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How Did the Post-Pandemic Migration Surge Affect Labor and Housing Markets?

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ABSTRACT:

In the wake of the pandemic, the United States experienced an unprecedented inflow of about 6.5 million migrants who entered outside of usual legal pathways. This 2021-2024 migration surge boosted employment by about 0.4 jobs for each surge migrant and raised average metropolitan area GDP by 1.5%. Although the surge may have reduced average wages due in part to compositional effects, it raised U.S. natives' wages 0.9% over the period. The surge left native employment rates unaffected, but increased natives' rent by 1.6%. Declines in net domestic in-migration almost wholly offset the surge migration in the average metro, suggesting surge migration's effect on wages and rents stemmed from changes in skill mix and neighborhood residential patterns. The response of domestic migration implies the surge migration impacts on affected metros came to be diffused across larger geographies over time and were no longer detectable by cross-metro comparisons. Meanwhile, the short-run (year-over-year) responsiveness of native wages and rents declined over 2021-2024 as the economy adjusted to the ongoing inflows.

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Immigrant flows into the United States began surging in 2021 as the COVID pandemic receded, causing the foreign-born population share to reach its highest rate in over a century. Many of the migrants were allowed to enter through various humanitarian programs or after being given a notice to appear in immigration court. In total, at least 6.5 million migrants entered the country outside of usual legal pathways between 2021 and 2024, adding at least 2.0% to the 2021 population (Congressional Budget Office 2026, Edelberg et al. 2026). Unlike waves of migrants in the past who entered without permission or inspection, many of the new migrants were able to receive a work permit by filing an asylum application or because they were eligible for a humanitarian program that included employment eligibility.

The migration wave's effects on macroeconomic aggregates were sizeable and significant, although hard to initially detect given the US economy was in the midst of a steep recovery from the pandemic recession, and official government data initially failed to document the unexpected rise in migration inflows that began in 2021.[1] Estimates suggest that the "extra" immigration, which persisted for three years and greatly fueled labor supply, lifted potential monthly job growth by at least 70,000 in 2022 and 100,000 in each of 2023 and 2024 (Edelberg and Watson 2024; Foote 2024). The migration wave also boosted aggregate demand, lifting annual spending and GDP growth by 0.1 to 0.3 percentage points (CBO 2024; Edelberg and Watson 2024). Migration-induced increases in aggregate demand and supply are believed to have roughly canceled each other out in terms of price effects, leaving inflation unchanged (Cheremukhin et al. 2025).

This study examines how these "surge migrants" affected labor and housing markets in major metro areas across the United States in 2021-2024. Most of the surge migrants went to large cities, and the busing campaign conducted by Texas and Arizona to several cities created a novel exogenous source of variation in migrant destinations, which we utilize in addition to a conventional shift-share instrument. Our contribution relative to two working papers on the same topic (Clemens et al. 2026; Wilson and Zhou 2026) is the use of the "buslift" instrument, as well as a careful analysis of the timing of impacts over 2021-2024 and the study of domestic

[1] The exception was CBO (2024), who incorporated Department of Homeland Security (DHS) data on border encounters, asylum seekers, parolees, and gotaways in developing their estimates of 2023 net immigration. Regarding the pandemic recession, according to the BLS CES (payroll survey), over 14.3 percent of U.S. jobs were lost from February to April 2020. Following that, in the period 2021-2024, monthly net job creation averaged 328,000, up from 182,000 in 2016-2019.

migration as a crucial adjustment mechanism.[2] The instruments we use provide two different local average treatment effects (LATE) of the surge impact: the impact of surge migration caused by buslifts to a metro, and thus likely the effect of Venezuelan and Colombian migrants who lacked U.S. networks; and the impact of surge migrants more generally whose destination choices were driven by ethnic enclaves. Because surge migrants are poorly captured in the American Community Survey (ACS), we measure them using administrative data from the U.S. Department of Justice's Executive Office for Immigration Review (EOIR).[3] Outcomes for U.S. natives are based on the ACS, while measures of net domestic in-migration are based on Census Bureau data, and we use a variety of datasets for aggregate outcomes.[4]

Based on results using the shift-share instrument, we find that each surge migrant increased employment by 0.4 workers over 2021-2024, with the greatest increases in leisure/hospitality and professional and business services. The additional workers and consumers increased GDP by 1.4% for each one percentage point increase in the share of surge migrants, implying an increase in GDP per capita. Our estimates indicate that the surge may have reduced average hourly wages. However, the estimated weakly significant wage decline of 1.4% in response to a one percentage point increase in the surge migrant share is too large to be accounted for by just composition effects. That said, Wilson and Zhou (2026) find a similar responsiveness of wages and employment to surge migration. Because the average metro experienced a 0.9 percentage point increase in the surge migrant share, we estimate the surge increased the average metro's GDP by 1.5% and may have reduced its average hourly wage by 1.5%.

