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Administrative Microdata**

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

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Abstract

From early 2021 through early 2024, the U.S. experienced an unprecedented boom in unauthorized immigration, with substantial heterogeneity across local economies. Using administrative microdata, we construct new measures of national and local unauthorized immigration flows and exploit exogenous variation using multiple empirical designs. Unauthorized immigrant worker flows increased local employment approximately one-for-one, with mixed evidence of wage depression. They also raised house prices and rents without expanding housing supply. Finally, these flows reduced labor income per capita and government transfers. These findings help inform debates surrounding how unauthorized immigration impacts labor and housing markets and public finances in the U.S.

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

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

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1 Introduction

Between early 2021 and early 2024, the U.S. experienced an enormous increase in the number of individuals entering the country without prior authorization.¹ This increase in “unauthorized immigration,” and its subsequent rapid decline, have been unprecedented in modern times. According to the Congressional Budget Office (CBO), net entry of this category of immigration added roughly 7 million people to the U.S. population over 2021–2024, or about 1.75 million per year—nearly double the rate of legal permanent immigration over the same period.² This episode marked a sharp break from the preceding two decades, when unauthorized immigration averaged only 100,000 per year and was slightly negative during the 2010s. Based on the administrative microdata measures we construct in this paper, net unauthorized immigration peaked in January 2024, declined steeply thereafter, and turned negative by February 2025 (see Figure 2).

We study how this large and sudden immigration shock affected local labor markets, housing markets, and other economic outcomes. The question is central to current policy debates as well as the general understanding of immigration’s economic effects, but direct evidence remains limited. Prior research on this episode (i) largely relied on aggregate data to measure unauthorized immigration after 2020, and (ii) inferred its economic effects from pre-pandemic estimates or through model-based simulations.

More broadly, the empirical immigration literature has focused mainly on legal or total immigration, leaving scant evidence on the effects of unauthorized immigration. The economic effects of such immigration on local markets may be quite distinct. For instance, it is unclear *a priori* how well unauthorized immigrant workers are captured in official data measuring employment and wages. Moreover, such workers may have different spillovers on incumbent workers from authorized immigrants, leading to different impacts on local labor markets. The effects of unauthorized immigration on house prices and rents could also differ from those found for other immigration given its unique characteristics. 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.

Using variation across local labor market areas, we provide the first systematic empirical assessment of the economic effects of the recent boom in unauthorized immigration.³ We use

¹We follow the general convention of referring to this type of immigration as “unauthorized.” The term refers to an individual’s immigration status at entry. Many individuals subsequently obtained authorization to remain in the country, to work, or both (Foote 2024). Eligibility for work authorization, however, is legally distinct from formal admission under U.S. immigration law; our definition is based on the latter. We discuss these institutional features further in Section 3.

²More generally, net immigration of lawful permanent residents averaged 0.8 million per year since 2000, never exceeding 900,000 (Congressional Budget Office, 2026).

³Two subsequent working papers, released well after the initial draft of this paper (Wilson and Zhou 2026a), also 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, we use two alternative instrumental-variable (IV) designs. The first follows the standard approach in the literature to construct an “enclave” shift-share (ESS) instrument that interacts national immigration inflows from an origin with the pre-existing share of immigrants from that origin residing in a U.S. destination, and aggregates these interactions across origins (Card 2001, 2009).

Recent studies, however, have highlighted the concern with this IV design that realized settlement shares may themselves reflect persistent origin-destination-specific sorting, creating a spurious relationship between predicted immigration inflows and subsequent local outcomes (Burchardi et al. 2019; Terry et al. 2026). To address this concern, we propose a second, alternative IV design. We adapt the push-pull identification approach developed in Burchardi et al. (2019) and Terry et al. (2026) to our setting and construct a measure of predicted settlement exposure that does not rely on the full variation in realized settlement shares. Specifically, we use the pre-existing geographic distribution of immigrants from other origins to measure a destination’s general attractiveness to immigrants, and allow each origin’s settlement pattern to load differently on this generic destination pull. We then interact the resulting predicted settlement exposure with national immigration inflows with leave-one-out adjustments (excluding the destination’s own inflow from each national shift), and aggregate across origins. We refer to this instrument as the general pull shift-share (GPSS) instrument. We show that this empirical design passes validation tests recommended in the recent microeconometrics literature (e.g., Goldsmith-Pinkham et al., 2020; Borusyak et al., 2025) and delivers strong first-stage relevance.

Using each of these IV approaches, we estimate the causal effects of unauthorized immigration on a range of local economic outcomes, focusing mainly on the immigration boom period from March 2021 to March 2024. To measure these outcomes, we rely primarily on administrative and market-based data, including employment and average weekly wages from the Quarterly Census of Employment and Wages (QCEW), house prices and rents from Zillow, building permits from

immigration court data and shift-share designs to examine the local economic effects of the same immigration episode (Clemens et al. 2026 on labor market outcomes; Hunt et al. 2026 on labor and housing outcomes). Despite some differences in identifying assumptions and outcome variables, and narrower coverage of unauthorized immigration, their results are broadly consistent with corresponding estimates presented here and in Wilson and Zhou (2026a).

the Census Bureau, and BEA income and transfer data. These sources are especially useful in this setting, because household survey data tend to undercount unauthorized immigrants—and hence local employment—and can be noisy at disaggregated geographic levels ([Brown et al. 2023](#); [Coglianese et al. 2025](#)). That said, we complement the QCEW employment and wage results with estimates from the American Community Survey (ACS). The ACS allows us to examine average hourly earnings, which are a more direct measure of wages than weekly earnings (as the latter also reflect hours worked), and to study effects separately for native- and foreign-born individuals and by education. Our analysis uses observations at the CZ or MSA level, depending on the outcome.

We start by examining the impacts of UIWF on labor market outcomes. We reach three main findings. First, an increase in UIWF equal to 1% of initial employment increases local employment by approximately 1%. This one-for-one effect is highly statistically significant and holds under OLS and both IV approaches. It is also robust to alternative specifications, including controlling for each CZ’s domestic migration and work-from-home intensity. Second, while the results on wages tend to be imprecise and not always consistent across data sources and wage measures, the preponderance of evidence generally points to a range of zero to modestly negative effects from UIWF. Third, we examine effects by industry and find particularly large employment effects in Leisure & Hospitality, an industry in which recent unauthorized immigrants are likely to be strongly represented. By contrast, the wage results are mixed: the IV point estimates are generally negative, but only weakly significant for two industries (Construction and Education & Health).

We assess the validity of our empirical designs using several diagnostics, focusing on employment effects. First, pre-period placebo regressions yield no statistically significant evidence of pre-trends. Second, following [Goldsmith-Pinkham et al. \(2020\)](#), we compute Rotemberg weights and just-identified IV estimates associated with each country’s immigration share component. For the GPSS IV, no individual country accounts for more than 15% of the identifying variation, and the just-identified estimates are uniformly close to the baseline estimate of approximately one. The ESS diagnostics are somewhat more concerning: three countries (Venezuela, Haiti, and Cuba) collectively account for 78% of the total Rotemberg weight, indicating heavy reliance on the identifying variation associated with these origins, while the just-identified estimates vary widely across origins. Overidentification tests using the origin-specific components as separate instruments do not reject the joint orthogonality restrictions under either design. Nevertheless, these diagnostic and potential identification concerns about the ESS IV, together with its sensitivity to outliers documented in Section 4, support our preference for the GPSS IV design.

We then turn to the effects of unauthorized immigration on local housing markets. The estimates are generally consistent across identification strategies and robust to controlling for a variety of factors, including domestic migration and work-from-home intensity. Focusing on the GPSS IV

estimates, we highlight three main findings. First, an increase in UIWF equal to 1% of initial employment raises local house prices by 2.2% and local rents by 1.4%. Multiplying these marginal effect estimates by the size of the local UIWF shock implies that UIWF increased house prices and rents for the *average* metropolitan area by 6.7% and 4.2%, respectively, accounting for roughly 30% of its total house price growth and 20% of its rent growth over the boom period. Similar back-of-the-envelope calculations for the *median* MSA yield smaller contributions, about 13% for house price growth and 8% for rent growth, reflecting its smaller UIWF shock.

Second, our IV estimates indicate small and insignificant effects on housing supply, measured by new building permits issued relative to the initial stock of housing units. Third, we find that the positive effects of UIWF on shelter prices are driven by housing markets with above-median population density, where housing supply is likely to be less elastic in the short run. These housing market results, along with the muted construction-sector employment effects we document with both QCEW and ACS data, suggest that unauthorized immigration acted primarily as a housing demand shock in the presence of relatively fixed short-run housing supply.

Finally, we examine broader local economic outcomes using our preferred GPSS IV design. While UIWF had positive but insignificant effects on total personal income, it reduced labor income per capita significantly, consistent with compositional changes associated with the entry of lower-earning workers. Government transfers also decline in both total and per capita terms: an increase in UIWF equal to 1% of initial employment reduces total transfers received by local residents by 4.5% and transfers per capita by 5%. These effects are consistent with the employment gains we find, which may reduce eligibility and need for safety net programs such as unemployment insurance, food assistance, and Medicaid. We note, however, that the ESS IV estimates for labor income and government transfers are less conclusive and sensitive to the exact specification choices.

Taken together, our findings provide insights on two central margins through which local economies adjust to unauthorized immigration. To complement this discussion, we also present a parsimonious theoretical framework in Appendix A that highlights the potential structural channels underlying our empirical results. We review the related literature in Section 2; here, we emphasize how the results map into the main economic mechanisms more generally.

First, the one-for-one employment response sheds light on the extent of crowd-out in local labor markets. In a simple labor-supply framework, an inflow of immigrant workers can reduce employment or wages of similarly skilled native workers when the two groups are close substitutes. But local labor markets may also adjust through imperfect substitution, task reallocation, capital adjustment, and local demand effects. Our estimate is closer to the latter interpretation, implying little native employment displacement in the short run.

Second, the housing market responses show that labor-market absorption need not imply muted

effects in other markets. The same immigrant inflows that expand local employment also raise local housing demand. When housing supply is inelastic, this shock raises rents and house prices. Unauthorized immigration could, in principle, dampen this pressure if new arrivals live in higher-occupancy units or work disproportionately in the construction sector. Our estimates suggest that, over the relatively short horizon we study, the demand channel dominated: rents and house prices rose, while building permits and construction employment did not respond significantly.

This contrast also highlights the value of measuring unauthorized immigration directly. Unauthorized immigrants may differ from legal immigrants in labor-market opportunities, housing consumption, program eligibility, and geographic settlement patterns. Standard survey data such as the CPS and ACS typically do not distinguish between authorized and unauthorized status, and aggregate national estimates provide limited local variation. Our administrative microdata allow us to measure the relevant local shock at high frequency and to estimate its effects directly.

The remainder of the paper proceeds as follows. Section 2 discusses the related literature. Section 3 describes the institutional background, data, and construction of our national and local measures of unauthorized immigrant worker flows. Section 4 presents the empirical strategy. Section 5 estimates the effects of unauthorized immigrant worker flows on local labor market outcomes and assesses the validity and robustness of the identification strategy. Section 6 examines broader local economic outcomes, including house prices, rents, housing supply, personal income, and government transfers. Section 7 concludes.

2 Literature

Our paper contributes to research on the economic effects of immigration along several dimensions. First, it adds to the local labor markets literature by estimating the employment and wage effects of a large, recent shock in the U.S. driven by unauthorized immigration. Second, it builds on the shift-share and immigrant-network literature by using the general pull shift-share design in addition to the conventional enclave-based shift-share. Third, it contributes to work on unauthorized immigration by measuring these flows directly in administrative microdata. Fourth, it connects the labor-market outcomes, housing-market outcomes and broader local economic conditions by estimating their responses in a unified empirical framework. Lastly, it provides evidence-based results for important ongoing debates related to recent swings in immigration policy. That said, we stress that this episode was unique, given that many unauthorized immigrants were permitted to stay and work in the U.S. while their status was adjudicated, and hence the economic effects of UIWF during this period may not be representative of UIWF's effects in other settings.

A large literature studies the effects of immigration on local labor markets. Classic studies exploiting variation across geographies and immigrant settlement networks generally find modest

adverse effects, if any, on native labor-market outcomes (Card 1990; Altonji and Card 1991; Card 2001). A related set of papers emphasizes adjustment margins that can attenuate crowding out, including imperfect substitution between immigrants and natives, task specialization, capital adjustment, and local demand effects (Peri and Sparber 2009; Ottaviano and Peri 2012; Caiumi and Peri 2024). Other work finds more adverse effects, especially for workers who are close substitutes for immigrants or in settings with limited short-run adjustment (Borjas 2003; Dustmann et al. 2017). Our findings are most closely related to the evidence emphasizing limited displacement: local employment rises approximately one-for-one with unauthorized immigrant worker flows.

Our empirical design builds on the immigration-network approach, which exploits the tendency of new immigrants to settle in locations with earlier immigrants from the same origin country (Card 2001; Card 2009). Recent work has clarified the identifying assumptions behind these designs and proposed refinements to address concerns arising from persistent local shocks, origin-destination-specific sorting, and the concentration of identifying variation in particular groups (Jaeger et al. 2019; Burchardi et al. 2019; Goldsmith-Pinkham et al. 2020; Terry et al. 2026). We follow these advancements using a general pull shift-share design and assess its credibility through first-stage diagnostics, Rotemberg weights, over-identification tests, and placebo tests. We also compare these results to those obtained from the conventional enclave shift-share design.

The paper is also related to work on unauthorized immigration and immigration enforcement. Because legal status is difficult to observe in standard survey data, many studies infer unauthorized status indirectly and study enforcement-induced reductions in immigrant labor supply (Borjas 2017; East et al. 2023; Cravino et al. 2026; Cox and East 2026). We complement this work by measuring unauthorized immigration directly at fine geographic levels using administrative microdata.⁴ This distinction is important because unauthorized immigrants may differ from legal immigrants even conditional on the country of origin or other observed characteristics.

A separate literature studies the effects of immigration on local housing markets. Immigration can raise rents and house prices by increasing housing demand (Saiz 2007; Cabral and Steingress 2026). At the same time, low-skilled immigration, in particular, may also affect housing supply by lowering construction costs or raising construction employment, generating potentially different short- and long-run implications (Monras 2020; Howard et al. 2024). We contribute by jointly estimating the effects of UIWF on house prices, rents, building permits, and construction employment during the post-pandemic episode, showing that demand-side forces dominated in the short run.

Finally, we note that our estimates should be interpreted as local, place-based effects, which can differ from national aggregate effects. Cross-geographic designs compare areas more exposed

⁴See also Duzhak (2024), who uses state-level data on new immigration court cases to document the correlation between unauthorized immigrant inflows and changes in vacancy-to-unemployment ratios over 2023 to 2024.

to immigration with areas less exposed to immigration. While informative about local adjustment, these estimates need not equal national effects if immigration changes aggregate demand, prices, or government budgets. This distinction is central to work emphasizing native mobility and general equilibrium spillovers (Borjas 2003; Dustmann et al. 2025). It is also central to recent work studying the macroeconomic effects of unauthorized immigration in one-sector models of output growth and inflation (Cheremukhin et al. 2025) and in multi-sector models with input-output linkages (Cravino et al. 2026). Our paper complements this work by using a causal inference design and rich microdata to provide direct empirical discipline on local labor- and housing-market responses to unauthorized immigration.

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 in unauthorized immigration from early 2021 to early 2024, which was followed by a sharp slowdown. We then describe the data and the construction of unauthorized immigration measures at both the national and disaggregated 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 administrative microdata.

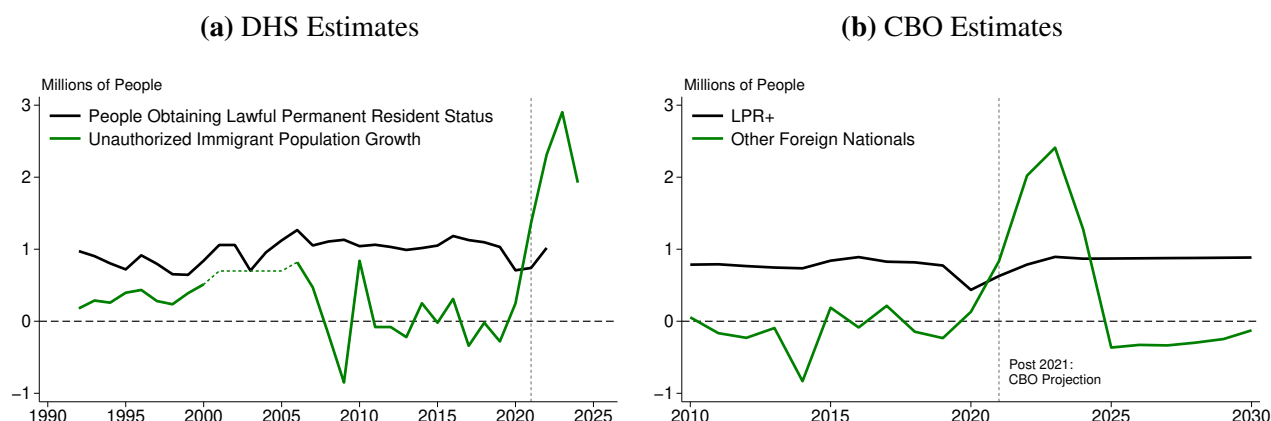
3.1 Background

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 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

⁵See <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 resident 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.

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 CBO and several recent studies similarly estimate a large increase in net unauthorized immigration beginning in 2021 (e.g., [Edelberg et al. 2025](#); [Duzhak and New-Schmidt 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 2026b](#)).

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 to enter and remain temporarily (Con-

⁶By 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”.

gressional 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 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.

Another distinctive feature of this immigration surge is that a relatively large share of new arrivals were eligible to apply for a work permit (Employment Authorization Document or EAD) or became eligible after a statutory waiting period (Foote 2024). This differs from many earlier immigration waves and reflects the large number of asylum applicants and parolees in the recent episode. Parolees generally may apply for work authorization after receiving parole, while individuals with pending asylum applications may apply after their application has been pending for 150 days. Thus, although many immigrants in the recent surge entered without formal admission for purposes of immigration law, they may subsequently become eligible to work while remaining in the U.S.

Work-permit application data published by U.S. Citizenship and Immigration Services (USCIS) provide useful information about potential changes in labor supply, but they do not measure employment (Foote 2024; Edelberg and Powell 2025). This is because not all eligible immigrants apply for work authorization, applications may not result in approval, and approval does not imply the recipient obtains employment. Conversely, immigrants without work permits may nevertheless work and be captured in payroll employment data. These challenges, together with the lack of local geographic information in the publicly available application-count data, make it difficult to map work-permit application counts into actual employment attributable to the recent immigration surge. Ultimately, the extent to which immigrant arrivals translate into employment is an empirical question, which we assess using administrative payroll employment data described below.

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 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 date of entry, date of NTA issuance, court decisions, custody status, and the individual’s U.S. mailing address on record with the immigration court, including zip code. Individuals are required to update this address when they move, and we use the most recent address recorded in the court data to measure local residence. The associated zip code allows us to assign NTA-based entries to detailed geographic areas.

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 use these microdata to construct national parole inflows by origin and age, which enter our measures of unauthorized immigration flows and the origin-specific national shifts used in our IV strategy. We access both datasets through TRAC.

We supplement these microdata with DHS estimates of nationwide “got-aways,” defined as individuals who entered without being apprehended by border authorities.⁸ For external validation of our estimates of unauthorized immigration flows, we also collect DHS aggregate statistics on encounters and deportations (the sum of removals, returns, and expulsions). The aggregate

⁷<https://tracreports.org/>

⁸DHS 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).

