative effect could be due to a couple reasons. First, unauthorized immigrant workers, by our measure, are working-age. Working-age individuals are less likely to utilize government services related to education and health

thorized immigrant households based on the self-reported characteristics of foreign-born survey participants; that estimate is 61%, very close to the 59% utilization rate for all non-citizen households.

care than the general population. Second, due to their potentially ambiguous legal status, such individuals may be less likely than the general population to receive unemployment insurance payments and social safety net payments such as Temporary Assistance for Needy Families (TANF), Supplemental Nutrition Assistance Program (SNAP; "food stamps"), and Medicaid.[28]

7 Conclusion

From early 2021 to early 2024, the U.S. experienced a dramatic surge in immigration that was unprecedented in scale, unusual in composition (dominated by unauthorized rather than legal permanent entries), and marked by substantial heterogeneity across local labor markets. The subsequent rapid decline was likewise unprecedented. These tremendous swings have fostered much interest and debate in economic and policy circles about how unauthorized immigration affects local economies. To date, studies addressing these questions have largely relied on aggregate data, extrapolated pre-pandemic estimates, or structural model simulations. In addition, a scarcity of research on the effects of unauthorized immigration have left policymakers to rely on past studies on legal or total immigrations, the effects of which could be quite different than those of unauthorized immigration.

Our paper provides the first systematic empirical assessment of the local labor- and housing-market effects of post-pandemic unauthorized immigration. We combine newly available administrative microdata that help measure net unauthorized immigration at the national and local levels with a rigorous cross-sectional empirical strategy that exploits exogenous variation across local markets. The identification strategy utilizes a version of the immigration network shift-share instrument used in several past studies of immigration.

On labor market effects, we find that unauthorized immigrant worker flows increased local employment approximately one-for-one during the 2021-2024 surge, without generating significant declines in local wages. Focusing on the subsequent slowdown period of mid-2024 to mid-2025, we document qualitatively similar effects, albeit with larger standard errors reflecting reduced cross-sectional variation in immigration. These employment and wage responses are robust to alternative empirical strategies, geographical aggregations, and survey-based outcome measures. Thus, the unauthorized immigration boom acted as a

[28] We note that we also find a negative and significant effect of UIWF on transfers for the 2021 to 2024 period, as shown in Appendix Table B6. However, the coefficient is smaller at about -2.0. One possible explanation is that newly arrived immigrants, while not utilizing government programs at first, increasingly utilize these services over time as they become settled in their communities. They may also bring in other family members over time that utilize government services.

supply shock to local labor markets as a whole. In contrast, we find that unauthorized immigrant worker flows increased local house prices and rents, without significantly expanding new housing supply. Thus, the unauthorized immigration boom acted as a housing demand shock, boosting prices in the face of relatively inelastic short-run housing supply.

In an extension, we look at the effect of UIWF on some key components of personal income. While we find modest and statistically insignificant effects on overall personal income (in total or per capita), we see significantly negative effects on per capita labor income. The decline in per capita labor income is consistent with unauthorized immigrants earning lower wages on average than native-born workers. This compositional shift in the local workforce pulls down average labor income.

Lastly, we estimate that UIWF significantly reduces local income received from government transfers, both in levels and per capita. This negative effect on government transfers is consistent with both higher employment growth and lower utilization of social safety net programs such as unemployment insurance, food assistance, and Medicaid services among newly arrived unauthorized immigrant workers.

Overall, our findings provide new evidence on the local labor-market, housing-market and fiscal effects of large-scale unauthorized immigration shocks to the U.S. economy, which are unprecedented in the modern era, and offer a data-driven approach for evaluating their economic implications.

References

Albouy, D., G. Ehrlich, and Y. Liu (2016). Housing demand, cost-of-living inequality, and the affordability crisis. NBER Working Paper 22816.

Altonji, J. G. and D. Card (1991). The effects of immigration on the labor market outcomes of less-skilled natives. In *Immigration, Trade, and the Labor Market*. University of Chicago Press.

Aramayo, A., K. Y. Santamaria, and H. R. Smith (2024). The biden administration's june 2024 proclamation and rule, securing the border. Congressional Research Service LSB11178.

Baker, B. and R. Warren (2024). Estimates of the unauthorized immigrant population residing in the united states: January 2018–january 2022. Office of Homeland Security Statistics, U.S. Department of Homeland Security.

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

Borusyak, K., P. Hull, and X. Jaravel (2025). A practical guide to shift-share instruments. *Journal of Economic Perspectives 39*(1), 181–204.