The part of the average wage decline not explained by composition cannot be explained by a decline in U.S. natives' wages. On the contrary, natives' wages rose by 0.9% over 2021-2024 in response to the surge. Offsetting this, metropolitan area rents measured either for all renters or U.S. natives rose 1.4-1.6%. We calculate, however, that the average native's wage net of rent rose at least 0.6%. Meanwhile, native employment rates did not change as a result of the surge. The responsiveness of rents is similar to that estimated by Wilson and Zhou (2026),

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[2] There is also a nascent literature on the Trump administration's increase in immigration enforcement in the wake of the surge, including Cox and East (2026), Escobari et al. (2026), and Hernandez (2026).

[3] EOIR is the agency responsible for administering and overseeing the U.S. immigration court system. Other studies that use the EOIR data include Galli and Padilla (2026), Clemens et al. (2026), Scarborough et al. (2026), and Wilson and Zhou (2026).

[4] U.S. natives are defined as individuals who are either born in the United States or abroad to U.S. citizen parents.

although our aggregate rent effect is smaller, while the responsiveness of native wages, but not employment, is similar to estimates by Clemens et al. (2026) for 2021-2024.[5] The moderate impact of the surge migration reflects the crucial adjustment role of domestic migration. In the average metro, 90% of surge migration was offset by reduced net domestic in-migration.

In addition to examining effects for the overall period, we also look at shorter-run dynamics, namely year-to-year changes. Although there is no clear temporal pattern in the surge effects on most of the aggregate variables, the positive effects on U.S. natives' wages and rents were highest early in the surge. After 2022 (for wages) or 2023 (for rents), the short-run effects fell to zero (based on the shift-share instrument) or even reversed (using the buslift instrument), thus dragging down the magnitude of the longer-run effects. The declining responsiveness of native wages and rents over time was only partly offset by the increasing size of the surge. The results do not mean that there were no impacts of the late surge, only that the impacts were spread over a wider geography and cannot be detected by comparing metropolitan areas.

The early increase in wages of U.S. natives may have been the result of a stimulus to demand by surge migrants, particularly those who were initially barred from working legally. Based on examination of native wages and employment by education, we cannot rule out the hypothesis that the overall wage increase stemmed from complementarity of surge migrants and natives, though the point estimates are not entirely supportive. The late surge results based on the buslift instrument suggest that migrants whose location choice was based on the bus destination had different effects on natives than other surge migrants who were likely more influenced by their networks.

I. Documenting the Migration Surge

We begin with an overview of the migration surge and its similarities and differences from historical flows of unauthorized migrants. The surge was larger than previous migration waves, such as the sizeable inflows the United States received from Mexico in the 1980s and 1990s.[6] Much like those earlier waves, surge migration was due to a combination of the pull of a strong U.S. labor market, which attracted migrants as the economy rebounded from pandemic closures,

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[5] However, Clemens et al. primarily focus on 2021-2023, a period for which our results differ.

[6] Historically, large migration waves of people entering the US without inspection have occurred during periods of robust economic growth in the United States and/or economic crisis in Mexico (see, e.g., Hanson 2006; Hanson et al. 2017).