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). The QCEW is constructed from establishment-level records drawn from state unemployment insurance (UI) filings and covers nearly the universe of private nonfarm employment. Its employment measure is monthly payroll employment: the number of workers on employer payrolls during the pay period that includes the 12th of the month, matching the definition used in the national Employment Situation report. Wages are measured by average weekly wages of employees over a quarter.⁹

Importantly, QCEW coverage is not conditioned on workers' immigration or work-authorization status. These characteristics are not collected by state UI agencies or the BLS for QCEW reporting.¹⁰ A worker enters the employment counts when the employer reports the worker as covered under the state UI system. This feature makes QCEW data particularly useful in our setting, because workers are not excluded from the employment counts based on immigration status, allowing recent unauthorized immigrants to be captured when they are reported on payrolls.

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 and the CoreLogic Home Price Index. For measuring new housing supply, we collect monthly MSA-level building permit data from the Census Bureau and annual housing unit counts from the ACS.

To study the effects of UIWF on income and government transfers, we obtain annual county-level data on personal income and its components from the BEA Regional Economic Accounts and aggregate them to the CZ level. Finally, to address concerns about possible correlation between international and domestic migration, we construct domestic net in-migration flows (DNIF) by

⁹The QCEW data are also used to annually benchmark the employment counts in the Current Employment Statistics (CES). By contrast, the CES is a monthly survey of a sample of employers, is subject to sampling error, and is not available below the MSA level.

¹⁰See U.S. Bureau of Labor Statistics, *Quarterly Census of Employment and Wages: Questions and Answers*.

aggregating the Census Bureau’s annual county-level net domestic migration to CZs and MSAs. We cumulate these flows from mid-2021 to mid-2024 and divide by each locality’s initial employment.

Household Survey Data. We use the ACS, a large-scale annual household survey that provides detailed demographic and labor market information, for three main purposes. First, we use 2015–2019 five-year ACS data to measure the geographic distribution of immigrants across CZs and MSAs by country of origin, which is central to our empirical designs. Second, we use 2021–2023 one-year ACS data to construct local labor-market outcomes that complement our QCEW-based analyses. Specifically, we construct employment and average hourly wages by nativity, education and industry, consistent with survey-based measures commonly used in prior studies (e.g., [East et al. 2023](#); [Caiumi and Peri 2024](#); [Albert et al. 2026](#)). Third, we measure work-from-home (WFH) intensity to use as a potential control. We use ACS annual data to construct the number of WFH workers in each local area, following [Mondragon and Wieland \(2025\)](#), and then divide the change in this number from 2021 to 2024 by the area’s initial employment.

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 entries, and got-aways.^{11,12} 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 B for construction details). Appendix Figure B1 presents all entry and exit categories incorporated in our estimates.¹³

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 the start and

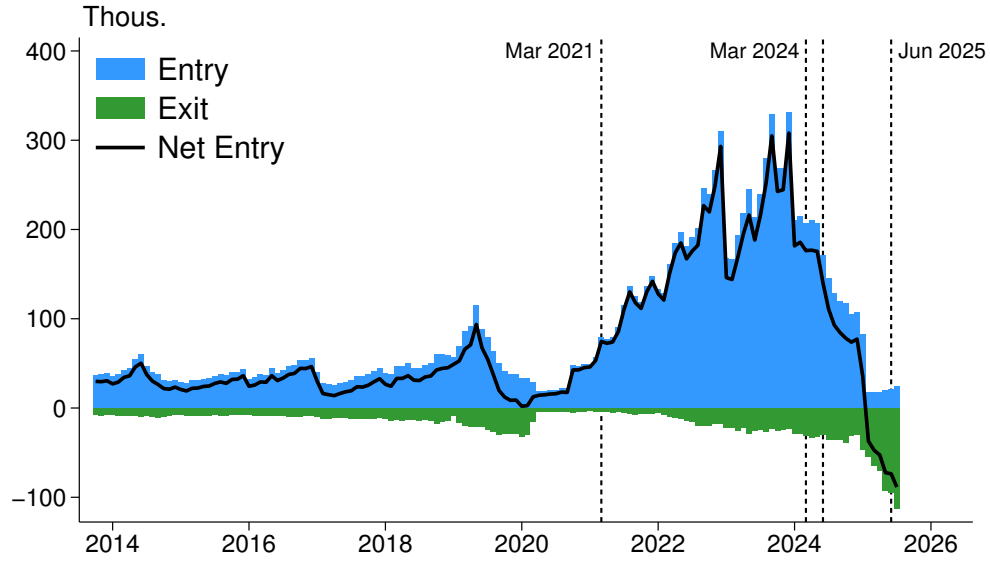
¹¹In NTA microdata, the recorded date of entry is missing for approximately 30% of individuals. For these cases, we use the date of initial NTA issuance—available for all individuals—as a proxy for the entry date. This is supported by the fact that in cases where both dates are observed, roughly 75% differ by no more than one month.

¹²Using 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 DHS aggregate got-away estimates to mitigate potential double-counting, as such individuals may already be captured in the NTA-based inflow measure.

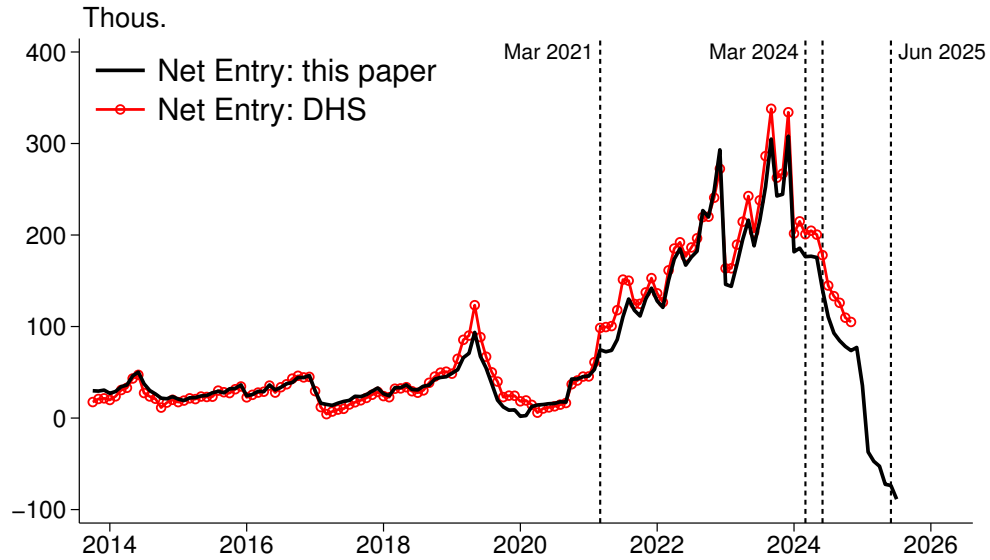
¹³The entry and exit categories incorporated in our estimates are broadly consistent with those used by the CBO in its annual projections of “Other Foreign Nationals.” We exclude two additional categories—temporary visa overstays (which contribute to entries) and adjustments to lawful permanent resident 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).

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 earlier. 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 2021-2024, a relatively small set of origin countries in Central and South America accounted for the majority of unauthorized entries. In Appendix Figure B2, we present monthly net inflows from eleven countries that, based on DHS encounter data, had the highest numbers of encounters between early 2021 and 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.¹⁴

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 accounted for the vast majority of post-pandemic immigration inflows and drove the aggregate dynamics during the boom and subsequent retrenchment.

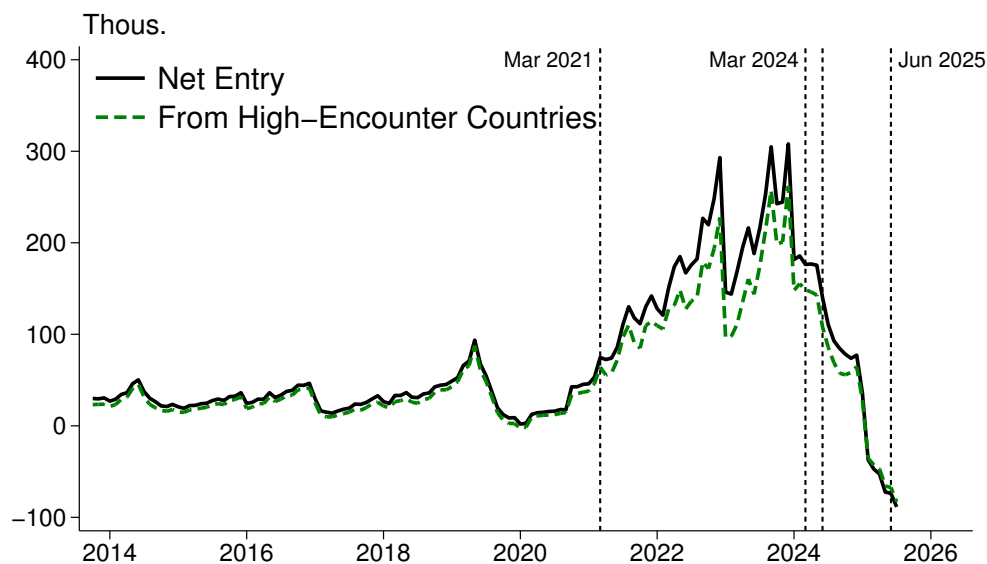
So far, we have focused on measuring total unauthorized immigration. Since our empirical analysis examines the impact of UIWF on local labor markets, we next assess the working-age composition and employment of unauthorized immigrants. Because the individual-level NTA and parole data record each person’s 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

¹⁴We 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 (at and between ports of entry), given that these two entry categories are most similar to got-aways.

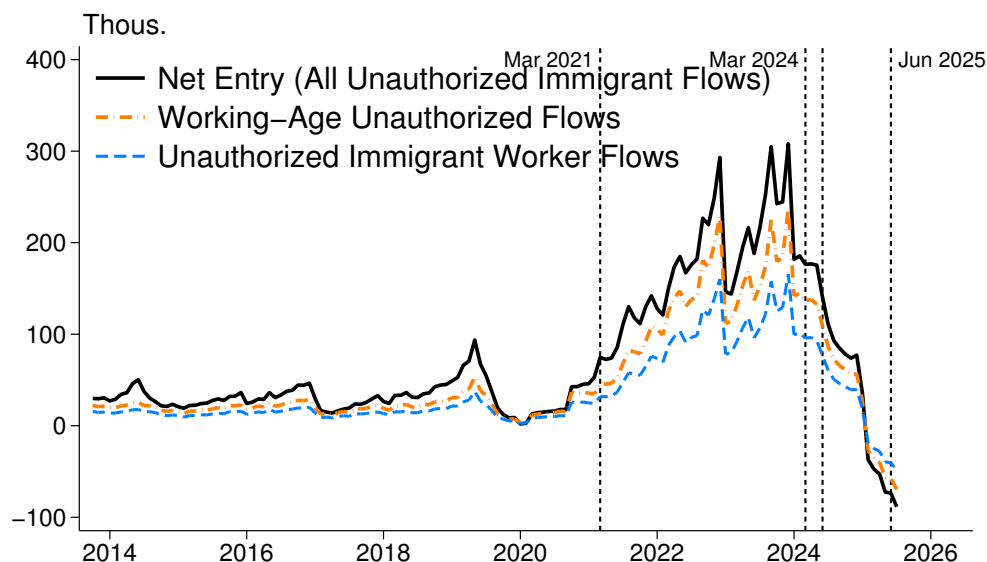
one-year microdata. Taken together, these figures imply that between 50% and 60% of unauthorized immigrants entering the U.S. in recent years are likely workers.

Figure 3: Country and Age Composition of Net Unauthorized Immigration

(a) By Country



(b) By Age



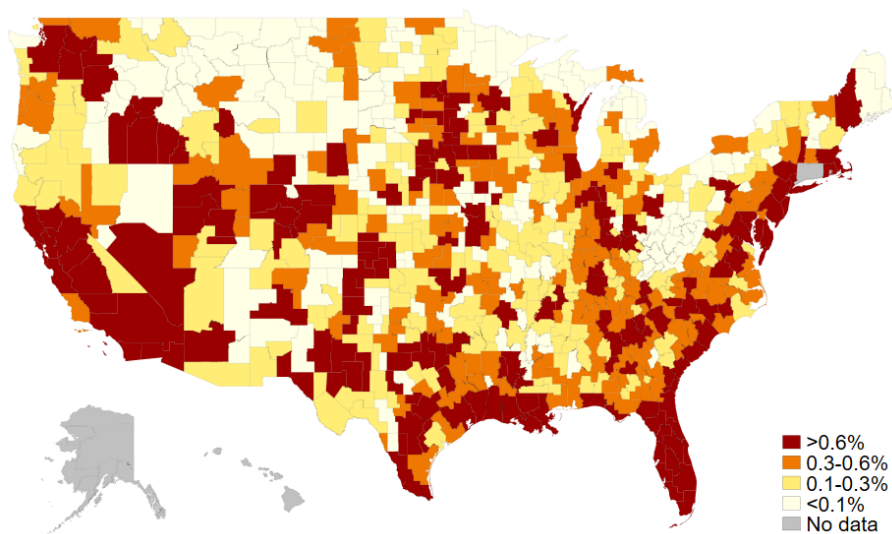
Notes: High-Encounter countries include Mexico, Guatemala, Honduras, El Salvador, Venezuela, Colombia, Cuba, Ecuador, Nicaragua, Haiti, and Peru. Working age refers to age 16-65. Unauthorized immigrant worker flows refer to working-age unauthorized immigrants who are employed.

3.4 The Geography of Unauthorized Immigrant Worker Flows

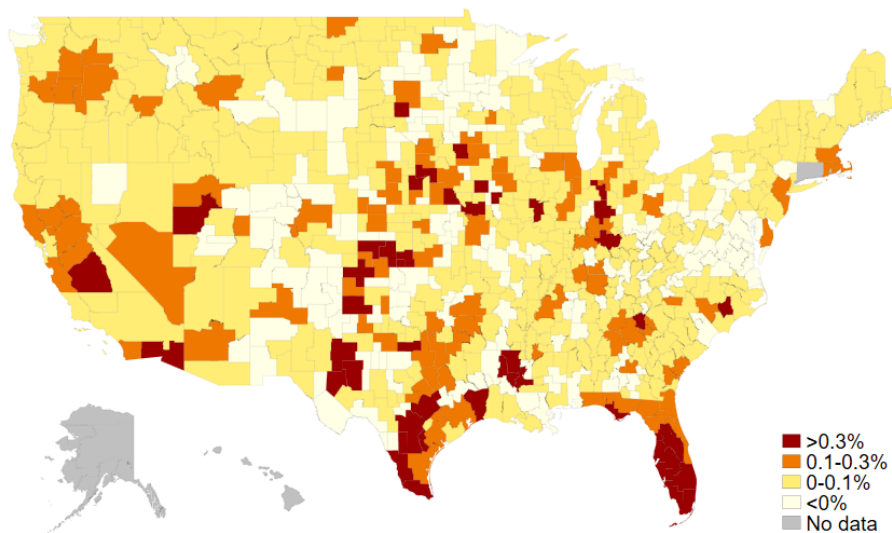
For individuals issued NTAs, who account for the vast majority of unauthorized immigration since 2021, the administrative records contain the most recent U.S. address on file with the immigration court, which we use to assign individuals to counties. Because residential locations are unavailable for parolees and got-aways, we allocate national net unauthorized immigration from each origin country across counties using the county distribution of NTA recipients from the same origin.

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 B for details). We then construct county-level measures analogous to the national series: net inflows for all individuals, working-age individuals, and likely workers (i.e., UIWF). For our empirical analysis, we aggregate these county-level measures to CZs and MSAs.¹⁵

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 2026b), 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 labor and housing market outcomes, as well as broader economic conditions. Our baseline analysis focuses on the immigration boom from March 2021 to March 2024. Because this episode represents a large, discrete national shock, we use a cross-sectional long-difference design that relates variation in local exposure to the immigration boom to changes in local outcomes over the same period.¹⁶ This design captures the cumulative local effects, which are our primary object

¹⁵The 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 settlement patterns may differ across these categories of unauthorized immigrants, especially since the pandemic. However, it is important to note that any resulting measurement error in UIWF is unlikely to meaningfully affect the validity of our IV estimates in Sections 5 and 6 for two reasons. First, as shown in Appendix Figure B1, the categories for which we lack geographic information are small relative to the NTA category. Second, our IVs are constructed primarily from aggregate origin-specific inflows and geographical settlement shares from the ACS. 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.

¹⁶In principle, one could estimate effects separately by year or in a short panel. We do not do so because annual outcome changes may reflect both contemporaneous UIWF effects and delayed responses to earlier inflows; with

of interest. In Section 5.4, we assess the robustness of the baseline results to alternative sample periods using an expanding-windows analysis.

We also examine the subsequent slowdown in unauthorized immigration from June 2024 to June 2025 (Appendix D.5), but treat these results as an extension and interpret them with greater caution 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), precluding a comprehensive evaluation of deportation effects. Second, measures of unauthorized outflows are subject to greater uncertainty, particularly from 2025 onward, given limited data on voluntary exits. Third, economic outcomes during the slowdown may reflect both contemporaneous immigration flows and lagged effects of the earlier boom. Separately identifying these effects while accounting for the potential endogeneity of both would introduce substantial multicollinearity and imprecision. By contrast, lagged effects of earlier UIWF are unlikely to be important for the boom-period estimates, because UIWF was generally close to zero across local areas immediately before the boom in 2020–2021.

4.1 OLS Specification

We begin with the OLS specification:

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

where d indexes destinations, either commuting zones or MSAs. T denotes the period of interest, and t_0 is the initial month of T . The dependent variable is the percentage change in the outcome of interest over period T . $I_{d,T}$ denotes UIWF over the same period (constructed as described in Section 3.4), and E_{d,t_0} is beginning-of-period employment. The vector $\mathbf{x}_d$ includes predetermined controls intended to account for local characteristics that may be correlated with UIWF. Specifically, we include NAICS 2-digit industry employment shares from 2015–2019 QCEW data, together with 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 and bachelor’s degree or above), and the 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 range of outcomes for $Y_{d,t}$. In addition to employment, we examine wages, house prices, market rents, total personal income, labor income, and income from government

only three annual intervals, these components cannot be separately identified without strong dynamic assumptions.

transfers. For the latter three, we report results both in total and per capita terms.¹⁷ To examine housing-supply responses, we use as the dependent variable the cumulative number of new building permits issued over period T , scaled by the initial housing stock, $\frac{\text{Permits}_{d,T}}{\text{HousingUnits}_{d,t_0}}$. This measure is analogous to an investment-to-capital ratio.

4.2 Identification

The primary concern with the OLS estimates is that local immigration flows may respond endogenously to local economic conditions, even though equation (1) is estimated in differences and includes a rich set of controls. In particular, contemporaneous reverse causality may arise if positive local demand shocks attract immigrant workers. We address this concern using two alternative instrumental-variable designs. The first uses the conventional “enclave shift-share” (ESS) instrument, which predicts the geographic distribution of recent immigrants from each origin country based on realized historical settlement shares of immigrants from that same country. This design is motivated by the idea that new immigrants are pulled toward particular destinations by the presence of large, established communities of immigrants from the same origin country (i.e., enclaves). The second uses a “general pull shift-share” (GPSS) instrument, which predicts the geographic distribution of recent immigrants based not on destinations’ pull factors specific to an origin, but rather on destinations’ pull factors that attracted past immigrants from *other* countries. This approach reduces reliance on realized same-origin settlement shares, which may reflect persistent origin-destination-specific sorting.

Conventional shift-share instrument and endogenous sorting. The conventional ESS instrument of Card (2001, 2009) interacts current national immigration inflows from origin o with the pre-existing share of immigrants from that origin residing in destination d , and aggregates these interactions across origins. To avoid a mechanical relationship between destination d ’s predicted and actual immigration flows, we follow the literature and construct a leave-one-out version using national inflows excluding destination d :

$$ESS_{d,T} = \sum_o \left(I_{o,-d,T} \times s_{o,d} \times \frac{1}{\bar{E}_d} \right), \quad (2)$$

where $I_{o,-d,T}$ denotes national UIWF from origin o over period T , excluding flows into d . The term $s_{o,d} = \frac{M_{o,d}}{M_{o,.}}$ denotes the realized pre-boom settlement share, where $M_{o,d}$ is the number of immigrant workers from origin o residing in d , and $M_{o,.}$ is the corresponding national total. The shift-share interaction is scaled by pre-period employment, $\bar{E}_d$, to match the normalization of the

¹⁷For 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.

endogenous variable.¹⁸ To isolate the “innovation” relative to a destination’s expected exposure, we also *recenter* the ESS IV, following [Maroto et al. \(2026\)](#) and [Borusyak et al. \(2025\)](#), by including the destination-level sum of the exposure shares, $\sum_o \frac{M_{o,d}}{\bar{E}_d}$, which is the pre-boom immigrant worker share in d , as a control variable.