Brown, J. D., M. L. Heggeness, and M. Murray-Close (2023). Noncitizen coverage and its effects on u.s. population statistics. Center for Economic Studies Working Paper CES-23-42, U.S. Census Bureau.

Burchardi, K. B., T. Chaney, and T. A. Hassan (2019). Migrants, ancestors, and foreign investments. *Review of Economic Studies 86*(4), 1448–1486.

Caballero, M. E., B. C. Cadena, and B. K. Kovak (2023). The international transmission of local economic shocks through migrant networks. *Journal of international economics 145*, 103832.

Cabral, J. and W. Steingress (2026). Immigration and us shelter prices: The role of geographical and immigrant heterogeneity. *European Economic Review 182*.

Caiumi, A. and G. Peri (2024). Immigration's effect on us wages and employment redux. NBER Working Paper 32389.

Camarota, S. A. and K. Zeigler (2026). Welfare use by immigrants and the us-born, 2024. Center for Immigration Studies Report.

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.

Card, D. (2009). Immigration and inequality. *American Economic Review 99*(2), 1–21.

Chen, S. S.-H., X. Huang, and W. Li (2025). Beyond the hype: Unauthorized immigrants and the myth of rising house price. *Finance Research Letters 75*, 106885.

Cheremukhin, A., S. Hur, R. Mau, K. Mertens, A. W. Richter, and X. Zhou (2025). The postpandemic u.s. immigration surge: New facts and inflationary implications. Federal Reserve Bank of Dallas Working Paper 2407.

Clemens, M. A., E. G. Lewis, and H. M. Postel (2018). Immigration restrictions as active labor market policy: Evidence from the mexican bracero exclusion. *American Economic Review 108*(6), 1468–1487.

Coglianese, J. M., S. Murray, and C. J. Nekarda (2025). Harmonized population and labor force statistics. Finance and Economics Discussion Series 2025-057, Board of Governors of the Federal Reserve System.

Congressional Budget Office (2024). The demographic outlook: 2024 to 2054. Report, January 18, 2024.

Congressional Budget Office (2026). The demographic outlook: 2026 to 2056. Report, January 7, 2026.

Cravino, J., A. A. Levchenko, F. Ortega, and N. Pandalai-Nayar (2026). The economic impact of mass deportations. NBER Working Paper 34790.

Department of Homeland Security (2003). Estimates of the unauthorized immigrant population residing in the united states: 1990 to 2000. Office of Policy and Planning, U.S. Immigration and Naturalization Service.

Dustmann, C., B. Fitzenberger, and M. Zimmermann (2022). Housing expenditure and income inequality. *The Economic Journal 132*(645), 1709–1736.

Dustmann, C., S. Otten, U. Schönberg, and J. Stuhler (2025). The effects of immigration on places and people—identification and interpretation. *Forthcoming: Journal of Labor Economics*.

Dustmann, C., U. Schönberg, and J. Stuhler (2017). Labor supply shocks, native wages, and the adjustment of local employment. *Quarterly Journal of Economics 132*(1), 435–483.

Duzhak, E. and A. New-Schmidt (2025). Immigration and changes in labor force demographics. *FRBSF Economic Letter*, Federal Reserve Bank of San Francisco.

East, C. N., A. L. Hines, P. Luck, H. Mansour, and A. Velásquez (2023). The labor market effects of immigration enforcement. *Journal of Labor Economics 41*(4), 957–996.

Edelberg, W., S. Veuger, and T. Watson (2025). Immigration policy and its macroeconomic effects in the second trump administration. American Enterprise Institute Economic Perspectives.

Galaasen, S., A. R. Kostøl, J. Monras, and J. Vogel (2025). The labor supply curve is upward sloping: The effects of immigrant-induced demand shocks. NBER Working Paper 33930.

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

Howard, T., M. Wang, and D. Zhang (2024). Cracking down, pricing up: Housing supply in the wake of mass deportation. Working Paper.

Jaeger, D. A., J. Ruist, and J. Stuhler (2019). Shift-share instruments and dynamic adjustments: The case of immigration. NBER Working Paper 24285.

Krogstad, J. M., J. S. Passel, and D. Cohn (2019). 5 facts about illegal immigration in the u.s. Pew Research Center.

Monras, J. (2020). Immigration and wage dynamics: Evidence from the mexican peso crisis. *Journal of Political Economy 128*(8), 3017–3089.

Orrenius, P., G. Ozor, M. Zavodny, and X. Zhou (2025). Declining immigration weighs on gdp growth, with little impact on inflation. *Dallas Fed Economics*, Federal Reserve Bank of Dallas.