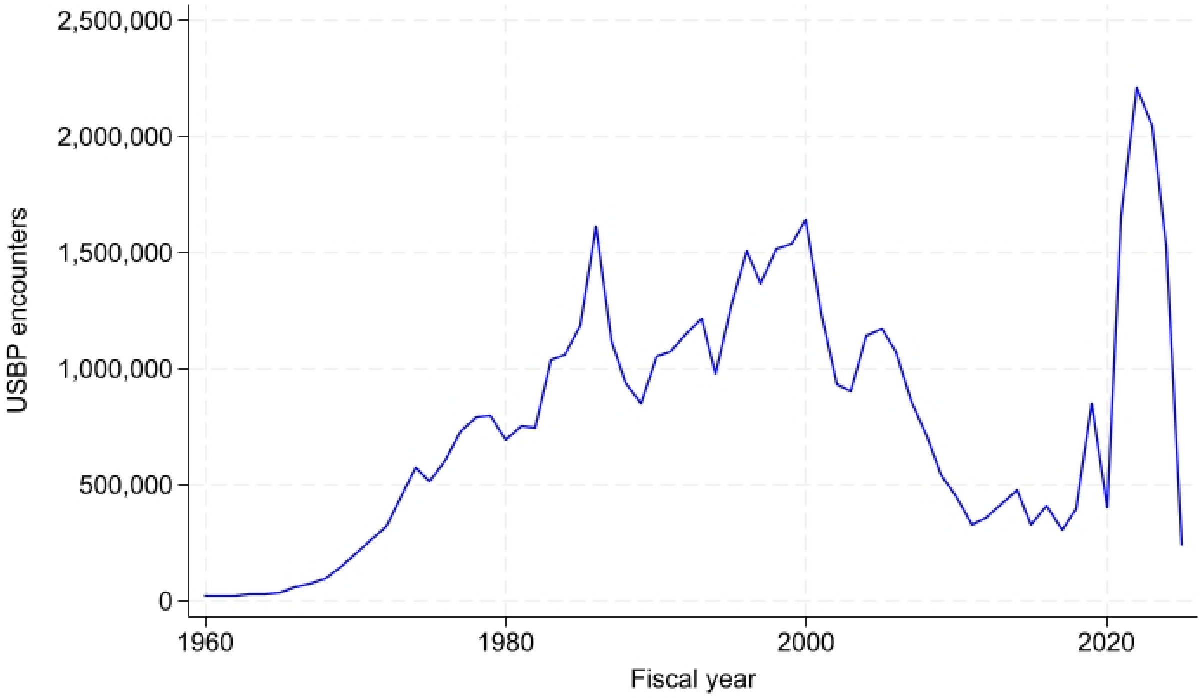
and push factors in origin countries, including weak economic conditions, political instability, violence, and natural disasters. However, the surge differed notably from historical episodes by encompassing a diverse set of origin countries, not just Mexico. And whereas previous waves were largely composed of working-age men, the surge included many family units and unaccompanied minors. It also differed in that permissive immigration policies resulted in many of the surge migrants entering under humanitarian programs or to seek asylum, which at least temporarily shielded them from removal and made them eligible for a work permit, whereas in the past unauthorized migrants typically lacked such protections.[7]

Figure 1 puts the size of the surge into historical context by showing the number of migrants encountered by U.S. Border Patrol (USBP) after illicitly crossing the U.S.-Mexico land border by fiscal year during 1960-2025. During the post-pandemic period, the number of those encounters exceeded even the massive flows the United States experienced during Mexican economic crises in the mid-1980s and mid-1990s.[8] Encounters began to fall in 2024, and in 2025 reached their lowest level in more than four decades. Many of the surge migrants encountered by USBP after illicitly crossing the border were released into the country, typically with a notice to appear (NTA) in immigration court. This was particularly the case for family units with children and migrants from countries that refused repatriation flights or that Mexico would not accept as expulsions. These releases occurred even during the Title 42 era that stretched from March 2020 until May 2023, during which USBP expelled many migrants to Mexico immediately and without an asylum screening.

[7] Asylum seekers who did not enter with parole were eligible to apply for an employment authorization document (EAD) once their asylum application had been pending for at least 150 days, and they could be approved for an EAD once their asylum application had been pending for at least 180 days. Surge migrants who were able to apply for a work permit sooner potentially faced delays since the processing agency, USCIS, was overwhelmed and often unable to process those requests quickly during the surge.

[8] The difference may be even greater than it appears since in earlier migration waves, migrants would typically make multiple attempts to cross the border and were often caught several times. Earlier numbers of encounters are thus inflated as compared with later ones. Since 2008, migrants caught crossing the border illegally after their first time are typically arrested, detained, and convicted, reducing recidivism (Bazzi et al. 2021). However, recidivism increased again under Title 42 since many migrants were rapidly expelled back into Mexico with no other consequences (Chishti et al. 2024).