A concern with the ESS instrument, however, is that realized historical settlement shares may themselves reflect persistent origin-destination-specific sorting. For example, persistent demand for particular skills in a destination may attract successive waves of immigrants from the same origin. As emphasized by [Burchardi et al. \(2019\)](#) and [Terry et al. \(2026\)](#), using realized settlement shares to predict current immigration, as in equation (2), may embed these persistent forces in the instrument, generating a spurious relationship between predicted immigration inflows and subsequent local outcomes.¹⁹

To address this concern, [Burchardi et al. \(2019\)](#) and [Terry et al. \(2026\)](#) develop a strategy that isolates variation in origin-specific settlement without relying on persistent origin-destination sorting. The idea is to combine historical origin-specific migration shocks, or “push” factors, with destination-specific “pull” factors measured using immigrants from *other* origins. The latter capture a destination’s general attractiveness to immigrants, rather than its particular attractiveness to immigrants from origin o . These past push-pull interactions are used to predict origin-specific ancestry. Building on this approach, [Terry et al. \(2026\)](#) interact predicted ancestry with current immigration inflows to construct an instrument for local immigration exposure. Thus, rather than using the full variation in realized settlement shares, identification comes from the component predicted by generic destination pull factors.

Predicted settlement exposure and GPSS instrument. We adapt this insight to the large, one-time immigration shock studied here. Because our setting differs from the long historical panel considered by [Burchardi et al. \(2019\)](#) and [Terry et al. \(2026\)](#), and our primary interest is in cumulative cross-sectional effects, we construct a parsimonious measure of predicted settlement exposure and embed it in the conventional shift-share instrument. Specifically, define

$$g_{o,d} = \frac{M_{-o,d}}{M_{-o,}}$$

as the leave-origin-out destination pull, where $M_{-o,d}$ is the number of pre-boom immigrant workers

¹⁸We use 2021 ACS employment data for this scaling to be consistent with the ACS data used to construct the settlement shares. Using March 2021 QCEW employment instead yields very similar results.

¹⁹[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.

in destination d from all origins other than o , and $M_{-o,\cdot}$ is the corresponding pre-boom national total. As in [Burchardi et al. \(2019\)](#), $g_{o,d}$ measures destination d 's general attractiveness to immigrants in the pre-period, while deliberately excluding immigrants from origin o .

A simple implementation of the general-pull idea would use $g_{o,d}$ directly as the predicted settlement exposure for each origin, i.e., setting $\hat{s}_{o,d} = g_{o,d}$, and construct the corresponding GPSS instrument:

$$GPSS_{d,T} = \sum_o \left(I_{o,-d,T} \times \hat{s}_{o,d} \times \frac{1}{\bar{E}_d} \right). \quad (3)$$

This implementation effectively imposes a common unit loading of origin-specific settlement on general destination pull. We relax this restriction by allowing the strength of this relationship to vary across origins, since different origin groups may rely, to different degrees, on general destination attractiveness. For this purpose, we estimate

$$s_{o,d} = \delta_o + \lambda_o g_{o,d} + v_{o,d}, \quad (4)$$

where $s_{o,d}$ is the realized pre-boom settlement share defined in equation (2), δ_o is an origin fixed effect, λ_o is the origin-specific loading on the leave-origin-out destination pull, and $v_{o,d}$ captures residual variation, which may reflect origin-destination-specific sorting driven by persistent local demand for particular skills. Our preferred measure of predicted settlement exposure uses the component of $s_{o,d}$ attributable to general destination pull:

$$\hat{s}_{o,d} = \hat{\lambda}_o g_{o,d}.$$

Substituting this predicted exposure for $\hat{s}_{o,d}$ in equation (3) yields our preferred GPSS instrument.

The instrument therefore combines cross-origin variation in national UIWF with cross-destination variation in leave-origin-out immigrant pull, with the strength of the latter allowed to vary across origins. The intuition is to use the part of origin o 's historical settlement pattern explained by destinations' general pull for immigrants from *other* origins, with the strength of this relationship captured by an origin-specific loading. These earlier same-origin communities, in turn, attract new immigrants when current push factors increase national inflows from o . The identifying assumption is that, conditional on the predetermined controls, predicted exposure to the origin-specific national immigration shocks is uncorrelated with unobserved changes in local outcomes over period T . In Section 5, we further assess the identifying assumption using pre-period placebo tests and alternative sets of controls.

We prefer the GPSS instrument to the ESS instrument because it reduces direct reliance on realized settlement shares, which may reflect persistent origin-destination-specific sorting. The

ESS IV is also sensitive to destinations with highly concentrated immigrant enclaves, where large historical settlement shares interact with large national inflows to generate extreme predicted exposure. When this exposure is not matched by correspondingly large realized local inflows, individual destinations can exert disproportionate influence on the first-stage relationship.²⁰ Indeed, in our sample, this concern is particularly relevant for the CZ containing Miami: its ESS value is roughly three times the next highest value, eight times the 99th percentile of the instrument’s distribution, and four times its realized UIWF measure.²¹ As shown in Section 5, our preferred instrument exhibits strong first-stage relevance and is not sensitive to outliers.

The GPSS and ESS estimates yield broadly consistent conclusions for most outcomes, with detailed comparisons presented in subsequent sections. We also report estimates in Appendix D based on the full historical predicted-ancestry instrument of Burchardi et al. (2019) and Terry et al. (2026). The construction of that instrument is detailed in Appendix C.

5 Impacts on the Labor Market

In this section, we present OLS and IV estimates of the effects of UIWF on local labor markets during the March 2021–March 2024 immigration boom. Subsection 5.1 presents the main employment results, drawing primarily on QCEW administrative data on nonfarm employment, with complementary estimates from the ACS. Subsection 5.2 assesses the validity of our identification strategies by investigating pre-trends, Rotemberg weights, over-identification tests, and robustness to alternative sets of controls. In subsection 5.3, we turn to wage effects, using both QCEW data on average weekly wages and ACS data on average hourly earnings. Finally, subsection 5.4 presents three extensions: (i) employment and wage effects by industry, (ii) expanding-window estimates that extend the endpoint month by month from March 2024 to June 2025, and (iii) back-of-the-envelope calculations of the implied cumulative effects for the median and average local areas.

In Appendix A, we develop a simple local-equilibrium framework that helps interpret labor-market responses. UIWF directly expand local labor supply, but may also shift labor demand outward through immigrants’ expenditure on local goods and services. By raising housing costs, they may further affect the labor supply of pre-existing workers. The resulting employment response therefore need not be one-for-one, while the sign of the wage response is theoretically ambiguous. We next examine the employment and wage responses in turn.

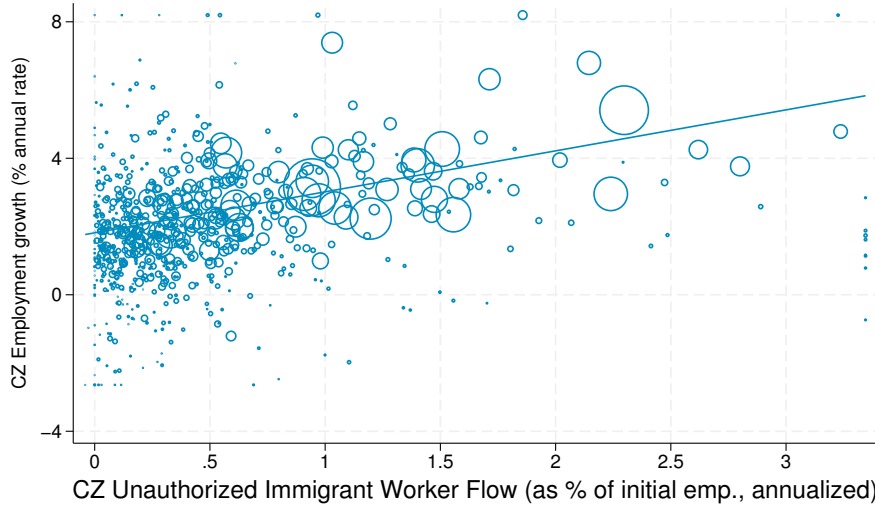
²⁰See Young (2022) for evidence on the sensitivity of IV inference to high-leverage observations, particularly in the presence of heteroskedastic or clustered errors.

²¹This outlier leads to a very low first-stage F -statistic. For this reason, we cap the ESS value for the Miami CZ at the next-highest value in the distribution. Excluding this CZ from the sample yields similar results.

5.1 Impacts on Employment

Before presenting the regression results, we first visualize the correlation between UIWF and employment growth across CZs over the boom period. Figure 5 provides a scatter plot of these two variables. Each circle represents a CZ, with its size proportional to initial employment. The fitted OLS line shows a clear positive relationship between UIWF and employment growth.

Figure 5: Employment Growth vs. Immigrant Worker Flows Across CZs, 2021m3–2024m3



Notes: Each circle represents a commuting zone (CZ), with size proportional to its initial employment.

Table 1 shows estimates of β_1 in equation (1) for the employment effect. The OLS estimate is 0.84 (column 1), statistically distinguishable from zero, but not from one. The ESS and GPSS IV estimates are 0.95 and 0.97, respectively (columns 2–3), and highly significant. The preferred GPSS estimate implies that an increase in UIWF equal to 1% of initial employment raises local employment by 0.97%—a nearly one-for-one response. It is important to note that the first-stage F -statistics are approximately 68 for the ESS instrument and 29 for the GPSS instrument, both well above standard critical values for weak instrument bias.

So far, our key explanatory variable has been UIWF as a share of initial employment. For comparison, Appendix Table D1 reports estimates using *all* unauthorized immigrant flows and *all working-age* unauthorized immigrant flows, both scaled by initial employment but unadjusted for employment rates. The GPSS IV estimates are 0.54 and 0.68, respectively, both statistically significant at the 1% level. The corresponding ESS IV estimates are similar, at 0.54 and 0.66. These estimates are close to the corresponding worker shares in our data, approximately 56% of all unauthorized immigrants and 70% of working-age unauthorized immigrants, consistent with the nearly one-for-one employment response to UIWF.

Because many prior studies of the labor market effects of immigration have relied on household

Table 1: Effect of Unauthorized Immigrant Worker Flows on Employment, 2021m3–2024m3

	(1)	(2)	(3)
	OLS, Emp.	IV(ESS), Emp.	IV(GPSS), Emp.
UIWF	0.840*** (0.143)	0.947*** (0.251)	0.968*** (0.274)
Constant	-0.130 (0.113)	-0.0984 (0.111)	-0.103 (0.123)
Observations	721	721	721
R^2	0.581	0.580	0.579
First-Stage Results:			
Coefficient Estimate		0.647***	0.939***
Coefficient Std. Error		(0.078)	(0.174)
F Statistic		67.99	28.87

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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

survey data, we also estimate employment effects using annual ACS data to facilitate comparisons. These estimates warrant caution for several reasons. Unauthorized immigrants may be underrepresented because of reluctance to participate in government surveys and incomplete survey coverage (Brown et al. 2023). These concerns are particularly relevant during the post-pandemic boom, when ACS-based immigration estimates did not fully capture the rise in inflows (Coglianese et al. 2025). Moreover, the annual frequency of the ACS prevents exact alignment with the boom period identified in our monthly UIWF data.

With these caveats in mind, we nonetheless provide ACS employment estimates over 2021–2023 for all individuals, the foreign-born, the foreign-born from high-encounter (HE) countries, and the native-born, with additional estimates for low- and high-education native-born individuals. Because ACS responses are collected throughout the year, we use midyear endpoints to approximate the timing of the annual employment measures. Specifically, we measure cumulative UIWF from June 2021 to June 2023, scaled by initial employment, and apply the same ESS and GPSS IV strategies as above.

Table 2 shows the results. The GPSS IV estimate for total employment is 0.902 (panel a), close to the baseline QCEW estimate but imprecise and statistically insignificant. The estimated effect on foreign-born employment is approximately 0.75 and significant at the 5% level. This response is driven almost entirely by individuals from high-encounter countries (column 3), which accounted for the vast majority of unauthorized immigrant worker inflows during this period. The estimated effect on native-born employment is small (column 4), with no statistically significant effects for

either low- or high-education native-born individuals (columns 5-6).

The ESS IV estimates in panel (b) are broadly similar to the GPSS IV estimates, with a few notable differences. The estimated total employment effect is 0.72, lower than the GPSS estimate and likewise statistically insignificant. The subgroup estimates show a sharper contrast: the foreign-born employment effect is approximately 1.2, while the native-born effect is approximately -0.5 and insignificant. Among native-born individuals, the estimated effect is negative and significant for those with low education, but near zero and insignificant for those with high education. The former is consistent with possible crowd-out of low-skilled native-born workers by unauthorized immigrant workers, although the corresponding GPSS IV estimate is statistically insignificant.

Taken together with the roughly one-for-one employment response in the QCEW data, the ACS results suggest that the increase in total employment primarily reflects the direct contribution of unauthorized immigrant workers to foreign-born employment. Estimated effects on native employment range from near zero (under GPSS) to modestly negative (under ESS), but are insignificant under both instruments. This interpretation speaks directly to the long-standing debate over whether immigration displaces native employment. Since local employment includes both immigrant and native workers, the coefficient β_1 implies that native employment changes by approximately $\beta_1 - 1$ for each additional immigrant worker. Our estimate of a one-for-one employment response thus implies little to no crowd-out and is consistent with a large empirical literature finding limited native displacement (e.g., [Card 2001](#); [Peri and Sparber 2009](#); [Ottaviano and Peri 2012](#)).

5.2 Validity of the Identification Strategy

Before turning to other outcomes and extensions, we assess the validity of our identification strategies in the context of the employment effect estimates.

5.2.1 Assessing the Parallel Trends Assumption

First, we assess our identifying assumption using pre-period placebo tests. Specifically, we reestimate equation (1) for employment growth, holding the boom-period UIWF measure, controls, and instruments fixed but replacing the outcome with employment growth over four earlier 36-month intervals: March 2015–March 2018, March 2016–March 2019, March 2017–March 2020, and March 2018–March 2021. We perform these tests separately for GPSS and ESS instruments.

Figure 6 plots the estimated coefficients on UIWF and their 95% confidence intervals, with GPSS in panel (a) and ESS in panel (b). For comparison, the baseline estimates for March 2021–March 2024 are shown on the far right of each panel. The GPSS placebo estimates are statistically insignificant in all four intervals. The point estimates are approximately 0.5 for the first three intervals and negative for the most recent pre-boom period, March 2018–March 2021. The ESS

Table 2: Effects of Unauthorized Immigrant Worker Flows on ACS Employment, 2021–2023

(a) General Pull Shift-Share (GPSS) IV

	(1)	(2)	(3)	(4)	(5)	(6)
	CZ Total	Foreign-Born	HE Immigrants	Native-Born	Low-Ed Native	High-Ed Native
UIWF	0.902 (0.786)	0.748** (0.350)	0.694*** (0.245)	0.154 (0.839)	0.166 (0.383)	-0.0119 (0.522)
Constant	-0.0390 (0.166)	-0.0937** (0.0364)	-0.0438* (0.0223)	0.0547 (0.147)	0.170*** (0.0558)	-0.115 (0.119)
Observations	712	712	708	712	712	712
R ²	0.045	0.319	0.114	0.041	0.031	0.049
First-Stage Results:						
Coefficient Estimate	0.572***	0.572***	0.570***	0.572***	0.572***	0.572***
Coefficient Std. Error	(0.099)	(0.099)	(0.099)	(0.099)	(0.099)	(0.099)
F Statistic	32.83	32.83	32.61	32.83	32.83	32.83

(b) Enclave Shift-Share (ESS) IV

	(1)	(2)	(3)	(4)	(5)	(6)
	CZ Total	Foreign-Born	HE Immigrants	Native-Born	Low-Ed Native	High-Ed Native
UIWF	0.723 (0.551)	1.246*** (0.226)	0.995*** (0.140)	-0.523 (0.499)	-0.559*** (0.212)	0.0366 (0.442)
Constant	-0.0457 (0.170)	-0.0746 (0.0494)	-0.0336 (0.0325)	0.0290 (0.143)	0.141*** (0.0474)	-0.112 (0.122)
Observations	712	712	708	712	712	712
R ²	0.047	0.170	-0.083	0.035	0.030	0.050
First-Stage Results:						
Coefficient Estimate	0.406***	0.406***	0.406***	0.406***	0.406***	0.406***
Coefficient Std. Error	(0.048)	(0.048)	(0.048)	(0.048)	(0.048)	(0.048)
F Statistic	69.08	69.08	69.29	69.08	69.08	69.08

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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

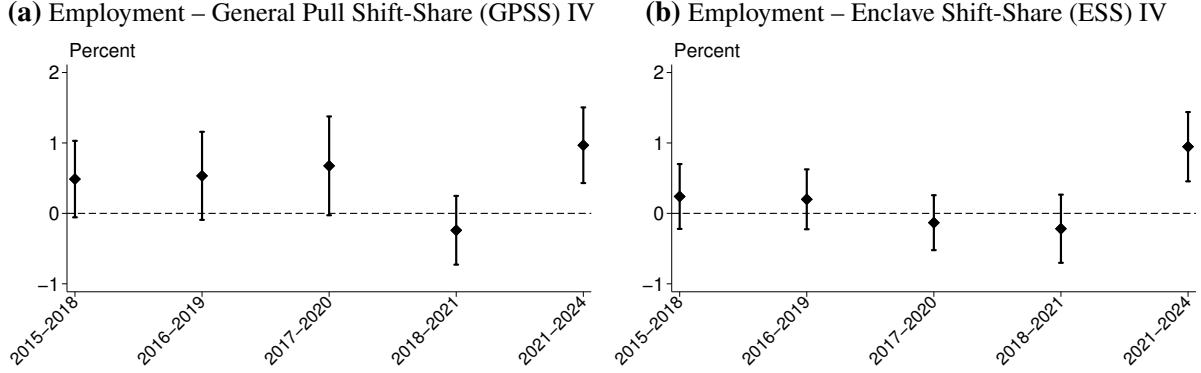
placebo estimates are uniformly close to zero and statistically insignificant. Thus, we view these results as supportive of the parallel trends assumption for both IV designs.

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

We now examine the concentration and consistency of the origin-specific variation underlying our IV estimates, following recommendations from [Goldsmith-Pinkham et al. \(2020\)](#) and [Borusyak et al. \(2025\)](#). We report Rotemberg weights, which summarize the influence of each origin-specific exposure on the aggregate IV estimate, alongside the just-identified estimates obtained using each exposure separately as an instrument. We also estimate over-identified specifications using the origin-specific instruments jointly.

Panel A of Table 3 reports these diagnostics for the employment effects of UIWF, with smaller origin countries grouped into a single “Other” category. The ESS estimate places substantial

Figure 6: Placebo Regressions Using Prior 3-Year Periods: Employment



Notes: Each figure plots the UIWF coefficient (diamond) and 95% confidence interval (bracket) from a cross-CZ IV regression of employment growth on the same set of regressors and instruments as used in our baseline boom-period specifications. Panel (a) uses the general pull shift-share IV; panel (b) uses the leave-one-out enclave shift-share IV. Analogous placebo regressions for wage growth are shown in Figure D3.

weight on just three countries—Venezuela, Haiti, and Cuba—with their individual Rotemberg weights ranging from 0.20 to 0.30, suggesting that immigration inflows from these three countries play an outsized role in determining the ESS IV estimates. In addition, the just-identified estimates vary widely across origins. These two results raise some concern that the ESS IV estimates may be unstable and overly sensitive to a small set of origin countries. By contrast, no individual country’s GPSS Rotemberg weight exceeds 0.15, and the corresponding just-identified estimates are uniformly close to one, ranging from 0.99 to 1.14. Finally, panel B reports results from the over-identified specifications. Both yield employment estimates close to one, and neither over-identification test rejects the null of instrument exogeneity.