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

Peri, G. and C. Sparber (2009). Task specialization, immigration, and wages. *American Economic Journal: Applied Economics 1*(3), 135–169.

Saiz, A. (2007). Immigration and housing rents in american cities. *Journal of Urban Economics 61*(2), 345–371.

Terry, S. J., T. Chaney, K. B. Burchardi, L. Tarquinio, and T. A. Hassan (2026). Immigration, innovation, and growth. *American Economic Review 116*(3), 828–861.

Wilson, D. J. and X. Zhou (2026). New data show intensifying unauthorized immigration decline, with large local variations. *Dallas Fed Economics*, Federal Reserve Bank of Dallas.

Online Appendix

A Construction of Net Unauthorized Immigration

Entry categories. As shown in panel (a) of Figure A1, we distinguish three entry categories: (i) NTA-based inflows, constructed from the NTA microdata; (ii) parole-based entries, constructed from individual-level parole records at and between ports of entry (see panel (c) of Figure A1); and (iii) got-aways, linearly interpolated from DHS annual estimates and adjusted to exclude "likely got-aways" identified in the NTA microdata.

Exit categories. As shown in panel (b) of Figure A1, we distinguish four exit categories: (i) individuals ordered removed or granted voluntary departure by immigration courts; (ii) individuals newly booked into detention while awaiting immigration court proceedings; (iii) voluntary exits of individuals who entered under major parole programs; and (iv) voluntary exits of other parolees and got-aways.

Construction of voluntary exits. We construct voluntary exits as follows. For exit category (iii), we rely on DHS announcements regarding the termination of major parole programs and country-specific Temporary Protected Status (TPS) designations to estimate voluntary departures among Ukrainians, Cubans, Haitians, Nicaraguans, and Venezuelans.

Specifically, we assume zero voluntary exits for Ukrainian, Cuban, and Haitian parolees during our sample period. As of July 2025, the Uniting for Ukraine (U4U) program remained in effect, Haiti's TPS designation had not expired, and Cuban parolees retain a pathway to lawful permanent residence under the Cuban Adjustment Act of 1966. In contrast, the Nicaraguan and Venezuelan parole programs and their respective TPS designations terminated at different points in 2025. We therefore assume that 50% of Nicaraguan and Venezuelan parolees exited the U.S. following the termination of these programs (using the later of the parole or TPS end date), with departures distributed evenly over a three-month window.

For exit category (iv), we proceed in two steps. First, we compute a monthly NTA-based exit rate, defined as total monthly exits observed in 2025 divided by cumulative NTA-based entries during 2021–2024. Variation in this rate reflects the timing and scale of prior inflows. We then apply one-half of this rate to the total entries of non-CHNV, non-Ukrainian parolees and got-aways during 2021–2024 to estimate voluntary exits for this group. We scale the rate by one-half because, unlike NTA-based entrants, these individuals are not required to undergo immigration court proceedings and therefore have weaker exit incentives.

Construction of local net unauthorized immigration. We first decompose national net unauthorized immigration N_t into NTA categories (with superscript NTA) and remaining categories, i.e., paroles and got-aways (with superscript R):

$$N_t = I_t - O_t = \underbrace{I_t^{NTA} + I_t^R}_{\text{inflows}} - \underbrace{\left(O_t^{NTA} + O_t^R\right)}_{\text{outflows}}.$$

This equality holds across origin countries (indexed by o) and U.S. destination units (indexed by d):

$$N_t = \sum_o \sum_d \underbrace{(I_{o,d,t} - O_{o,d,t})}_{N_{o,d,t}} = \sum_o \sum_d \left(I_{o,d,t}^{NTA} + I_{o,d,t}^R - O_{o,d,t}^{NTA} - O_{o,d,t}^R\right).$$

We can directly measure $I_{o,d,t}^{NTA}$ and $O_{o,d,t}^{NTA}$ using our NTA microdata, as well as any origin-specific aggregate, such as $I_{o,\cdot,t} := \sum_d I_{o,d,t}$.

- **Inflows**: Let $S_{o,t} = \frac{I_{o,\cdot,t}^{NTA}}{I_{o,\cdot,t}}$ be the origin-specific share of NTA inflows of aggregate unauthorized inflows from country o. We measure inflows from origin o to destination d as

$$I_{o,d,t} = \frac{I_{o,d,t}^{NTA}}{S_{o,t}} = I_{o,d,t}^{NTA} \times \frac{I_{o,\cdot,t}}{I_{o,\cdot,t}^{NTA}}.$$

  Using origin-specific shares helps capture variation across countries. For example, this share is high for Mexican migrants but relatively low for Haitian migrants who had the parole option.

- **Outflows**: At the national level, we can write outflows as

$$O_t = \begin{cases} O_t^{NTA} & t \leq 2024m12, \\ O_t^{NTA} + E_t\left(\sum_{\tau \in 2021-2024} I_\tau^R\right) & t > 2024m12, \end{cases}$$

  where the term in parenthesis is the cumulative number of non-NTA entries during 2021-2024. E_t is the effective monthly voluntary exit rate in 2025, which can be computed using non-NTA exists constructed earlier.

  Analogous to the national outflows, let $E_{o,t}$ be the origin-specific effective monthly voluntary exit rate, which can be computed using our data. We construct outflows from origin o and geographic unit d as

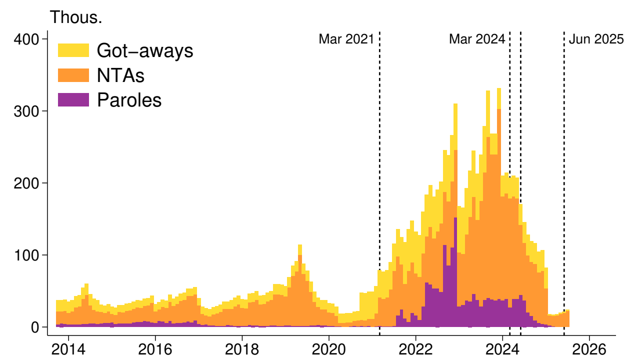
$$O_{o,d,t} = \begin{cases} O_{o,d,t}^{NTA} & t \leq 2024m12, \\ O_{o,d,t}^{NTA} + E_{o,t} \sum_{\tau \in 2021-2024}\left(I_{o,d,\tau} - I_{o,d,\tau}^{NTA}\right) & t > 2024m12. \end{cases}$$

The term in parenthesis can be computed using the previously constructed inflows.

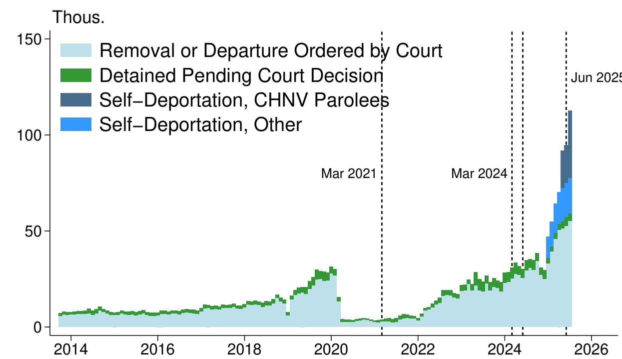
- **Local net unauthorized immigration:** Taken together, $N_{.,d,t} = \sum_o (I_{o,d,t} - O_{o,d,t})$. This approach ensures that the sum of net unauthorized immigration, as well as its components, across all locations matches the national total and origin-specific net flows.