Overall, Table 3 shows greater cross-origin stability for GPSS than for ESS. This finding, together with the concerns about persistent origin-destination-specific sorting and the sensitivity of ESS estimates to influential destinations discussed in Section 4, supports our preference for the GPSS instrument. Nonetheless, for completeness and transparency, we continue to report results based on both IV designs throughout the paper and note when the two yield material differences.

In Appendix Table D2, we also perform the same diagnostics for another alternative IV design, the full historical predicted-ancestry IV of Burchardi et al. (2019) and Terry et al. (2026). This design yields an estimate of UIWF’s employment effect of 2.5, statistically significant but with a wider confidence interval. The corresponding over-identified estimate is 1.5. Two diagnostics, however, raise concerns about this design in our setting. First, the Kleibergen–Paap first-stage F -statistic is below 7, indicating limited instrument relevance. Second, a single country, Mexico, receives a Rotemberg weight of 0.67, indicating that the estimate depends heavily on the Mexico-specific component of the instrument.

Table 3: Validation of Empirical Design

Panel A. Rotemberg weights	<u>Enclave Shift-Share IV</u>		<u>General Pull Shift-Share IV</u>	
	Rotemberg $\hat{\alpha}_k$	Just-identified $\hat{\beta}_k$	Rotemberg $\hat{\alpha}_k$	Just-identified $\hat{\beta}_k$
Venezuela	0.302	1.180	0.143	1.068
Haiti	0.276	0.936	0.074	1.138
Cuba	0.199	0.217	0.093	1.129
Colombia	0.142	1.392	0.081	1.032
Ecuador	0.091	2.146	0.058	1.014
Honduras	0.026	1.437	0.037	1.014
Peru	0.021	1.239	0.036	1.017
Nicaragua	0.004	-20.756	0.128	1.028
Guatemala	-0.008	1.525	0.051	1.013
El Salvador	-0.012	0.961	0.020	1.002
Mexico	-0.058	0.987	0.059	0.986
Other	0.016	4.728	0.220	0.803

Panel B. Over-ID test				
Full IV estimate ($\hat{\beta}$)		0.947***		0.968***
$Se(\hat{\beta})$		(0.251)		(0.274)
First-Stage F		67.99		28.87
Over-identified IV estimate ($\hat{\beta}^{OID}$)		1.203***		0.913***
$Se(\hat{\beta}^{OID})$		(0.316)		(0.217)
Over-identification test p-value		0.77		0.67

Notes: Rotemberg weights are computed following [Goldsmith-Pinkham et al. \(2020\)](#) for the two IV specifications: the leave-one-out ESS IV and GPSS IV. $\hat{\beta}_k$ is the just-identified IV estimate using only the pre-existing immigrant share associated with a given country as the instrument. $\hat{\beta}^{OID}$ is the IV estimate using all shares as separate instruments. Stars denote statistical significance (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

5.2.3 Robustness to Alternative Controls

We next examine the sensitivity of the employment estimates to alternative controls, beginning with three particular omitted variables of concern *a priori*. One such variable is CZ employment growth over the COVID lockdown period. A concern might be that boom-period employment growth could be stronger in CZs that had lower employment growth during the lockdown period and that the rebound pattern could be correlated with UIWF. To test this, we add employment growth over the lockdown period (February 2020–February 2021) to the baseline GPSS specification. It has a minimal effect: the UIWF coefficient changes from 0.968 (se=0.274) to 0.855 (se=0.304).

Two other *a priori* concerns are that either domestic net in-migration flows (DNIF) or the work-from-home (WFH) rise during the 2021–2024 period could be positively correlated with both UIWF and employment growth across CZs.²² To test this, we constructed CZ-level measures of

²²We exclude these variables from our baseline set of controls because they are potentially endogenous, themselves

each factor, as described in Section 3.2. Both measures are in fact slightly *negatively* correlated with UIWF: the unweighted and initial-employment-weighted correlations are -0.02 and -0.03 for DNIF, and -0.05 and -0.26 for WFH (Appendix Figure D1). Adding these controls to our baseline specification has little impact on our results: the GPSS IV coefficient changes from 0.968 to 1.048 (se=0.275) when including DNIF, 0.957 (se=0.262) when including WFH, and 1.029 (se=0.263) with both included.

Next, we examine the robustness of the GPSS IV estimates to controls more generally by conducting a specification-curve analysis (SCA; Simonsohn et al. 2020). This involves estimating the regression of interest separately for all possible combinations of control variable sets and then plotting the resulting treatment effect estimates and their confidence intervals. We perform this analysis for all combinations of the following six sets of controls: (1) industry shares, (2) age population shares, (3) white population share, (4) education population shares, (5) employment growth during the COVID lockdown period, and (6) DNIF and WFH.

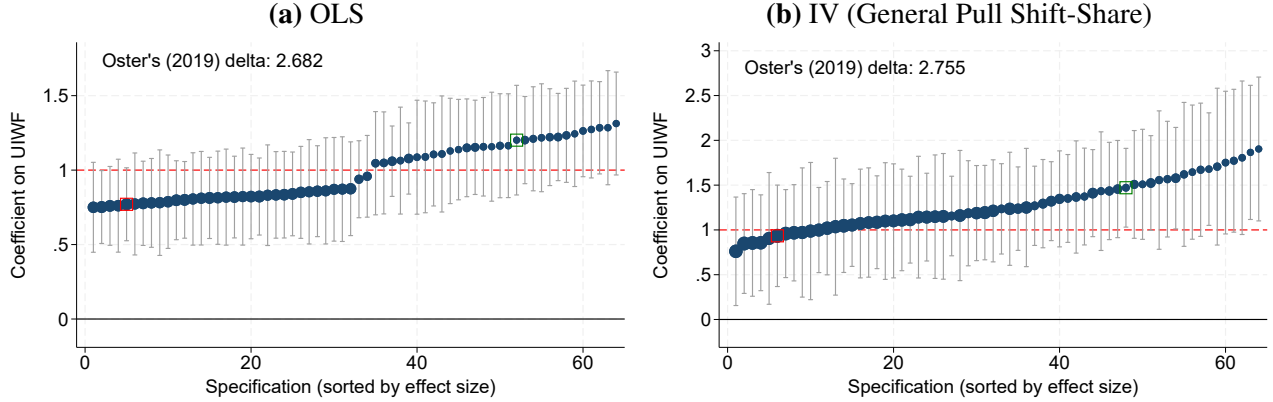
A closely related approach to assessing robustness is Oster (2019)’s δ metric. This δ measures the size of the estimated treatment effect in the full controls specification relative to the difference between the effect with zero controls and with full controls, scaled to account for differences in explanatory power (R^2). Intuitively, if the treatment effect with full controls is far from zero and the difference between having and not having controls is minimal, then there is less concern about potential omitted variable bias.

We conduct these analyses for both the OLS and IV estimations of equation (1). Regarding the IV analysis, a caveat is that Oster’s (2019) diagnostic was derived for OLS and has not been formally extended to the IV setting. We therefore apply the approach to IV using the second-stage R^2 . Figure 7 shows the OLS results in panel (a) and the GPSS IV results in panel (b); analogous ESS IV results are provided in Appendix Figure D2. Within each panel, the estimated UIWF effects are ordered from lowest to highest. Circle size is proportional to the R^2 , and brackets denote 95% confidence intervals.

The analysis yields four main findings, all of which support the robustness of the baseline employment effect of approximately one. First, the estimated effect is positive and statistically significant in every specification. Second, all OLS specifications and all but four IV specifications fail to reject the hypothesis that the effect equals one; moreover, those four exceptions all reject on the high side (estimates above one) and are among the lowest- R^2 specifications. Third, the specifications with the highest R^2 s—indicating they best fit the data—tend to produce estimates close to one, and none of them reject an effect of one. Fourth, for both OLS and IV, Oster’s δ is well above one, indicative of minimal omitted variable bias concern.

responding to UIWF.

Figure 7: Specification Curve: UIWF Effect on Employment



Notes: The specifications consist of all combinations of the following six sets of controls (1) industry shares, (2) age population shares, (3) white population share, (4) education population shares, (5) COVID (2020m2—2021m2) employment growth, and (6) domestic net in-migration (DNIF) and the change in WFH workers over 2021–2024, each scaled by initial employment. The green square identifies the specification with no controls, while the red square identifies the specification with full controls. Circle size is proportional to the R^2 .

5.3 Impacts on Wages

Table 4 shows estimates of the effect of UIWF on average weekly wages using QCEW data.²³ Unlike the employment results, the wage estimates differ across specifications. The OLS estimate is modestly negative and statistically insignificant (column 1). The preferred GPSS IV estimate is approximately -0.9, but imprecise and statistically insignificant (column 3). By contrast, the ESS IV estimate is approximately 1.2 and statistically significant (column 2).²⁴

While we prefer the GPSS IV estimate for the reasons discussed above, we conclude from these conflicting estimates that the data do not allow a confident conclusion regarding UIWF's effects on average weekly wages. Moreover, weekly wages reflect both hourly earnings and weekly hours, which the QCEW does not report separately. An increase in hours per worker—consistent with the positive employment effect we find—could therefore offset a decline in hourly earnings, leaving the effect on weekly wages ambiguous.

For this reason, and to facilitate comparisons with prior survey-based studies, we also estimate the effects of UIWF on average hourly earnings in the ACS.²⁵ As in Section 5.1, this analysis covers 2021–2023 and remains subject to the data limitations discussed there. Table 5 reports negative and

²³Appendix Figure D3 reports the wage placebo regressions. All pre-boom placebo estimates are statistically insignificant under both ESS and GPSS IV designs.

²⁴We note, however, that the ESS IV wage estimate, unlike its employment counterpart, is highly sensitive to the inclusion of the pre-boom immigrant worker share. Omitting this variable yields a near-zero, insignificant UIWF coefficient. This sensitivity, together with the previously discussed *a priori* identification concerns, concentrated Rotemberg weights, and influence of the Miami outlier, leads us to place greater weight on the GPSS IV results.

²⁵Following standard practice in the literature, we measure CZ-level average hourly earnings as the ratio of total wage income to total hours worked among wage earners.

Table 4: Effect of UIWF on QCEW Average Weekly Wages, 2021m3–2024m3

	(1)	(2)	(3)
	OLS, Wages	IV(ESS), Wages	IV(GPSS), Wages
UIWF	-0.140 (0.197)	1.267*** (0.198)	-0.944 (0.605)
Constant	0.0877 (0.121)	0.199* (0.118)	-0.0825 (0.192)
Observations	720	720	720
R^2	0.636	0.607	0.578
First-Stage Results:			
Coefficient Estimate		0.647***	0.939***
Coefficient Std. Error		(0.078)	(0.174)
F Statistic		67.97	28.88

Notes: The dependent variable is the growth in average weekly wages. 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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

statistically significant effects under both GPSS (panel a) and ESS (panel b). The GPSS estimate of -0.872 implies that an increase in UIWF equal to 1% of initial employment reduces hourly wages by approximately 0.9%, similar to the GPSS estimate for QCEW average weekly wages. The ESS IV estimate is similar in magnitude, at approximately -1, and highly statistically significant, in contrast to the positive ESS estimate for QCEW average weekly wages.

Both instruments also yield similar negative estimates for native-born hourly wages (column 4). The education breakdown is less conclusive (columns 5-6). Under GPSS, the point estimate is modestly more negative for low-education than for high-education native-born workers, although only the latter is significant at the 10% level. The ESS estimates show a sharper contrast, with a substantially more negative and significant estimate for the low-education group. Taken together, both IV designs suggest that UIWF reduced hourly wages, due partly to native-born workers' wage responses, which in turn were primarily driven by a negative effect among those with low education.

5.4 Extensions

5.4.1 Employment and Wage Impacts by Industry

We next examine industry-level employment and wage effects for each NAICS major sector, using QCEW and ACS data under both the GPSS and ESS IV designs. We focus on the QCEW results using our preferred GPSS instrument; corresponding ESS estimates and ACS results under both instruments are shown in the appendix. Industry employment changes are scaled by the same initial CZ employment measure used in the aggregate analysis. This normalization allows the

Table 5: Effects of UIWF on ACS Average Hourly Earnings, 2021–2023

(a) General Pull Shift-Share (GPSS) IV

	(1)	(2)	(3)	(4)	(5)	(6)
	CZ Total	Foreign-Born	HE Immigrants	Native-Born	Low-Ed Native	High-Ed Native
UIWF	-0.872** (0.341)	1.847 (1.510)	0.373 (2.444)	-0.698* (0.374)	-0.935 (0.674)	-0.764* (0.437)
Constant	-0.105 (0.0907)	0.821* (0.483)	0.142 (0.523)	-0.179* (0.0959)	0.110 (0.139)	-0.244** (0.105)
Observations	712	712	710	712	712	712
R ²	0.096	0.007	0.023	0.114	0.111	0.089
First-Stage Results:						
Coefficient Estimate	0.572***	0.572***	0.572***	0.572***	0.572***	0.572***
Coefficient Std. Error	(0.099)	(0.099)	(0.100)	(0.099)	(0.099)	(0.099)
F Statistic	32.83	32.83	32.76	32.83	32.83	32.83

(b) Enclave Shift-Share (ESS) IV

	(1)	(2)	(3)	(4)	(5)	(6)
	CZ Total	Foreign-Born	HE Immigrants	Native-Born	Low-Ed Native	High-Ed Native
UIWF	-0.988** (0.427)	-0.968 (1.023)	-1.101 (1.374)	-0.711 (0.494)	-1.954** (0.812)	-0.582 (0.444)
Constant	-0.109 (0.0887)	0.707 (0.447)	0.0850 (0.487)	-0.181* (0.102)	0.0659 (0.128)	-0.238** (0.112)
Observations	712	712	710	712	712	712
R ²	0.090	0.020	0.023	0.113	0.086	0.093
First-Stage Results:						
Coefficient Estimate	0.406***	0.406***	0.406***	0.406***	0.406***	0.406***
Coefficient Std. Error	(0.048)	(0.048)	(0.048)	(0.048)	(0.048)	(0.048)
F Statistic	69.08	69.08	68.87	69.08	69.08	69.08

Notes: Companion to Table 2, reporting average hourly earnings effects under both the GPSS and ESS shift-share instruments. 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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

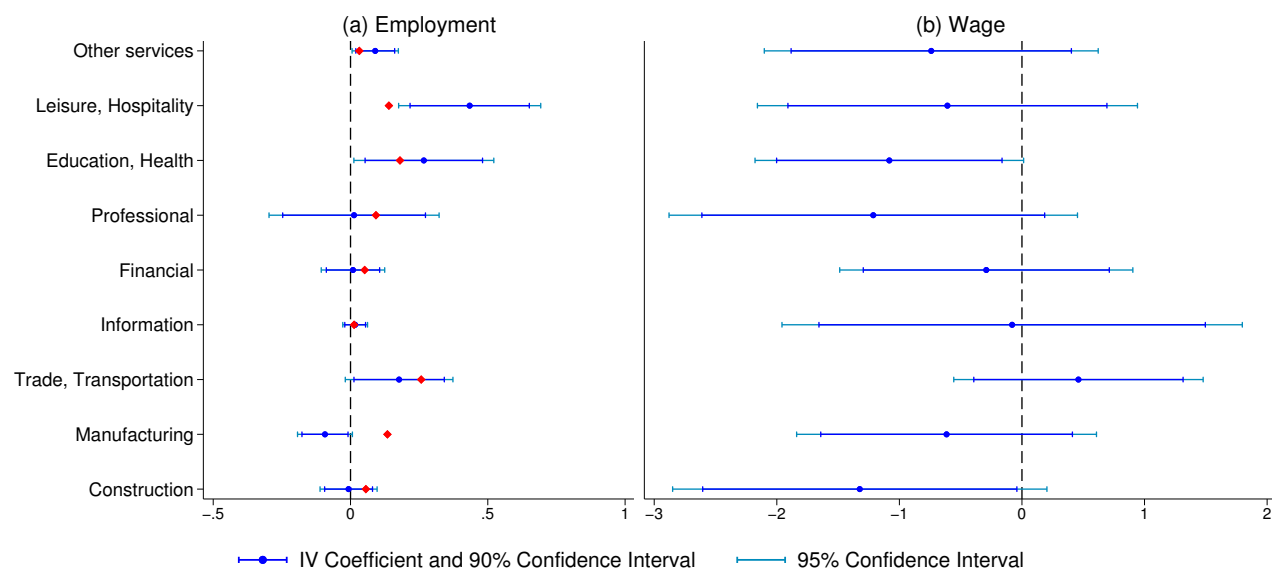
industry-specific estimates to be interpreted as contributions to the total employment response and avoids skewed industry-level growth rates arising from small initial employment levels in some CZ-industry cells. The wage outcomes are analogous to those in the aggregate analysis: log changes in QCEW average weekly wages and ACS average hourly earnings.

Panel (a) of Figure 8 reports the GPSS IV employment estimates and their 90% and 95% confidence intervals, alongside each industry's average employment share over 2015–2019. Given the roughly one-for-one employment response, these shares provide a benchmark for employment gains distributed proportionately across industries. The estimated effects are positive and significant in three industries: Leisure & Hospitality, Education & Health, and Other Services. Only Leisure & Hospitality has an estimate significantly above its historical employment share, indicating its disproportionate contribution to the total employment effect. We also find a negative effect of

UIWF on Manufacturing employment, significantly below its typical share. The appendix reports corresponding ESS estimates (Figure D4) and ACS results under both IV designs (Figures D5-D6). The ACS employment estimates under GPSS show a similar pattern, with large positive effects in Leisure & Hospitality, and Manufacturing estimates below its historical employment-share benchmark. The ESS IV estimates are somewhat different, pointing to disproportionately large positive effects for professional and financial services.

Panel (b) shows the GPSS IV estimates for QCEW industry-level average weekly wages. The point estimates are generally negative, but only weakly significant for two industries: Construction and Education & Health. The appendix results reveal the same contrast across outcomes and instruments as in the aggregate wage analysis: ESS yields positive estimates for QCEW weekly wages in all industries, several statistically significant (Figure D4), whereas the ACS hourly earnings estimates are negative in most industries under both IV designs (Figures D5-D6). Taken together, we view the wage estimates as too imprecise and sensitive to the outcome measure and instrument to draw firm conclusions about how UIWF impacted wages in specific industries.

Figure 8: Employment and Wage Effects of UIWF by Industry, 2021m3–2024m3



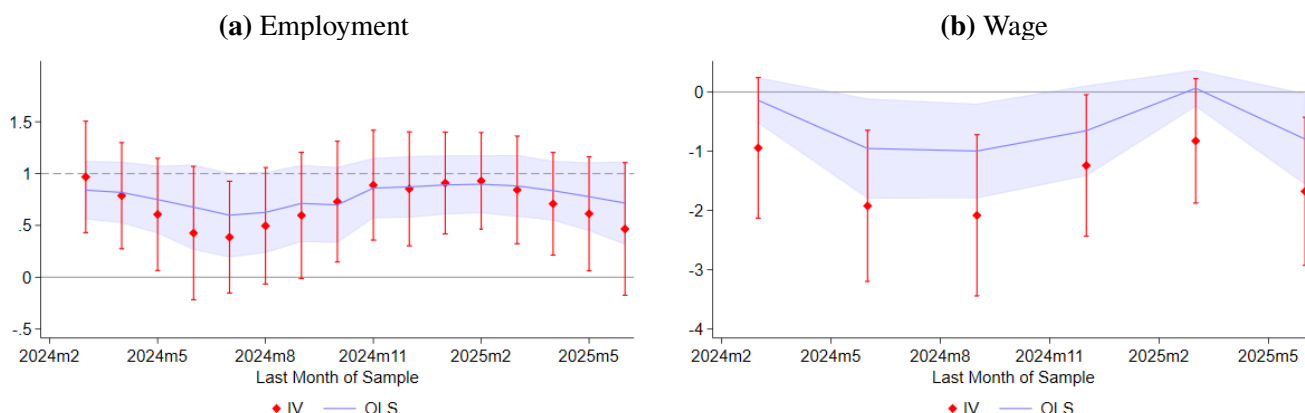
Notes: Panel (a) reports employment effects and panel (b) average weekly wage effects; both panels share the common industry axis at left. UIWF (as share of initial employment) is winsorized at 99th percentile due to outliers. Observations are weighted by beginning-of-period total employment. Brackets indicate the 95% confidence intervals based on standard errors that are heteroskedasticity-robust and clustered on state. Red diamonds denote each industry's average 2015–2019 employment share.

5.4.2 Employment and Wage Impacts Across Expanding-Window Samples

Figure 9 plots expanding-window OLS and GPSS IV estimates for employment (panel a) and average weekly wages (panel b). Holding the start date fixed at March 2021 (the first quarter of

2021 for wages), we extend the endpoint from March 2024 to June 2025 in monthly increments for employment and quarterly increments for wages. The initial-window estimates reproduce the baseline results in Tables 1 and 4, respectively.

Figure 9: Employment and Wage Impacts Across Expanding-Window Samples



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 are heteroskedasticity-robust and clustered on state.

Across these windows, the GPSS IV employment estimate ranges from approximately 0.5 to 1, while the wage estimate ranges from roughly -1 to -2. The point estimates thus remain positive for employment and negative for wages, although their magnitudes vary across endpoints. One caveat is that the QCEW data are not seasonally adjusted. The regression intercept absorbs seasonal changes common to all CZs, but not CZ-specific seasonality that may potentially be correlated with UIWF, for example, if unauthorized immigrant workers are concentrated in seasonal industries such as Leisure & Hospitality. Hence, the results for endpoints far from March or Q1—the calendar month and quarter in which our sample begins in 2021—should be interpreted with caution.

Lastly, we also provide results in Appendix D.5 using the sample period of June 2024–June 2025, which corresponds to the immigration slowdown period. They point to similar positive employment point estimates, but they are less precisely estimated.

5.4.3 Back-of-the-Envelope Implied Cumulative UIWF Effects

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 cumulative effects of UIWF over the boom period and their contributions to local economic outcomes. We consider the effects for both a *typical* local area, as given by the median CZ, and the *average* area, as given by the weighted mean of CZs. Table 6 presents the results from this exercise, based on the

GPSS IV estimates. The implied employment effects are shown in the first row.²⁶ (We discuss the housing results in Section 6.1.)

Table 6: Contribution of UIWF to Key Outcomes, 2021m3-2024m3

	Median CZ/MSA (unweighted)			Average CZ/MSA (weighted)		
	Implied Effect %, (1)	Total Change %, (2)	Contribution %, (1)/(2)	Implied Effect %, (4)	Total Change %, (5)	Contribution %, (4)/(5)
Employment	0.8	5.6	14.7	2.6	8.7	30.3
House Prices	3.0	22.3	13.3	6.7	22.4	30.1
Market Rents	1.9	22.3	8.4	4.2	21.6	19.6

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 CZ experienced cumulative UIWF of 0.8% relative to its March 2021 employment level. Multiplying this shock by our baseline GPSS IV estimate of 0.968 implies that, for the typical CZ, UIWF boosted nonfarm employment by about 0.8%. This 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 cumulative UIWF was relatively small, its importance for explaining overall employment growth was modest.

By contrast, the last three columns show a larger implied contribution of UIWF to employment growth in the *average* labor market. The employment-weighted average puts greater weight on larger, urban CZs, which experienced more substantial inflows. The weighted mean of UIWF as a share of initial employment—equivalent to the corresponding national ratio—was 2.7%. Multiplying this ratio by the baseline GPSS IV estimate yields an implied employment effect of 2.6%. Given that employment growth for the *average* CZ, as well as for the nation, was 8.7%, UIWF can account for roughly 30% of the employment growth over this period.²⁷ The corresponding ESS-based calculation yields a nearly identical contribution of 29%.

6 Impacts on the Broader Local Economy

This section examines the effects of unauthorized immigration during the boom on local outcomes beyond the labor market. We first study house prices, rents, and new housing supply, then turn to local personal income. Within personal income, we examine government transfers

²⁶We do not perform these calculations for wages because our preferred estimate of the marginal effect of UIWF on wages is statistically insignificant.

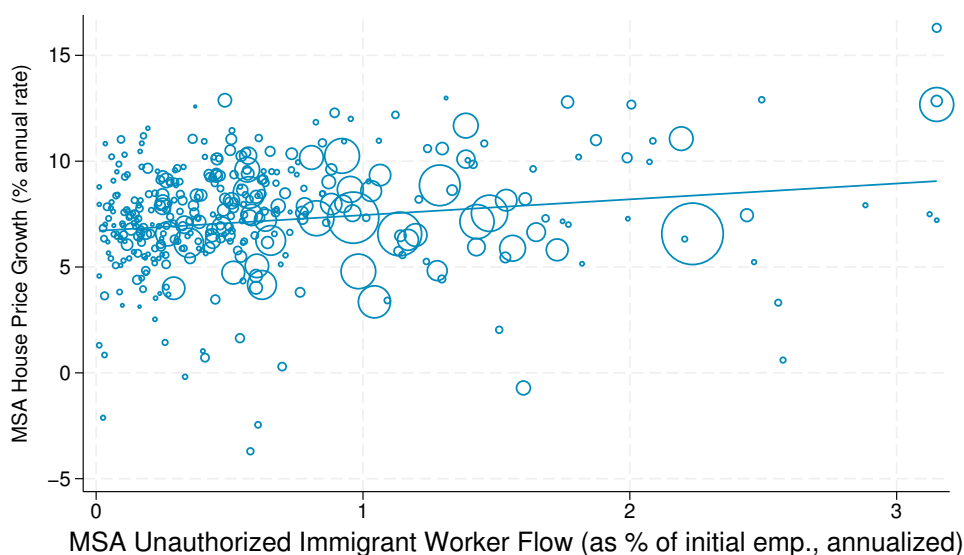
²⁷Although these weighted averages coincide with the corresponding national ratios, the implied contribution based on a local IV estimate need not equal the national effect of UIWF. National effects may also reflect cross-CZ spillovers and general equilibrium adjustments beyond local labor markets.

separately, motivated by public policy debates over immigration’s effects on welfare spending.

6.1 Housing Market

We estimate the effects of UIWF on local house prices, market rents, and new housing supply using MSA-level data. Our preferred price measures are Zillow’s house-price and rent indexes. For robustness and comparison with prior studies, we also use CoreLogic and Freddie Mac house-price indexes. We measure new housing supply as cumulative housing permits issued during the March 2021–March 2024 boom divided by the initial stock of housing units. Before discussing the regression results, we present a scatter plot in Figure 10 to visualize the relationship between UIWF and house price growth across MSAs for the boom period, with circle sizes proportional to initial employment. The plot shows a clear positive relationship. By contrast, the corresponding scatter plot for new housing supply shows no clear relationship (Appendix Figure D8).

Figure 10: House Price Growth vs. UIWF Across MSAs, 2021m3–2024m3



Notes: Each circle represents a metropolitan statistical area (MSA), with size proportional to its initial employment.

Effects on house prices and rents. The regression results for shelter prices are shown in Table 7. 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 estimates using the GPSS IV 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%. Using the ESS IV yields somewhat larger effects, with 3.0% for prices and 2.4% for rents.²⁸

Results using the CoreLogic and Freddie Mac house price indexes are reported in Appendix

²⁸Appendix Table D5 shows results for rents by structure type. The preferred GPSS IV estimates indicate modestly larger rent responses for multifamily than for single-family units.

Table D4. The evidence is less clear with these alternative house price measures. The IV estimates using the ESS instrument are large, between 2.6 and 3.6, and statistically significant, while those based on the GPSS instrument are smaller, between 0.6 and 1.2, and statistically insignificant.

Table 7: Effect of Unauthorized Immigrant Worker Flows on Shelter Prices, 2021m3–2024m3

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS, HP	IV(ESS), HP	IV(GPSS), HP	OLS, Rent	IV(ESS), Rent	IV(GPSS), Rent
UIWF	1.140** (0.488)	2.987*** (0.622)	2.231*** (0.763)	1.584*** (0.177)	2.427*** (0.236)	1.401*** (0.364)
Observations	346	346	346	278	278	278
R^2	0.369	0.216	0.309	0.537	0.500	0.534
First-Stage Results:						
Coefficient Estimate		0.471***	0.778***		0.473***	0.779***
Coefficient Std. Error		(0.045)	(0.217)		(0.047)	(0.220)
F Statistic		109.6	12.88		100.9	12.44

Notes: Effects of UIWF on Zillow house-price and rent growth over the boom period. UIWF (as share of initial employment) is winsorized at the 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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Identification checks. As in the employment analysis in Section 5.2, we assess our IV designs for house prices and rents using Rotemberg weights, over-identification tests, and pre-period placebo regressions. We also examine the sensitivity of the estimates to alternative controls. Appendix Tables D7–D10 show that no single origin receives a large GPSS weight, whereas ESS places substantial weight on three countries, Venezuela, Haiti, and Cuba, suggesting that the ESS estimates are heavily affected by them. Over-identification tests using the origin-specific components as separate instruments fail to reject the null of instrument exogeneity under either design.

Appendix Figure D9 shows pre-period placebo estimates following the procedure in Section 5.2. We replace boom-period house-price and rent growth with growth over four earlier 36-month intervals, keeping UIWF, controls, and instruments the same as in the baseline. For house prices, the ESS placebo estimates are all near zero and insignificant, in sharp contrast to the large, positive boom-period estimate. The GPSS placebo estimates are near zero in the two most recent pre-boom intervals, but positive and significant for 2015–2018 and 2016–2019. These earlier pre-trends indicate that GPSS house-price estimates should be viewed with some caution. For rents, the GPSS placebo estimates are close to zero and insignificant in all four intervals. The ESS estimates are negative and statistically significant in the first two intervals but near zero and insignificant in the last two. In sum, both IV designs yield positive and significant boom-period estimates for house prices and rents and near-zero placebo estimates in the two most recent pre-boom intervals.

Finally, adding controls for domestic net in-migration flows (DNIF) and work-from-home (WFH) intensity has little effect on the house-price and rent estimates. The preferred GPSS house-

price estimate changes from 2.231 to 2.240 (se=0.765) with DNIF included, to 2.338 (se=0.724) with WFH included, and to 2.342 (se=0.722) with both included. The corresponding rent coefficient changes from 1.401 to 1.399 (se=0.360), 1.446 (se=0.279), and 1.446 (se=0.275), respectively.

Housing supply and heterogeneous price responses. The results for new housing supply are shown in Table 8. We find no significant effect of UIWF on new housing supply across OLS and both IV designs.²⁹ These results, along with the subdued construction-sector employment effects in both QCEW data (Figure 8) and ACS data (Appendix Figures D5–D6), suggest that during the boom period, UIWF acted primarily as a housing demand shock in an environment of relatively fixed short-run housing supply.

Table 8: Effect of Unauthorized Immigrant Worker Flows on Housing Supply, 2021m3–2024m3

	(1)	(2)	(3)
	OLS, Total	IV(ESS), Total	IV(GPSS), Total
UIWF	0.152 (0.121)	0.0803 (0.307)	-0.215 (0.219)
Observations	341	341	341
R^2	0.500	0.503	0.464
First-Stage Results:			
Coefficient Estimate		0.471***	0.778***
Coefficient Std. Error		(0.045)	(0.216)
F Statistic		108.9	12.90

Notes: New housing supply is measured as cumulative permits over the period divided by initial housing units. UIWF (as share of initial employment) is winsorized at the 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 joint labor- and housing-market equilibrium framework in Appendix A highlights that the housing-market response to UIWF varies with the elasticity of local housing supply. Where supply is less elastic, shelter prices are more responsive. We examine this prediction by testing whether denser MSAs, which tend to have less elastic housing supply in the short run, experienced larger house-price and rent responses to UIWF. Specifically, we augment the GPSS IV specifications in columns (3) and (6) of Table 7 with an interaction between UIWF and an indicator for above-median MSA population density.³⁰ Figure 11 plots the implied effects for high- and low-density MSAs. Across all four house-price and rent outcomes, low-density MSAs exhibit near-zero responses to UIWF, while high-density MSAs exhibit strongly significant and positive responses.³¹ The differences between the high- and low-density responses are also statistically significant, with p -values of <0.001 , 0.029, 0.065, and 0.079 across the four columns of the figure. These results suggest

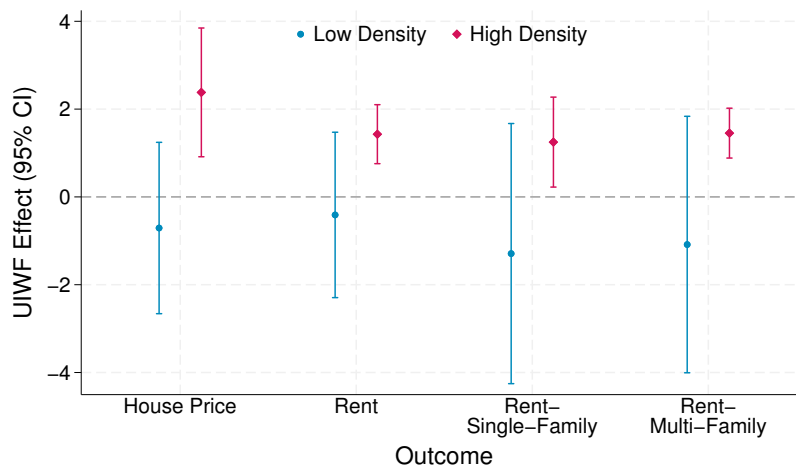
²⁹Analogous results by structure type also generally show small and insignificant housing supply responses (Table D6).

³⁰MSA density is measured as the population-weighted average of county density from the 2020 decennial census.

³¹Results based on the ESS IV design are very similar. See Appendix Figure D10.

that the positive average shelter-price responses are concentrated in denser markets, consistent with housing supply constraints amplifying the effects of UIWF.

Figure 11: Heterogeneous Shelter Price Responses by Population Density, 2021m3-2024m3



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.

Implied cumulative effects. To quantify the contribution of UIWF to local house price and rent growth during early 2021–early 2024, the last two rows of Table 6 apply our baseline GPSS IV estimates to the cumulative increase in UIWF. As in the employment exercise in Section 5.4.3, we report cumulative effects for both a typical local area, represented by the median MSA, and the average local area, represented by the employment-weighted mean across MSAs. In the median MSA, UIWF increased by approximately 1.3% of initial employment. Multiplying this value by the baseline estimates—2.2 for house prices and 1.4 for rents—implies UIWF effects of 3.0% on house prices and 1.9% on rents. These effects contribute modestly to the median MSA’s total house-price and rent growth over this period.

For the *average* MSA, however, the implied effects are substantially larger. Applying the baseline IV estimates to the employment-weighted mean of UIWF—3.1% of initial employment—yields increases of 6.7% in house prices and 4.2% in rents. These implied effects account for approximately 30% of total house-price growth (22.4%) and 20% of total rent growth (21.6%) in the average market over this period.

6.2 Personal Income and Government Transfers

Lastly, we examine the effects of UIWF on local personal income and its two key components—labor income and government transfers—using BEA regional accounts data for 2021–2023.³² We estimate equation (1) using growth in each income measure as the dependent variable. Table 9 presents the preferred GPSS IV estimates; corresponding OLS and ESS IV estimates appear in Appendix Table D11. The estimates in columns (1)–(4) point to no significant effects on total or per capita personal income, but a negative effect on labor income per capita. This decline may reflect a compositional effect: an inflow of unauthorized immigrants with lower labor income per person than existing residents can reduce the average labor income in the local area. This finding is also consistent with the negative GPSS estimates for QCEW average weekly wages and ACS average hourly earnings.

Despite the roughly one-for-one employment response, the lack of a significant boost from UIWF on total local income raises the interesting question of how UIWF can boost local housing prices without changing total 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.³³ In this case, unauthorized immigrant inflows lead 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, putting upward pressure on rents when supply cannot adjust.

We also 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 Census Bureau’s Survey of Income and 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 only measure 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, or between immigrants heading households containing U.S. citizen children or others.³⁴

The results in Table 9 reject the hypothesis that unauthorized immigration leads to higher transfers. Indeed, our GPSS IV estimates show a significant *negative* effect of UIWF on government

³²This period matches the ACS analysis and approximates the March 2021–March 2024 boom period examined using QCEW data. Results for 2021–2024 are similar.

³³Non-homothetic preferences and housing as a necessity explain the fact, documented in Albouy et al. (2016) and Dustmann et al. (2022), that expenditure shares on housing fall with income.

³⁴Camarota and Zeigler (2026) provide a probabilistic estimate of welfare program utilization for unauthorized 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.

Table 9: Effects of Unauthorized Immigrant Worker Flows on Personal Income, 2021–2023

General Pull Shift-Share IV

	(1)	(2)	(3)	(4)	(5)	(6)
	Pers. Inc.	Pers. Inc. p.c.	Labor Inc.	Labor Inc. p.c.	Gov. Transfers	Govt. Transfers p.c.
UIWF	0.326 (0.983)	-0.154 (0.729)	-0.671 (0.704)	-1.151** (0.498)	-4.455*** (1.372)	-4.935*** (1.309)
Constant	0.121 (0.152)	0.266** (0.123)	-0.0844 (0.173)	0.0612 (0.162)	0.133 (0.170)	0.278* (0.162)
Observations	712	712	712	712	712	712
R ²	0.507	0.431	0.322	0.153	0.074	0.122
First-Stage Results:						
Coefficient Estimate	0.642***	0.642***	0.642***	0.642***	0.642***	0.642***
Coefficient Std. Error	(0.096)	(0.096)	(0.096)	(0.096)	(0.096)	(0.096)
F Statistic	44.11	44.11	44.11	44.11	44.11	44.11

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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

transfers to local residents, both in total and per capita. The estimates imply that increased UIWF equal to 1% of initial employment lowers government transfer payments for the average resident in the commuting zone by nearly 5%.

There are three possible explanations for these negative effects. 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 care than the general population. Second, due to their potentially ambiguous legal status, such individuals may be less likely 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. These first two explanations involve a change in the composition of the local population towards a lower share of transfer program recipients. Yet, we also find a reduction in non-per capita terms, which could reflect positive spillovers to local incumbents.^{35,36}

A caveat is that the income and transfer results differ across IV designs: ESS yields positive and significant estimates for all six outcomes (Appendix Table D11). We find, however, that these ESS estimates are highly sensitive to the inclusion of a single control variable, the pre-boom immigrant worker share. Omitting this control roughly halves the coefficients for total personal

³⁵We note that we also find a negative and significant effect of UIWF on transfers and transfers per capita for the 2021 to 2024 period. However, the coefficients are about half as large as those for the 2021 to 2023 period. 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.

³⁶For instance, Clemens et al. (2026) find that increases in asylum-seekers as a share of local population are associated with decreases in household survey reports of Medicaid and welfare program utilization by incumbent residents.

income, personal income per capita, and total labor income. The coefficients for labor income per capita and total government transfers become negative and insignificant, while the coefficient for transfers per capita flips from positive and significant to negative and significant. This sensitivity, together with the previously discussed identification concerns, concentrated Rotemberg weights, and influence of the Miami outlier, adds to our preference for the GPSS IV design.

7 Conclusion

From early 2021 to early 2024, the U.S. experienced a dramatic increase 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. Much of the prior research has 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 has left policymakers to rely on past studies on legal or total immigration, the effects of which could be quite different from 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 rigorous cross-sectional empirical designs that exploit plausibly exogenous variation across local markets. These strategies build on the conventional enclave shift-share instrument that has been popularized in prior studies of immigration.