Figure A1: Monthly Net Unauthorized Immigration

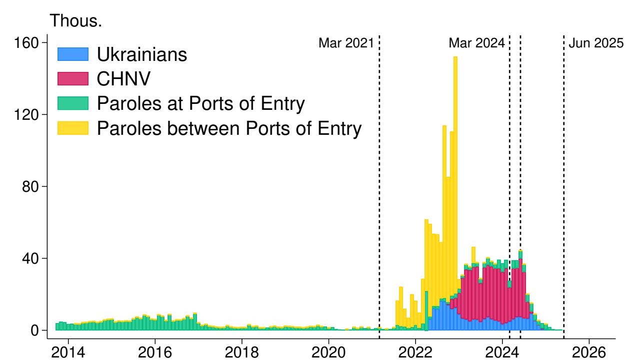
(a) Entry Categories



(b) Exit Categories

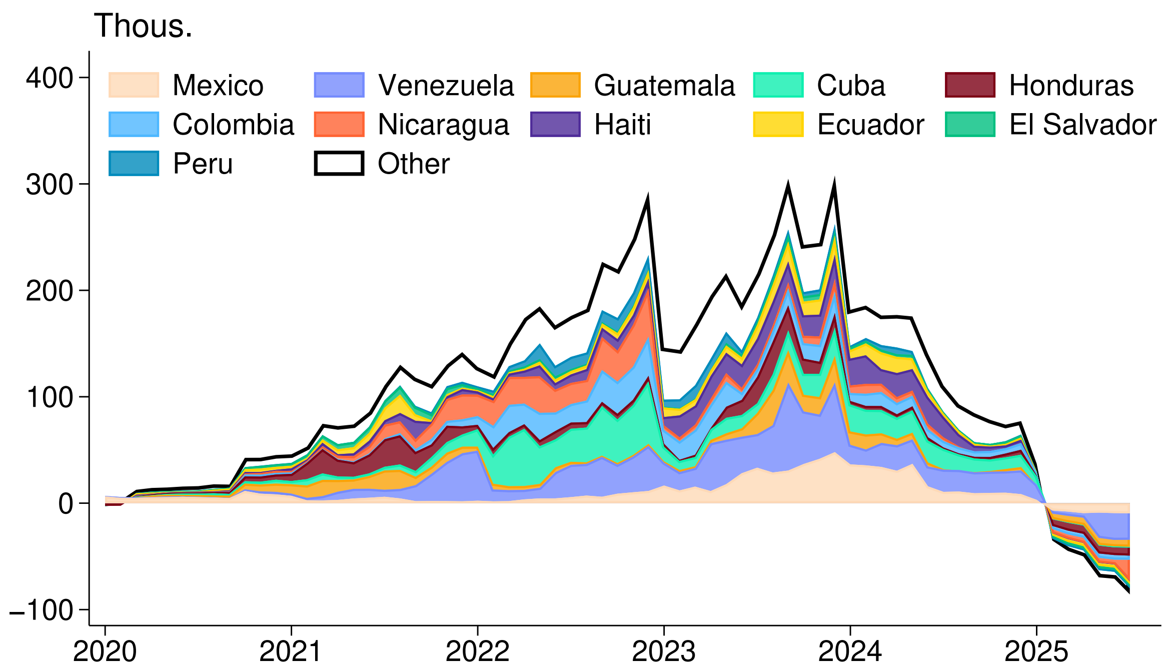


(c) Parole Categories



Notes: All categories of entry and exit are estimated using individual-level administrative data, except for got-aways, which are from DHS annual aggregate estimates. See Section 3.3 for the construction of these categories.

Figure A2: Net Unauthorized Immigration By Country



Notes: Country-specific net unauthorized immigration is constructed similarly as the national aggregate.

B Additional Results

B.1 Employment & Wage Results at MSA-level

Table B1: Effect of Unauthorized Immigrant Worker Flows on Employment & Wages

MSA Level

Boom Period: 2021m3–2024m3

	(1) OLS, Emp.	(2) IV, Emp.	(3) OLS, Wages	(4) IV, Wages
UIWF	0.784***	0.954***	0.154	-0.169
	(0.139)	(0.252)	(0.338)	(0.818)
Constant	-0.0351	0.00830	0.368*	0.284
	(0.127)	(0.128)	(0.189)	(0.198)
Observations	348	348	347	347
R^2	0.597	0.592	0.594	0.583
First-Stage Results:				
Coefficient Estimate		0.793***		0.794***
Coefficient Std. Error		(0.212)		(0.211)
F Statistic		14.02		14.07

Notes: UIWF (as share of initial employment) is winsorized at 99th percentile due to outliers. Observations are weighted by beginning-of-period employment. Standard errors (shown in parentheses) are heteroskedasticity-robust and clustered on state. Statistical significance is indicated by: * p<0.1, ** p <0.05, *** p < 0.01.

B.2 All and All Working-Age Unauthorized Immigrant Flows

Table B2: IV Estimates of Labor Market Impacts for Boom Period: 2021m3–2024m3

(a) *All Working-Age* Unauthorized Immigrant Flows

	(1) OLS, Emp.	(2) IV, Emp.	(3) OLS, Wages	(4) IV, Wages
d_imm_wa	0.588***	0.650***	-0.0982	-0.625
	(0.100)	(0.191)	(0.138)	(0.432)
Constant	-0.130	-0.111	0.0877	-0.0716
	(0.113)	(0.123)	(0.121)	(0.194)
Observations	721	721	720	720
R^2	0.581	0.580	0.636	0.585
First-Stage Results:				
Coefficient Estimate		1.166***		1.166***
Coefficient Std. Error		(0.224)		(0.224)
F Statistic		27.10		27.10

(b) *All* Unauthorized Immigrant Flows

	(1) OLS, Emp.	(2) IV, Emp.	(3) OLS, Wages	(4) IV, Wages
d_imm	0.470***	0.507***	-0.0660	-0.501
	(0.0804)	(0.149)	(0.106)	(0.358)
Constant	-0.121	-0.106	0.0911	-0.0823
	(0.114)	(0.123)	(0.120)	(0.208)
Observations	721	721	720	720
R^2	0.583	0.582	0.636	0.580
First-Stage Results:				
Coefficient Estimate		1.583***		1.583***
Coefficient Std. Error		(0.294)		(0.294)
F Statistic		28.84		28.85

Notes: Unauthorized immigrant flows (as share of initial employment) is winsorized at 99th percentile due to outliers. Observations are weighted by beginning-of-period employment. Standard errors (shown in parentheses) are heteroskedasticity-robust and clustered on state. Statistical significance is indicated by: * p<0.1, ** p <0.05, *** p < 0.01.

B.3 Specification Tests for Alternative Empirical Designs

Table B3: Rotemberg Weights and Over-ID Test

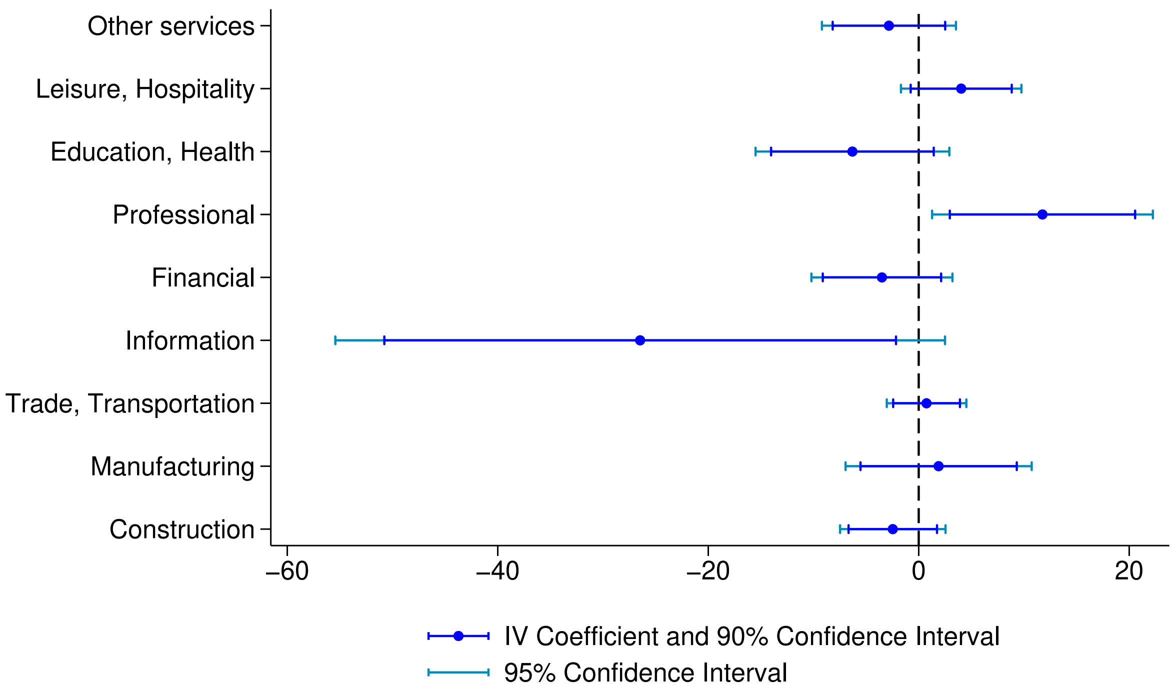
Panel A. Rotemberg weights	Leave-Out Ancestry IV		Conventional Shift-Share IV	
	Rotemberg $\hat{\alpha}_k$	Just-identified $\hat{\beta}_k$	Rotemberg $\hat{\alpha}_k$	Just-identified $\hat{\beta}_k$
Mexico	0.674	2.553	0.011	1.135
Cuba	0.193	2.034	0.241	0.238
Guatemala	0.044	2.518	0.013	0.782
Colombia	0.035	2.490	0.106	0.933
Haiti	0.020	2.629	0.166	0.773
Honduras	0.019	2.544	0.020	0.882
El Salvador	0.015	2.446	-0.000	1.079
Peru	0.009	2.878	0.023	0.830
Venezuela	0.002	13.577	0.230	0.681
Ecuador	-0.000	-31.944	0.054	1.607
Nicaragua	-0.008	1.054	0.085	-0.029
Other			0.052	1.290
Panel B. Over-ID test				
Full IV estimate ($\hat{\beta}$)		2.499***		0.651**
$Se(\hat{\beta})$		(0.822)		(0.260)
First-Stage F		6.80		14.09
Over-identified IV estimate ($\hat{\beta}^{OID}$)		1.484***		1.100***
$Se(\hat{\beta}^{OID})$		(0.272)		(0.221)
Over-identification test p-value		0.23		0.69

Notes: Rotemberg weights computed as in Goldsmith-Pinkham et al. (2020). The leave-out ancestry IV specification follows Terry et al. (2026) as described in Section 4.1. The conventional shift-share approach follows the Card (2001) instrument without leave-out adjustments.