On labor market effects, we first find that UIWF increased local employment approximately one-for-one during the 2021-2024 surge. These responses are robust to alternative empirical strategies, geographical aggregations, and survey-based outcome measures. Thus, the unauthorized immigration boom acted as a supply shock to local labor markets as a whole. For wages, the results are somewhat noisy and mixed but tend to point to a range of zero to modestly negative effects. Examining local housing markets, we find that UIWF increased house prices and rents, without significantly expanding new housing supply. Thus, the immigration boom likely acted not only as a local labor supply shock but also as a housing demand shock, boosting prices in the face of relatively inelastic short-run housing supply.

We also examine the effects of UIWF on key components of personal income. While we find modest and statistically insignificant effects on total and per capita personal income, we document significantly negative effects on labor income per capita, consistent with unauthorized immigrant workers earning lower wages, pushing down average labor income. Finally, we estimate that UIWF

significantly reduced local income received from government transfers, 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 immigrants.

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 and offer a data-driven approach for evaluating their economic implications.

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Online Appendix

A Theoretical Framework

Because UIWF generate simultaneous adjustments in local labor and housing markets, a theoretical framework is useful for interpreting the estimated responses of employment, wages, shelter prices, and housing supply jointly. We present a parsimonious framework that isolates the principal supply and demand channels linking the two markets. The model is deliberately stylized and abstracts from many dimensions of worker, industry, and location heterogeneity. These simplifying assumptions are used to clarify the underlying mechanisms, rather than being imposed on our empirical analysis.

Let I_d denote the cumulative UIWF over the boom period we consider, and $\bar{E}_d$ and $\bar{H}_d$ denote initial employment and housing stock, respectively, in destination d . To map model variables to our empirical specification, we define

$$m_d \equiv \frac{I_d}{\bar{E}_d}, \quad e_d \equiv \frac{\Delta E_d}{\bar{E}_d}, \quad h_d \equiv \frac{\Delta H_d}{\bar{H}_d},$$

and let $\hat{w}_d \equiv \Delta \log w_d$ and $\hat{r}_d \equiv \Delta \log r_d$ denote changes in the local nominal wage and price of housing services.³⁷ We take m_d as the local UIWF shock. Empirically, our instruments are designed to isolate exogenous variation in this shock, rather than reflecting contemporaneous local economic conditions.

Labor market. Let the linearized local labor demand be

$$e_d^D = \lambda m_d - \eta \hat{w}_d, \tag{5}$$

where $\eta > 0$ is the wage elasticity of labor demand. $\lambda \geq 0$ captures the outward shift in labor demand associated with UIWF, which may incorporate (i) immigrants' demand for locally produced goods and services, (ii) production complementarities, and (iii) other channels through which UIWF raise local output demand.

Local labor supply is

$$e_d^S = \rho m_d + \sigma (\hat{w}_d - \theta \hat{r}_d). \tag{6}$$

The parameter ρ maps a unit of UIWF into units of labor available for payroll employment over the estimation horizon. $\sigma \geq 0$ is the elasticity of labor supply with respect to the local real wage, and $\theta \hat{r}_d$ is the housing-cost component of the local cost of living. Higher rents reduce the attractiveness

³⁷The model is written in terms of the flow price of housing services. In the empirical analysis, we examine both market rents and house prices. House prices may respond through the capitalization of expected future rents.

of working in the destination, holding the nominal wage fixed.

Housing market. The housing market demand and supply satisfy:

$$h_d^D = \kappa m_d - \chi \widehat{r}_d, \quad h_d^S = \nu \widehat{r}_d,$$

where $\chi > 0$ and $\nu \geq 0$ are housing demand and supply elasticities. Because housing adjustment takes time, ν should be interpreted as the supply elasticity over the horizon of the empirical analysis. $\kappa > 0$ captures additional housing demand due to immigration inflows.

Local equilibrium. Equating housing demand and supply gives:

$$\widehat{r}_d = \frac{\kappa}{\chi + \nu} m_d, \quad h_d = \frac{\nu \kappa}{\chi + \nu} m_d. \quad (7)$$

Thus, UIWF raise rents whenever they raise housing demand. The division of adjustment between prices and quantities depends on the housing supply elasticity, ν . In destinations with less elastic supply, rents respond more.

Turning to the labor market equilibrium, $e_d^D = e_d^S = e_d$, we combine equations (5), (6) and (7) and obtain:

$$\widehat{w}_d = \frac{\lambda - \rho + \sigma \theta \kappa / (\chi + \nu)}{\eta + \sigma} m_d, \quad e_d = \frac{\sigma \lambda + \eta \rho - \eta \sigma \theta \kappa / (\chi + \nu)}{\eta + \sigma} m_d. \quad (8)$$

The nominal wage response is therefore theoretically ambiguous. The direct increase in labor supply, summarized by ρ , puts downward pressure on wages, while labor-demand and housing-demand effects put upward pressure on nominal wages required to attract workers. In a benchmark with limited labor-demand shift and housing-market spillovers ($\lambda \rightarrow 0$ and $\theta \rightarrow 0$), the model reduces to a conventional labor-supply shock and wages decline.

The equilibrium employment response in equation (8) shows that an employment coefficient near one need not be a mechanical accounting relationship. It potentially combines the direct employment of arriving immigrants, the increase in labor demand generated by local expenditures or production complementarities, and the feedback from rising housing costs. The strength of these indirect effects combined is an empirical question.

The framework can rationalize the combination of outcomes examined: employment rises approximately in proportion to UIWF even when nominal wage responses are modest; while shelter prices increase, housing supply adjusts little over the horizon studied. It also generates two heterogeneity predictions. First, rent effects should be larger in destinations with less elastic housing supply. Second, if immigrant expenditure is an important component of λ , UIWF generate an outward demand shift in locally produced nontradables. Relatively strong employment responses

in these industries (such as Leisure & Hospitality) would therefore be consistent with, though would not by themselves identify, the local-expenditure channel.

B Construction of Net Unauthorized Immigration

Entry categories. As shown in panel (a) of Figure B1, 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 B1); 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 B1, 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 and among Cubans, Haitians, Nicaraguans, and Venezuelans (CHNV).

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):

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

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

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

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 $I_{o,..,t} := \sum_d I_{o,d,t}$.

- **Inflows:** Let $S_{o,t} = \frac{I_{o,..,t}^{NTA}}{I_{o,..,t}}$ 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

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

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

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

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 exits constructed earlier.

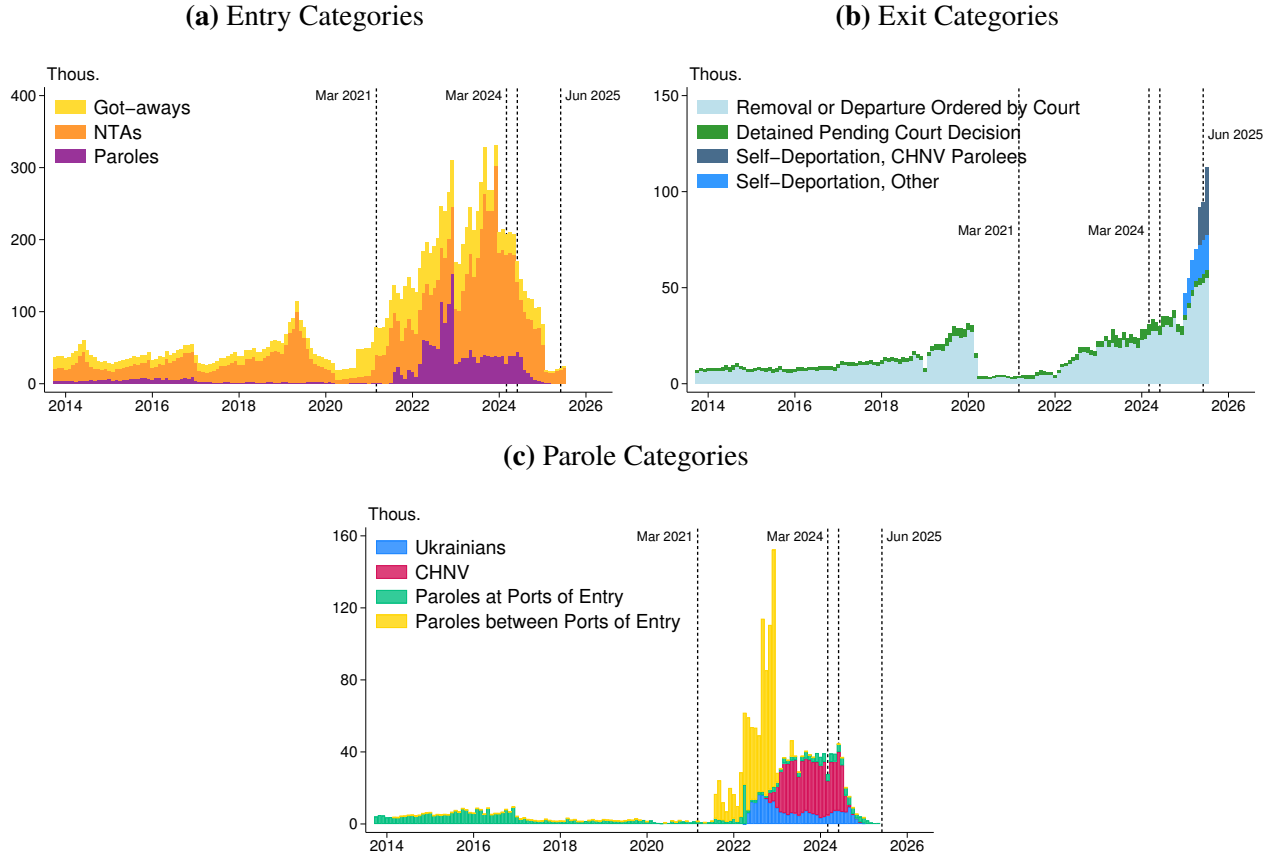
Analogous to the national outflows, let $E_{o,t}$ be the origin-specific effective monthly voluntary exit rate, which can be computed using our data. We construct outflows from origin o and geographic unit d as

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

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

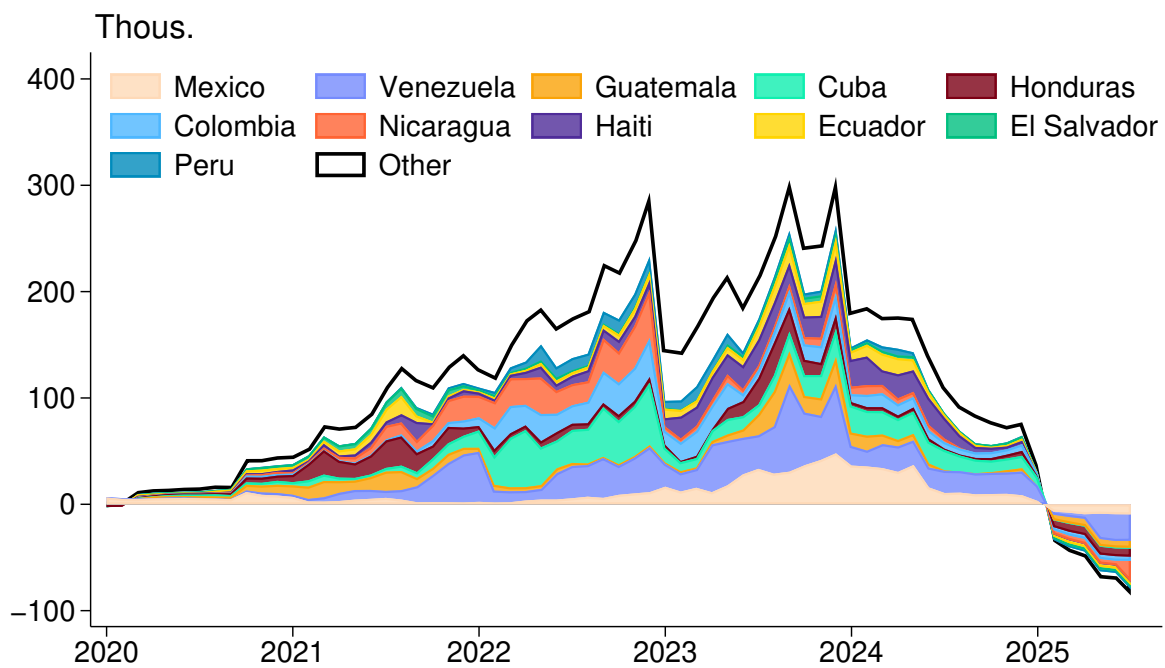
- **Local net unauthorized immigration:** Taken together, $N_{.,d,t} = \sum_o (I_{o,d,t} - O_{o,d,t})$. This approach ensures that the sum of net unauthorized immigration, as well as its components, across all locations matches the national total and origin-specific net flows.

Figure B1: Monthly Net Unauthorized Immigration



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 B2: Net Unauthorized Immigration By Country



Notes: Country-specific net unauthorized immigration is constructed in the same way as the national aggregate. The black line denotes total net unauthorized immigration; colored areas denote origin-country contributions.

C Historical Predicted-Ancestry Instrument

As a complementary identification strategy, we implement the full historical predicted-ancestry approach of [Burchardi et al. \(2019\)](#) and [Terry et al. \(2026\)](#). As discussed in Section 4, our preferred instrument uses recent pre-boom settlement patterns to estimate origin-specific loadings on generic destination pull, whereas their approach constructs predicted ancestry exposure from repeated historical push-pull interactions spanning more than a century of U.S. immigration through 2010.

Specifically, let $A_{o,d}^{2010}$ denote the number of residents in destination d in 2010 with ancestry from origin o . We first predict $A_{o,d}^{2010}$ using historical push-pull interactions by estimating

$$A_{o,d}^{2010} = \delta_o + \delta_d + \sum_{t=1880}^{2000} \alpha_t I_{o,-r(d),t} \times \frac{I_{-c(o),d,t}}{I_{-c(o),.,t}} + \mathbf{x}_{o,d}\boldsymbol{\gamma} + \epsilon_{o,d}, \quad (9)$$

where $I_{o,-r(d),t}$ denotes immigration from origin o to the U.S. at time t , excluding the census region containing destination d . $\frac{I_{-c(o),d,t}}{I_{-c(o),.,t}}$ denotes the share of immigrants from origins outside o 's continent who settle in destination d at time t . The specification also includes the fixed effects and geographic controls used by [Burchardi et al. \(2019\)](#) and [Terry et al. \(2026\)](#).

Predicted ancestry is constructed using only the residualized historical push-pull interactions:

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

where $\perp$ indicates that the term has been residualized with respect to the controls in equation (9). We then construct the cross-sectional instrument by interacting predicted ancestry with the same leave-one-out origin-specific UIWF used in our preferred specification:

$$\text{Predicted-Ancestry-SSIV}_{d,T} = \sum_o \left(I_{o,-d,T} \times \hat{A}_{o,d}^{2010} \times \frac{1}{\bar{E}_d} \right). \quad (10)$$

We refer to this instrument as the predicted-ancestry leave-out SSIV (PALO-SSIV). This instrument provides a complementary source of identification based on long-run historical push-pull variation. As discussed in Section 5, it nevertheless yields similar conclusions to our preferred approach for the key outcomes.

D Additional Empirical Results

D.1 All and All Working-Age Unauthorized Immigrant Flows

Table D1: IV Estimates of Labor Market Impacts for Boom Period: 2021m3–2024m3

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

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS, Emp.	IV(ESS), Emp.	IV(GPSS), Emp.	OLS, Wages	IV(ESS), Wages	IV(GPSS), Wages
d_imm_wa	0.588*** (0.100)	0.663*** (0.176)	0.678*** (0.192)	-0.0982 (0.138)	0.887*** (0.138)	-0.661 (0.424)
Constant	-0.130 (0.113)	-0.0984 (0.111)	-0.103 (0.123)	0.0877 (0.121)	0.199* (0.118)	-0.0825 (0.192)
Observations	721	721	721	720	720	720
R ²	0.581	0.580	0.579	0.636	0.607	0.578
First-Stage Results:						
Coefficient Estimate		0.924***	1.341***		0.924***	1.341***
Coefficient Std. Error		(0.112)	(0.249)		(0.112)	(0.249)
F Statistic		67.99	28.87		67.97	28.88

(b) *All* Unauthorized Immigrant Flows

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS, Emp.	IV(ESS), Emp.	IV(GPSS), Emp.	OLS, Wages	IV(ESS), Wages	IV(GPSS), Wages
d_imm	0.470*** (0.0804)	0.538*** (0.142)	0.543*** (0.150)	-0.0660 (0.106)	0.719*** (0.114)	-0.530 (0.335)
Constant	-0.121 (0.114)	-0.0860 (0.111)	-0.0919 (0.124)	0.0911 (0.120)	0.216* (0.115)	-0.0935 (0.201)
Observations	721	721	721	720	720	720
R ²	0.583	0.581	0.581	0.636	0.607	0.573
First-Stage Results:						
Coefficient Estimate		1.140***	1.673***		1.140***	1.673***
Coefficient Std. Error		(0.149)	(0.305)		(0.149)	(0.305)
F Statistic		58.22	29.98		58.20	29.98

Notes: Unauthorized immigrant flows (as share of initial employment) are 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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D.2 Specification Tests for Alternative Empirical Designs

Table D2: Rotemberg Weights and Over-ID Test: Leave-Out Ancestry IV

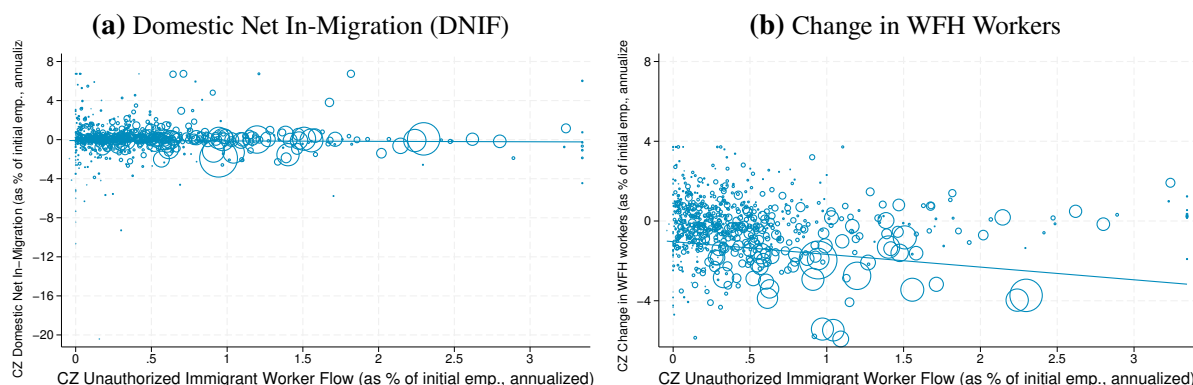
Panel A. Rotemberg weights	Rotemberg weight ($\hat{\alpha}_k$)	Just-identified estimate ($\hat{\beta}_k$)
Mexico	0.674	2.553
Cuba	0.193	2.034
Guatemala	0.044	2.518
Colombia	0.035	2.490
Haiti	0.020	2.629
Honduras	0.019	2.544
El Salvador	0.015	2.446
Peru	0.009	2.878
Venezuela	0.002	13.577
Ecuador	-0.000	-31.944
Nicaragua	-0.008	1.054
Panel B. Over-ID test		
Baseline IV estimate ($\hat{\beta}$)	2.499***	
$Se(\hat{\beta})$	(0.822)	
First-Stage F	6.80	
Over-identified IV estimate ($\hat{\beta}^{OID}$)	1.484***	
$Se(\hat{\beta}^{OID})$	(0.272)	
Over-identification test p-value	0.23	

Notes: Rotemberg weights computed as in [Goldsmith-Pinkham et al. \(2020\)](#) for the leave-out ancestry IV, which follows [Terry et al. \(2026\)](#) as described in Section C. $\hat{\beta}_k$ is the just-identified IV estimate using only the share associated with a given country as the instrument. Stars denote significance from the standard normal (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

D.3 Additional Specification Checks

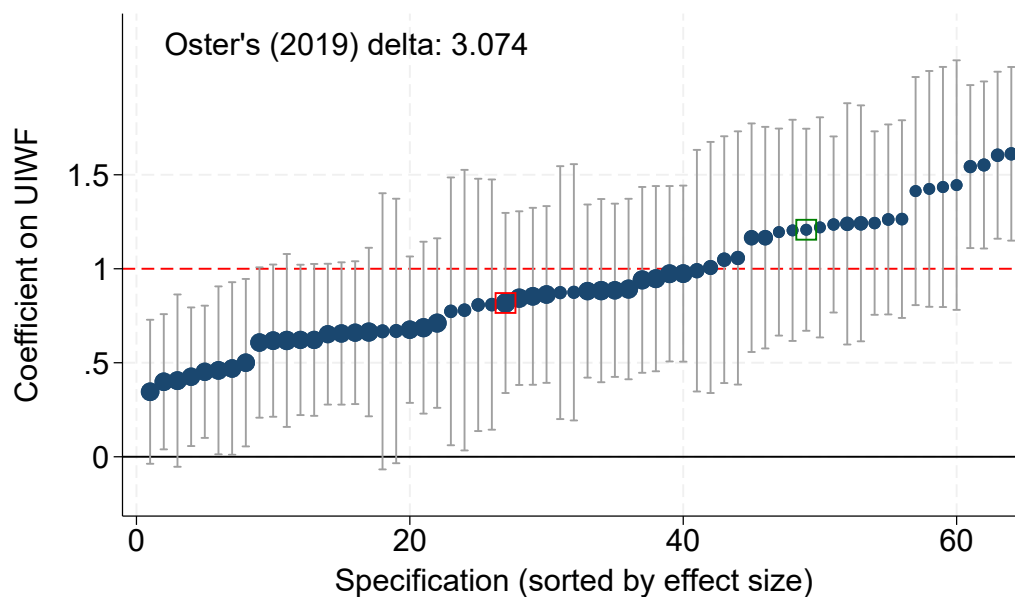
Figure D1: DNIF and WFH vs. Immigrant Worker Flows Across CZs

Boom Period: 2021m3–2024m3



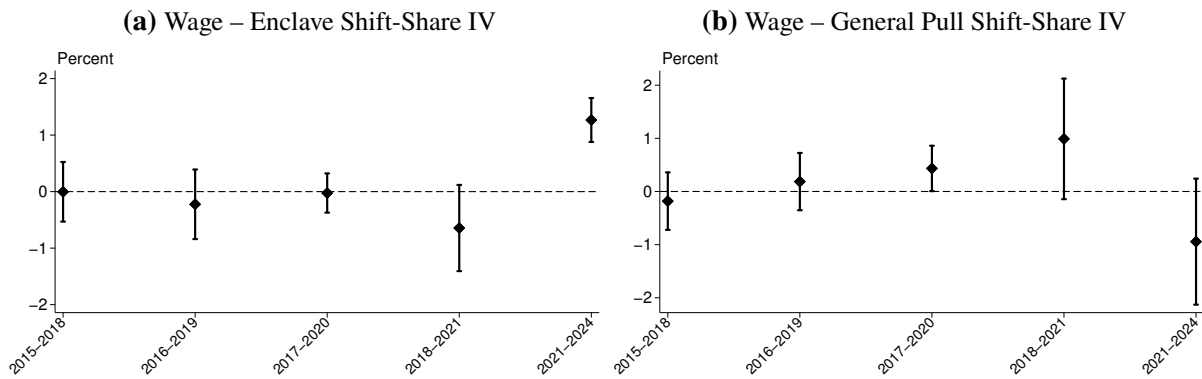
Notes: Each circle represents a commuting zone (CZ); circle size is proportional to CZ beginning-of-period employment. Panel (a) plots domestic net in-migration (DNIF) and panel (b) plots the change in work-from-home (WFH) workers over 2021–2024, each against UIWF. All variables are annualized and scaled by beginning-of-period employment. The fitted lines are employment-weighted OLS fits.

Figure D2: Specification Curve: UIWF Effect on Employment – IV (Enclave Shift-Share)



Notes: Analogous to panel (b) of Figure 7, using the ESS IV. The specifications consist of all combinations of the following six sets of controls (1) industry shares, (2) age population shares, (3) white population share, (4) education population shares, (5) COVID (2020m2–2021m2) employment growth, and (6) domestic net in-migration (DNIF) and the change in WFH workers over 2021–2024, each scaled by initial employment. All ESS specifications include the re-centering control. The green square identifies the specification with no controls, while the red square identifies the specification with full controls. Circle size is proportional to the R^2 .

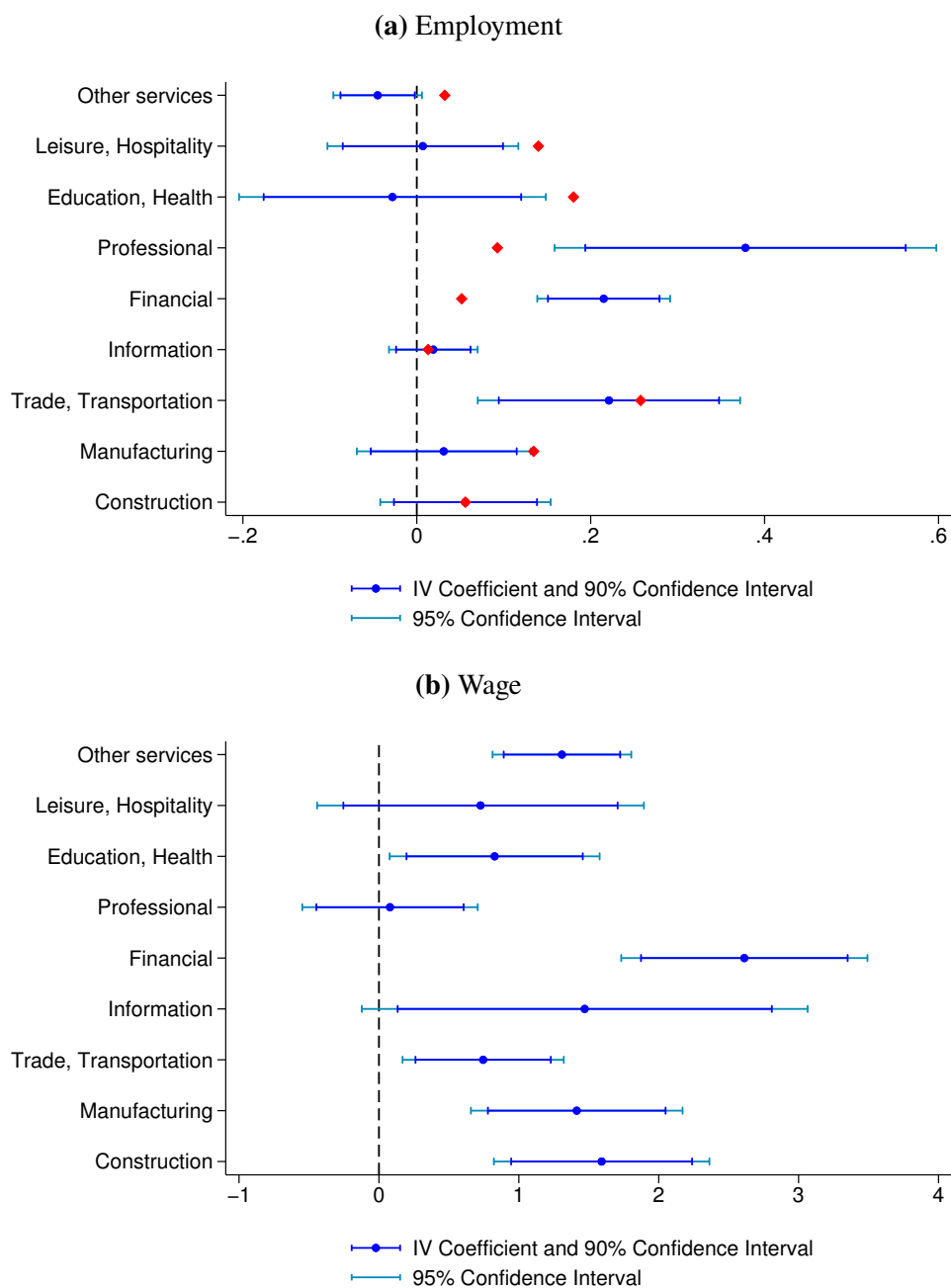
Figure D3: Placebo Regressions Using Prior 3-Year Periods: Wages



Notes: Each figure plots the UIWF coefficient (diamond) and 95% confidence interval (bracket) from a cross-CZ IV regression of average weekly wage growth on the same set of regressors and instruments as used in our baseline boom-period specifications. Panel (a) uses the leave-out (enclave) shift-share IV; panel (b) uses the predicted-settlement (general pull) shift-share IV. Analogous placebo regressions for employment growth are shown in Figure 6.

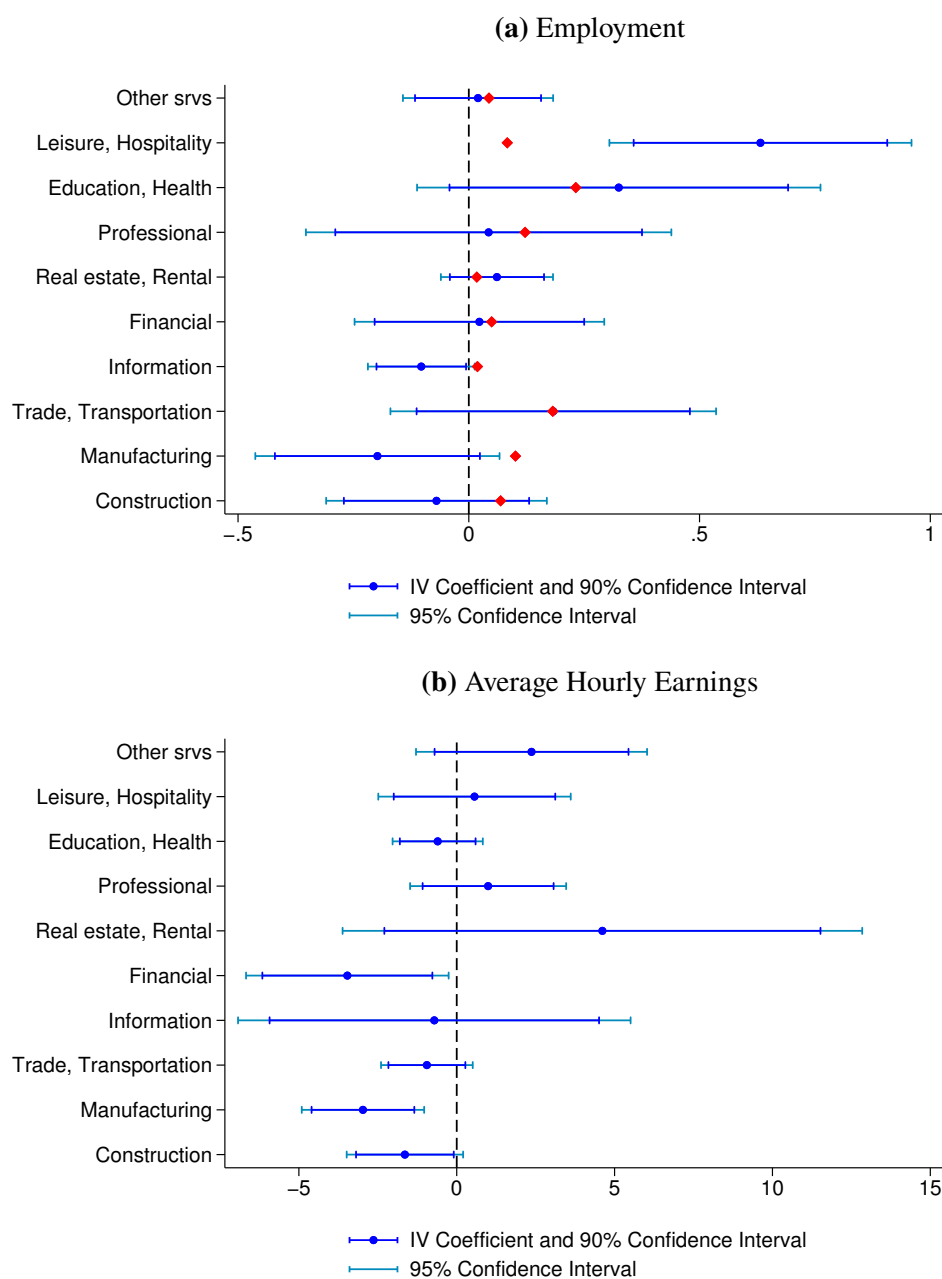
D.4 Alternative Industry-Specific Labor Market Results

Figure D4: ESS IV Employment and Wage Estimates by Industry Using QCEW, 2021m3–2024m3



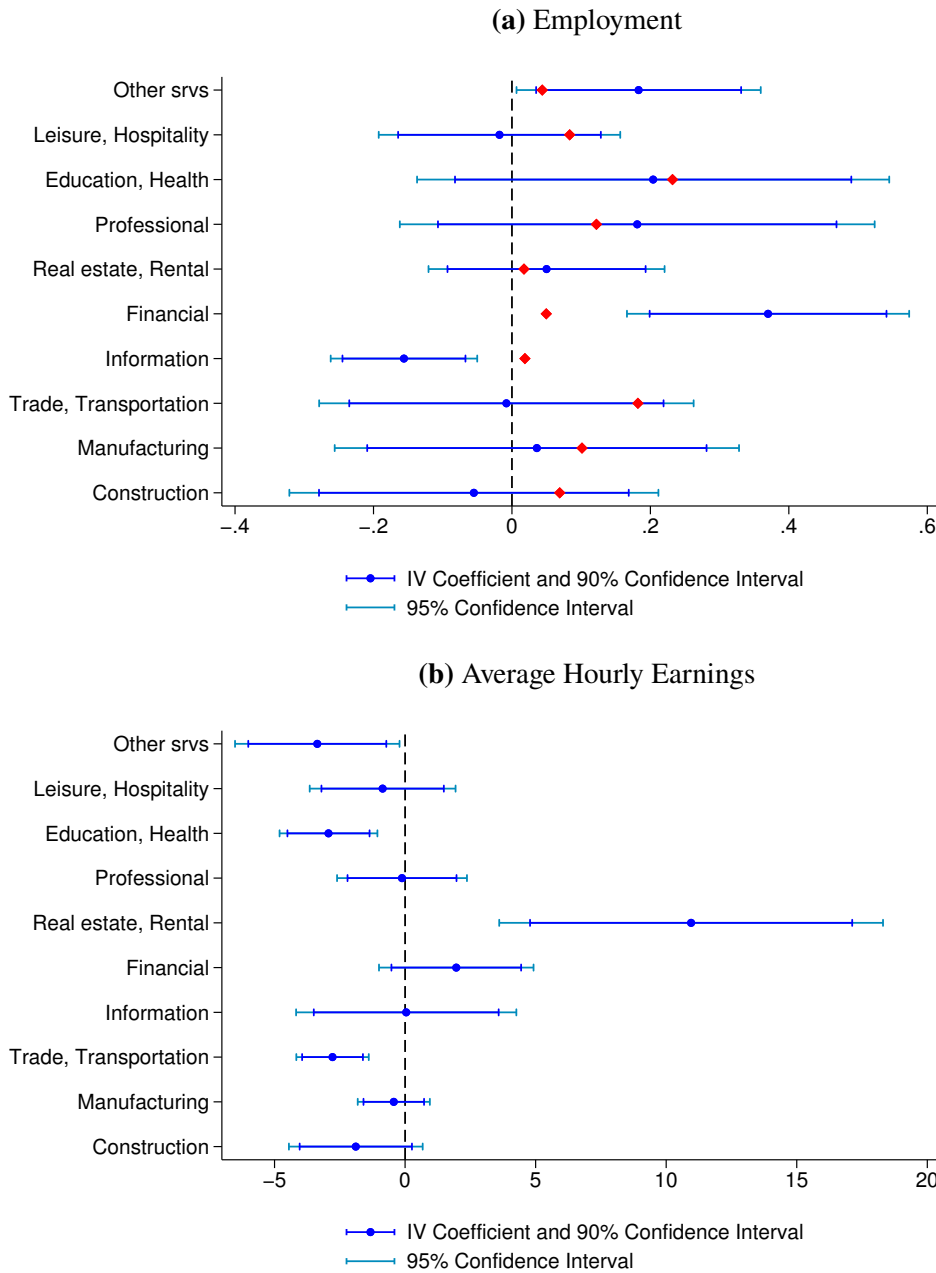
Notes: Analogous to Figure 8, using the enclave shift-share IV. UIWF (as share of initial employment) is winsorized at 99th percentile due to outliers. Observations are weighted by beginning-of-period total employment. Brackets indicate the 95% confidence intervals based on standard errors that are heteroskedasticity-robust and clustered on state. Red diamonds denote each industry's average 2015–2019 employment share.

Figure D5: GPSS IV Employment and Wage Estimates by Industry Using ACS, 2021–2023



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. Red diamonds denote each industry's average 2015–2019 employment share.

Figure D6: ESS IV Employment and Wage Estimates by Industry Using ACS, 2021–2023



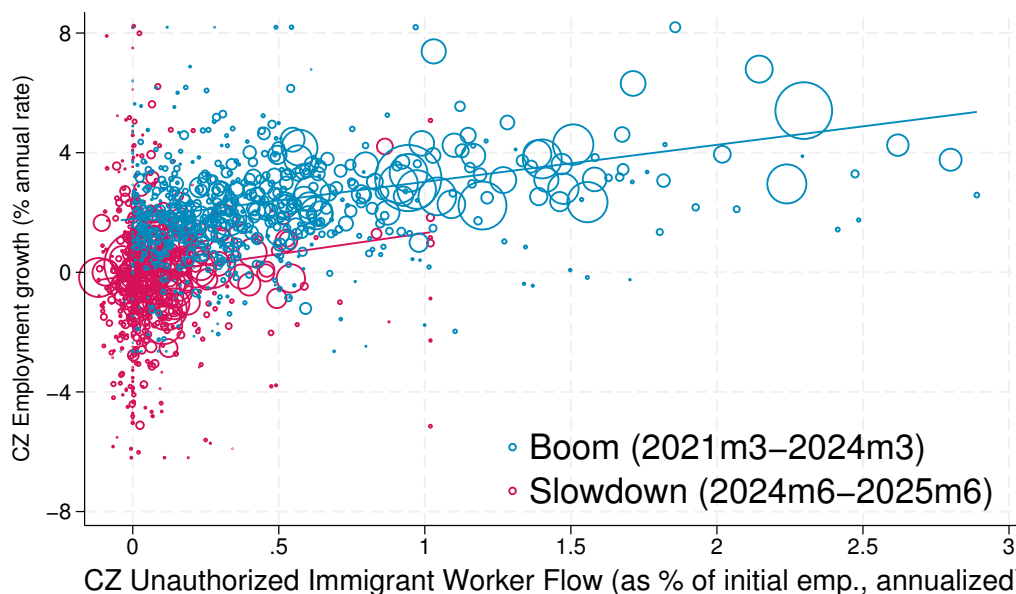
Notes: UIWF (as share of initial employment) is winsorized at 99th percentile due to outliers. Observations are weighted by beginning-of-period total employment. Brackets indicate the 95% confidence intervals based on standard errors that are heteroskedasticity-robust and clustered on state. Red diamonds denote each industry's average 2015–2019 employment share.

D.5 Employment and Wage Impacts During the Slowdown Period

Net unauthorized immigration to the U.S. peaked in early 2024 and then fell sharply, becoming negative by early 2025 (as shown in Figure 3). Here we examine whether the relationship between UIWF and labor market outcomes during the slowdown period—June 2024 to June 2025—was similar to that during the boom period.