B.4 Industry-Specific Wage Results for Slowdown Period

Figure B1: IV Estimated Effects of UIWF on Wages by Industry

Slowdown period: 2024m6–2025m6

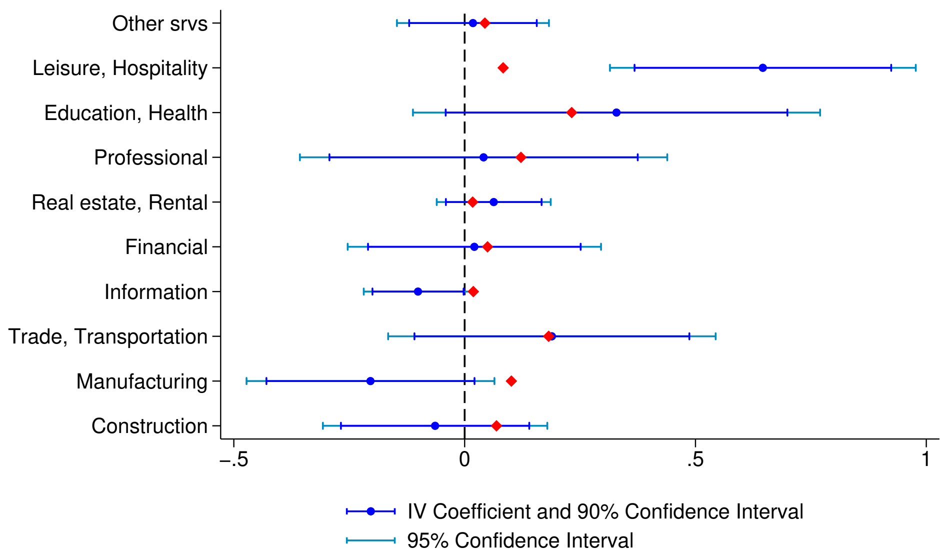


B.5 Industry-Specific Employment and Wage Effects for the Boom Period Using the ACS

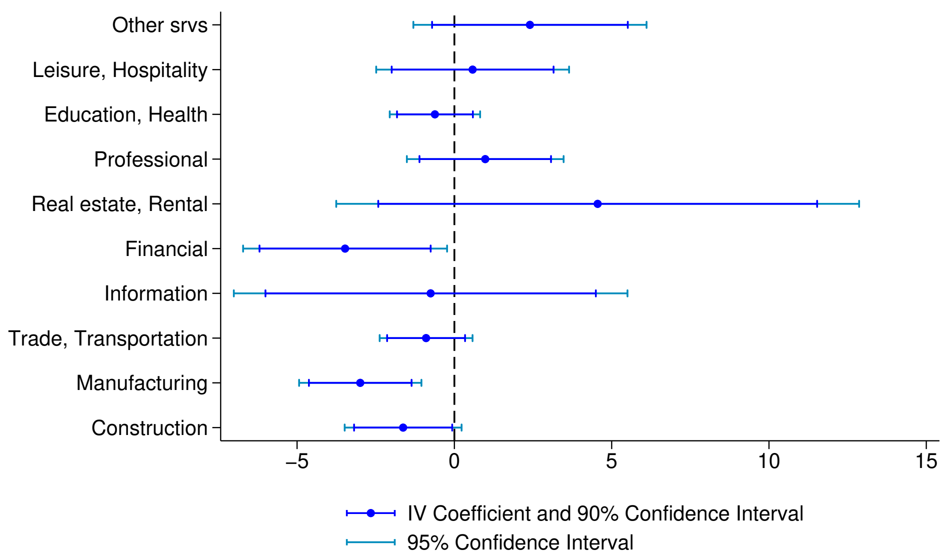
Figure B2: IV Estimated Employment and Wage Impacts by Industry Using ACS

2021–2023

(a) Employment



(b) Average Hourly Earnings



B.6 House Price Results Using CoreLogic and Freddie Mac House Price Indexes

Table B4: Effect of Unauthorized Immigrant Worker Flows on House Prices

Boom Period: 2021m3–2024m3

(a) Shelter Prices – OLS

	(1) CoreLogic HP	(2) Freddie Mac HP
UIWF	0.612	0.970*
	(0.509)	(0.500)
Observations	335	343
R^2	0.333	0.465