Figure D7 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. Second, the correlations between UIWF and employment growth, as measured by the slope of the OLS linear fit lines shown in the figure, were 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 D7: Employment Growth vs. Immigrant Worker Flows Across CZs



Notes: Each circle represents a commuting zone (CZ), with size proportional to its initial employment.

Table D3 shows the OLS and IV estimates of β_1 from estimating equation (1) for the slowdown period for both employment and wage growth. The effects of UIWF turn out to be much less precisely estimated than for the boom period, as reflected by the large standard errors. The preferred GPSS IV point estimate for employment growth is sizable, but statistically insignificant. The GPSS IV estimate for wage growth is also statistically insignificant. The greater imprecision of these estimates compared to those for the boom period likely stems in part from the lower first-stage relevance of the enclave shift-share instruments. One possible explanation is that immigrant

Table D3: Effect of Unauthorized Immigrant Worker Flows on Employment and Wages

Slowdown Period: 2024m6-2025m6

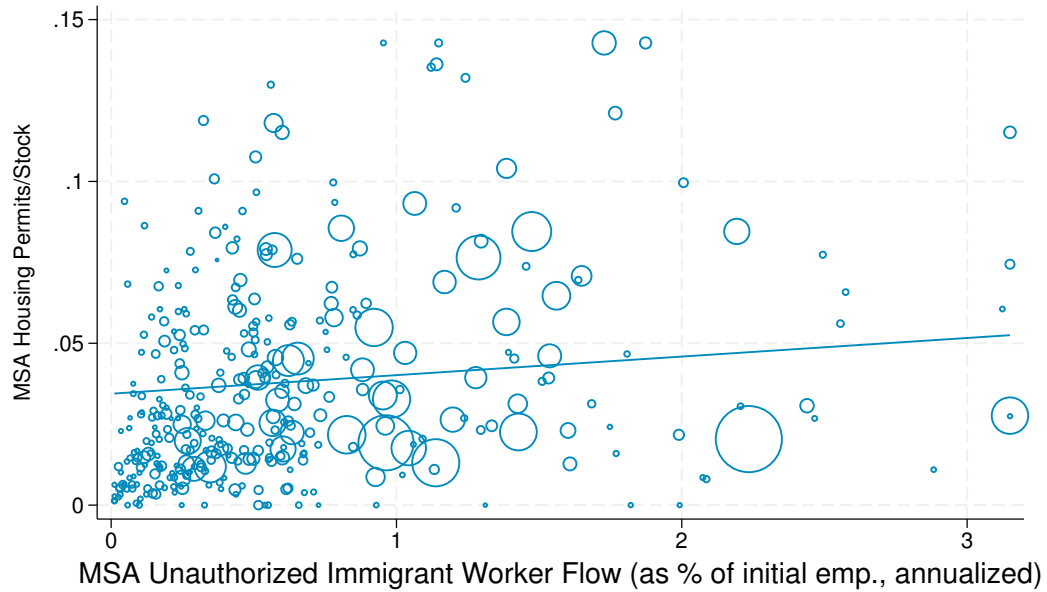
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS, Emp.	IV(ESS), Emp.	IV(GPSS), Emp.	OLS, Wages	IV(ESS), Wages	IV(GPSS), Wages
UIWF	0.650 (0.417)	0.312 (0.659)	2.892 (1.995)	0.195 (0.531)	2.469** (1.089)	-2.039 (4.066)
Constant	-0.0686* (0.0374)	-0.0663* (0.0379)	-0.0491 (0.0422)	0.0887 (0.0553)	0.106** (0.0510)	0.0692 (0.0517)
Observations	721	721	721	720	720	720
R^2	0.162	0.162	0.090	0.096	0.048	0.049
First-Stage Results:						
Coefficient Estimate		0.734***	2.186**		0.734***	2.186**
Coefficient Std. Error		(0.107)	(0.871)		(0.107)	(0.871)
F Statistic		46.38	6.292		46.37	6.291

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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

networks are better predictors of immigrant entries, which dominate net flows during the boom period, than of exits, which become increasingly important during the slowdown period.

D.6 Scatter Plot of New Housing Supply Against UIWF

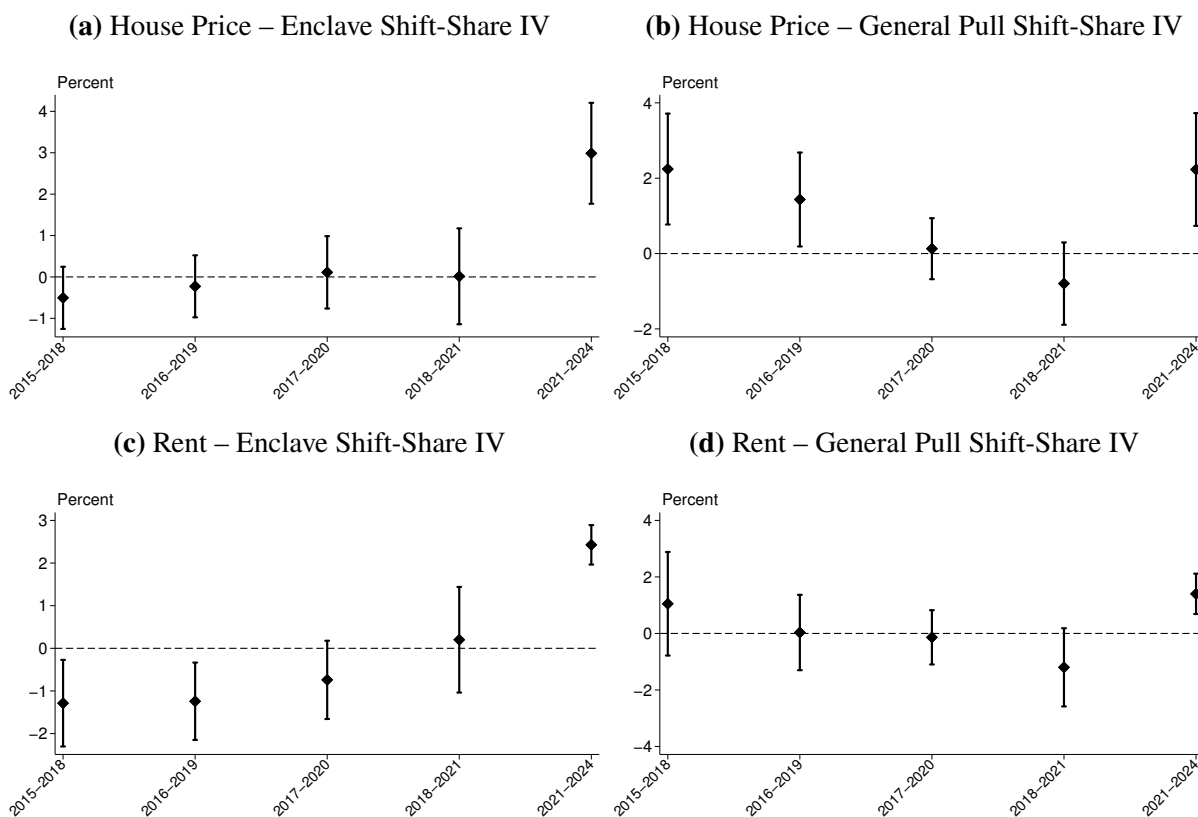
Figure D8: New Housing Supply vs. UIWF Across MSAs, 2021m3–2024m3



Notes: Each circle represents an MSA. Circle size is proportional to MSA beginning-of-period employment. New housing supply is measured as the ratio of new housing permits issued over the boom period to the initial stock of existing housing units (see text for details).

D.7 Placebo Regressions for Housing Outcomes

Figure D9: Placebo Regressions for Housing Using Prior 3-Year Periods



Notes: Each figure plots the UIWF coefficient (diamond) and 95% confidence interval (bracket) from a cross-MSA IV regression of either Zillow house price growth (panels a–b) or Zillow rent growth (panels c–d) on the same set of regressors and instruments as used in our baseline boom-period MSA-level housing specifications. The left column uses the ESS IV; the right column uses the GPSS IV.

D.8 Results Using CoreLogic and Freddie Mac House Price Indexes

Table D4: Effect of Unauthorized Immigrant Worker Flows on House Prices, 2021m3–2024m3

(a) Shelter Prices – OLS		
	(1) CoreLogic HP	(2) Freddie Mac HP
UIWF	0.615 (0.509)	0.972* (0.500)
Observations	333	341
R^2	0.334	0.466

(b) Shelter Prices – General Pull Shift-Share IV		
	(1) CoreLogic HP	(2) Freddie Mac HP
UIWF	0.642 (1.117)	1.172 (0.838)
Observations	333	341
R^2	0.334	0.464
First-Stage Results:		
Coefficient Estimate	0.493***	0.778***
Coefficient Std. Error	(0.129)	(0.216)
F Statistic	14.47	12.90

(c) Shelter Prices – Enclave Shift-Share IV		
	(1) CoreLogic HP	(2) Freddie Mac HP
UIWF	2.627*** (0.451)	3.638*** (0.531)
Observations	333	341
R^2	0.184	0.206
First-Stage Results:		
Coefficient Estimate	0.636***	0.471***
Coefficient Std. Error	(0.063)	(0.045)
F Statistic	100.1	108.9

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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D.9 Rent and Housing Supply Results by Structure Type

Table D5: Effect of Unauthorized Immigrant Worker Flows on Rents

Boom Period: 2021m3–2024m3

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS, SF Rent	IV(ESS), SF Rent	IV(GPSS), SF Rent	OLS, MF Rent	IV(ESS), MF Rent	IV(GPSS), MF Rent
UIWF	1.133*** (0.294)	2.786*** (0.338)	1.222** (0.541)	1.608*** (0.186)	1.959*** (0.274)	1.432*** (0.300)
Observations	233	233	233	217	217	217
R ²	0.506	0.328	0.506	0.534	0.529	0.532
First-Stage Results:						
Coefficient Estimate		0.479***	0.765***		0.474***	0.769***
Coefficient Std. Error		(0.049)	(0.220)		(0.052)	(0.216)
F Statistic		94.02	12.06		82.31	12.64

Notes: OLS and IV estimates of the effect of UIWF on Zillow single-family (SF) and multi-family (MF) rent growth over the boom period. The IV columns use the ESS and GPSS instruments. All columns use the full sample; for the ESS instrument, the single most extreme observation (the Miami MSA) is capped at the second-largest instrument value. 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.

Table D6: Effect of Unauthorized Immigrant Worker Flows on Housing Supply

Boom Period: 2021m3–2024m3

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS, SF	IV(ESS), SF	IV(GPSS), SF	OLS, MF	IV(ESS), MF	IV(GPSS), MF
UIWF	0.0319 (0.0948)	-0.0707 (0.220)	-0.230 (0.174)	0.126*** (0.0379)	0.151 (0.0988)	0.00605 (0.0685)
Observations	341	341	341	341	341	341
R ²	0.514	0.511	0.478	0.520	0.527	0.498
First-Stage Results:						
Coefficient Estimate		0.471***	0.778***		0.471***	0.778***
Coefficient Std. Error		(0.045)	(0.216)		(0.045)	(0.216)
F Statistic		108.9	12.90		108.9	12.90

Notes: New housing supply is measured as cumulative permits over the period divided by initial housing units, separately for single-family (SF) and multi-family (MF) units. OLS and IV estimates; the IV columns use the ESS and GPSS instruments. All columns use the full sample; for the ESS instrument, the single most extreme observation (the Miami MSA) is capped at the second-largest instrument value. 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.

D.10 Validation of Empirical Design for Housing Outcomes

Table D7: Validation of Empirical Design: House Prices (GPSS IV)

Panel A. Rotemberg weights	Rotemberg weight ($\hat{\alpha}_k$)		Just-identified estimate ($\hat{\beta}_k$)
	All countries	Excl. Other	
Other	0.245		2.481
Venezuela	0.137	0.182	1.792
Nicaragua	0.101	0.134	1.947
Colombia	0.094	0.124	1.823
Haiti	0.092	0.122	2.307
Cuba	0.082	0.109	1.614
Ecuador	0.077	0.101	2.089
Guatemala	0.041	0.054	1.790
Mexico	0.041	0.054	1.779
Peru	0.040	0.053	1.898
Honduras	0.035	0.046	1.828
El Salvador	0.016	0.021	1.808

Panel B. Over-ID test			
	All countries	Excl. Other	
Baseline IV estimate ($\hat{\beta}$)	2.231	2.059	
Over-identified IV estimate ($\hat{\beta}^{OID}$)	1.769	1.695	
Over-identification test p-value	0.23	0.19	

Notes: Rotemberg weights are computed following [Goldsmith-Pinkham et al. \(2020\)](#) for the MSA-level IV regression of Zillow house price growth on UIWF during the boom period (2021m3–2024m3), using the predicted-settlement IV. $\hat{\beta}_k$ is the just-identified IV estimate using the pre-existing immigrant shares associated with a given country. Standard errors are clustered on state.

Table D8: Validation of Empirical Design: House Prices (Enclave Shift-Share IV)

Panel A. Rotemberg weights	Rotemberg weight ($\hat{\alpha}_k$)		Just-identified estimate ($\hat{\beta}_k$)
	All countries	Excl. Other	
Venezuela	0.324	0.325	3.092
Cuba	0.282	0.282	3.644
Haiti	0.192	0.193	2.442
Colombia	0.122	0.123	2.411
Ecuador	0.063	0.064	-0.558
Nicaragua	0.062	0.061	2.795
Honduras	0.020	0.020	2.054
Peru	0.020	0.020	0.702
Other	0.003		-43.511
El Salvador	-0.011	-0.011	0.991
Guatemala	-0.014	-0.014	-1.248
Mexico	-0.063	-0.064	1.765

Panel B. Over-ID test			
	All countries	Excl. Other	
Baseline IV estimate ($\hat{\beta}$)	2.987	3.044	
Over-identified IV estimate ($\hat{\beta}^{OID}$)	1.666	1.705	
Over-identification test p-value	0.19	0.31	

Notes: Rotemberg weights are computed following [Goldsmith-Pinkham et al. \(2020\)](#) for the MSA-level IV regression of Zillow house price growth on UIWF during the boom period (2021m3–2024m3), using the ESS IV. $\hat{\beta}_k$ is the just-identified IV estimate using the pre-existing immigrant shares associated with a given country with leave-out adjustments. Standard errors are clustered on state.

Table D9: Validation of Empirical Design: Rent (GPSS IV)

Panel A. Rotemberg weights	Rotemberg weight ($\hat{\alpha}_k$)		Just-identified estimate ($\hat{\beta}_k$)
	All countries	Excl. Other	
Other	0.240		1.395
Venezuela	0.137	0.180	1.584
Nicaragua	0.102	0.134	1.584
Colombia	0.095	0.124	1.496
Haiti	0.093	0.123	1.702
Cuba	0.082	0.108	1.447
Ecuador	0.077	0.101	1.554
Mexico	0.042	0.056	1.915
Guatemala	0.041	0.054	1.544
Peru	0.040	0.053	1.543
Honduras	0.035	0.046	1.529
El Salvador	0.016	0.021	1.594

Panel B. Over-ID test			
	All countries	Excl. Other	
Baseline IV estimate ($\hat{\beta}$)	1.401	1.442	
Over-identified IV estimate ($\hat{\beta}^{OID}$)	1.991	1.986	
Over-identification test p-value	0.37	0.29	

Notes: Rotemberg weights are computed following [Goldsmith-Pinkham et al. \(2020\)](#) for the MSA-level IV regression of Zillow rent growth (SF+MF) on UIWF during the boom period (2021m3–2024m3), using the predicted-settlement IV. $\hat{\beta}_k$ is the just-identified IV estimate using the pre-existing immigrant shares associated with a given country. Standard errors are clustered on state.

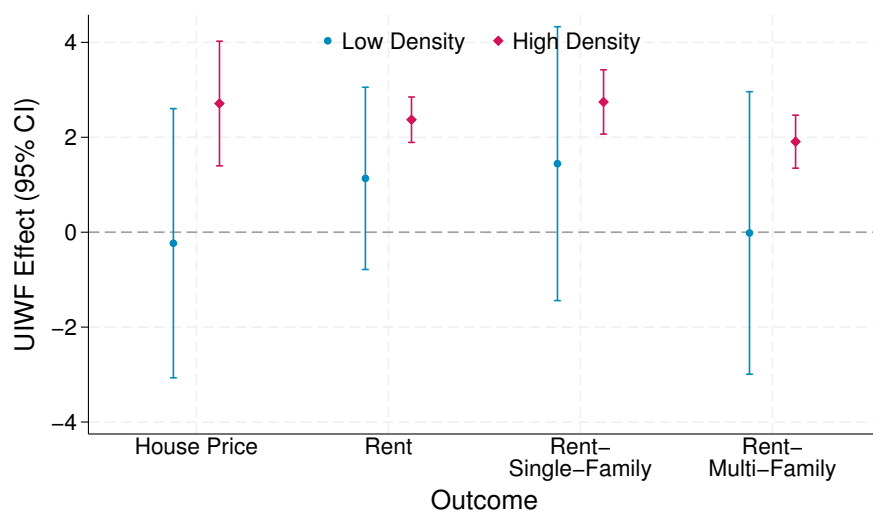
Table D10: Validation of Empirical Design: Rent (Enclave Shift-Share IV)

Panel A. Rotemberg weights	Rotemberg weight ($\hat{\alpha}_k$)		Just-identified estimate ($\hat{\beta}_k$)
	All countries	Excl. Other	
Venezuela	0.323	0.325	2.239
Cuba	0.279	0.281	2.714
Haiti	0.193	0.195	2.777
Colombia	0.124	0.125	3.097
Ecuador	0.066	0.066	3.590
Nicaragua	0.060	0.060	0.925
Peru	0.020	0.020	3.520
Honduras	0.020	0.020	1.734
Other	0.005		4.383
El Salvador	-0.011	-0.011	2.782
Guatemala	-0.015	-0.015	2.237
Mexico	-0.064	-0.065	2.745

Panel B. Over-ID test			
	All countries	Excl. Other	
Baseline IV estimate ($\hat{\beta}$)	2.427	2.464	
Over-identified IV estimate ($\hat{\beta}^{OID}$)	2.813	2.825	
Over-identification test p-value	0.29	0.61	

Notes: Rotemberg weights are computed following [Goldsmith-Pinkham et al. \(2020\)](#) for the MSA-level IV regression of Zillow rent growth (SF+MF) on UIWF during the boom period (2021m3–2024m3), using the ESS IV. $\hat{\beta}_k$ is the just-identified IV estimate using the pre-existing immigrant shares associated with a given country with leave-out adjustments. Standard errors are clustered on state.

Figure D10: Heterogeneous Shelter Price Responses—ESS IV, 2021m3–2024m3



Notes: Analogous to Figure 11, using the enclave shift-share IV. 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.

D.11 Alternative Income and Transfer Results

Table D11: Effects of Unauthorized Immigrant Worker Flows on Personal Income: OLS and Enclave Shift-Share IV, 2021–2023

(a) OLS

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

(b) Enclave Shift-Share IV

	(1)	(2)	(3)	(4)	(5)	(6)
	Pers. Inc.	Pers. Inc. p.c.	Labor Inc.	Labor Inc. p.c.	Gov. Transfers	Govt. Transfers p.c.
UIWF	2.826*** (0.524)	1.391*** (0.323)	1.893*** (0.330)	0.457** (0.216)	2.889*** (0.797)	1.454** (0.729)
Constant	0.221** (0.113)	0.327*** (0.0898)	0.0251 (0.124)	0.130 (0.136)	0.440*** (0.159)	0.545*** (0.130)
Observations	712	712	712	712	712	712
R^2	0.399	0.378	0.348	0.255	0.326	0.477
First-Stage Results:						
Coefficient Estimate	0.487***	0.487***	0.487***	0.487***	0.487***	0.487***
Coefficient Std. Error	(0.056)	(0.056)	(0.056)	(0.056)	(0.056)	(0.056)
F Statistic	74.26	74.26	74.26	74.26	74.26	74.26

Notes: OLS and Enclave Shift-Share IV counterparts to the General Pull Shift-Share IV results in Table 9. 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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.