(b) Shelter Prices – IV

	(1) CoreLogic HP	(2) Freddie Mac HP
UIWF	0.632	1.182
	(1.094)	(0.817)
Observations	335	343
R^2	0.333	0.463
First-Stage Results:		
Coefficient Estimate	0.501***	0.793***
Coefficient Std. Error	(0.131)	(0.211)
F Statistic	14.60	14.05

Notes: New housing supply measured as cumulative permits over period divided by initial housing units. UIWF (as share of initial employment) is winsorized at 99th percentile due to outliers. Observations are weighted by beginning-of-period employment. Standard errors (shown in parentheses) are heteroskedasticity-robust and clustered on state. Statistical significance is indicated by: * p<0.1, ** p <0.05, *** p < 0.01.

B.7 Housing Results Using Annual ACS Data

Table B5: Effect of Unauthorized Immigrant Worker Flows on Housing Prices

Boom Period (2021–2023) – Commuting Zone Level

Growth in Annual ACS Median & Mean House Price and Rent

(a) OLS

	(1) Median House Price	(2) Mean House Price	(3) Median Rent	(4) Mean Rent
UIWF	-0.0387	0.527	0.496	0.413
	(0.381)	(0.491)	(0.342)	(0.257)
Observations	712	712	712	712
R^2	0.432	0.342	0.268	0.270

(b) IV

	(1) Median House Price	(2) Mean House Price	(3) Median Rent	(4) Mean Rent
UIWF	-2.227	-1.068	0.734	0.674
	(1.827)	(1.461)	(1.567)	(1.449)
Observations	712	712	712	712
R^2	0.353	0.298	0.267	0.268
First-Stage Results:				
Coefficient Estimate	0.491***	0.491***	0.491***	0.491***
Coefficient Std. Error	(0.080)	(0.080)	(0.080)	(0.080)
F Statistic	37.72	37.72	37.72	37.72

Notes: UIWF (as share of initial employment) is winsorized at 99th percentile due to outliers. Observations are weighted by beginning-of-period employment. Standard errors (shown in parentheses) are heteroskedasticity-robust and clustered on state. Statistical significance is indicated by: * p<0.1, ** p <0.05, *** p < 0.01.

B.8 Effects on Personal Income and Subcomponents for 2021-2024

Table B6: Effects of Unauthorized Immigrant Worker Flows on Personal Income

Boom Period: 2021–2024

(a) OLS

	(1) Pers. Inc.	(2) Pers Inc p.c.	(3) Labor Inc.	(4) Labor Inc. p.c.	(5) Gov. Transfers	(6) Govt. Transfers p.c.
UIWF	0.520**	0.00313	0.435**	-0.0816	-0.506	-1.023***
	(0.218)	(0.131)	(0.210)	(0.146)	(0.311)	(0.258)
Constant	0.180	0.370***	0.0695	0.259	0.580***	0.770***
	(0.123)	(0.126)	(0.142)	(0.160)	(0.195)	(0.214)
Observations	712	712	712	712	712	712
R^2	0.521	0.381	0.411	0.270	0.383	0.433

(b) IV

	(1) Pers. Inc.	(2) Pers. Inc. p.c.	(3) Labor Inc.	(4) Labor Inc. p.c.	(5) Gov. Transfers	(6) Govt. Transfers p.c.
UIWF	0.413	-0.127	-0.563	-1.103***	-1.423**	-1.963**
	(0.575)	(0.398)	(0.548)	(0.330)	(0.677)	(0.803)
Constant	0.166	0.353***	-0.0627	0.124	0.459**	0.645***
	(0.165)	(0.136)	(0.189)	(0.174)	(0.210)	(0.222)
Observations	712	712	712	712	712	712
R^2	0.520	0.378	0.342	0.159	0.334	0.383
First-Stage Results:						
Coefficient Estimate	0.552***	0.552***	0.552***	0.552***	0.552***	0.552***
Coefficient Std. Error	(0.086)	(0.086)	(0.086)	(0.086)	(0.086)	(0.086)
F Statistic	40.40	40.40	40.40	40.40	40.40	40.40

Notes: UIWF (as share of initial employment) is winsorized at 99th percentile due to outliers. Observations are weighted by beginning-of-period employment. Standard errors (shown in parentheses) are heteroskedasticity-robust and clustered on state. Statistical significance is indicated by: * p<0.1, ** p <0.05, *** p < 0.01.