Figure 1: U.S. Border Patrol encounters with unauthorized migrants along the U.S.-Mexico border, FY 1960-2025



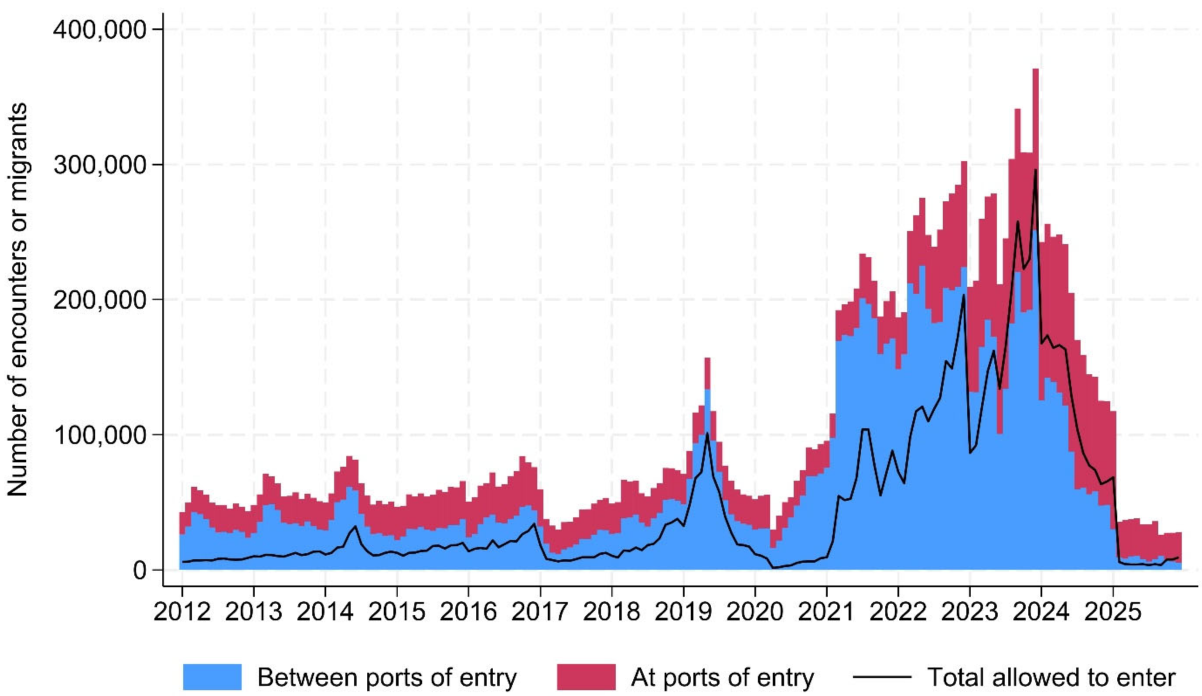
Source: U.S. Department of Homeland Security, Customs and Border Protection.

Migrants entered the United States not only by crossing the border illicitly (and, if in our data, encountering USBP) but also by presenting themselves at a port of entry and being granted entry despite being "inadmissible." During the surge, an unprecedented number of inadmissible migrants were allowed to enter at ports of entry via a humanitarian parole program or in order to pursue an asylum claim.[9] Indeed, the Biden administration encouraged unauthorized migrants to present themselves at ports of entry instead of crossing between ports during later stages of the surge. Figure 2 shows the total number of Customs and Border Protection (CBP) encounters with migrants who either crossed the border illicitly or presented themselves at a port of entry and were deemed inadmissible, as well as the number who were released or granted entry. The post-pandemic surge is again evident, with encounters reaching almost 400,000 per month in late 2023. About three-quarters of migrants encountered were allowed to enter during that peak period. The total number of encounters may reflect multiple interactions with the same people

[9] The data we use include few migrants from Afghanistan – fewer than 7,000 – despite reports that Operation Allies Welcome brought 76,000 Afghans to the United States (Montalvo and Batalova 2024).

and therefore give an overcount, but the number of migrants allowed to enter the country should be closely related to a count of unique individuals. In total, CBP data indicate that almost 5 million migrants were allowed to enter the country irregularly during 2021-2024. In addition, some migrants were able to evade USBP after crossing the border illicitly; CBP estimates there were over 1.9 million "gotaways" during fiscal years 2021-2024.[10]

Figure 2: Customs and Border Protection encounters with migrants, between and at ports of entry, and migrants allowed to enter, 2012-2025



Source: Authors' calculations using data from the U.S. Department of Homeland Security, Customs and Border Protection, and, for ports of entry during 2012-2013, from the Transactional Records Access Clearinghouse. Shown is the monthly sum of CBP encounters between ports of entry with migrants who crossed illicitly and at ports of entry with migrants deemed inadmissible, and the total number of those migrants who were released or granted entry into the United States.

The surge was notable not just for its size but also for the diversity of migrants' origin countries. During 2014-2019, Mexico and the Northern Triangle countries of El Salvador, Guatemala, and Honduras together accounted for almost three-quarters of migrants without a

[10] See https://www.cbp.gov/sites/default/files/2024-10/usbp_gotaway_and_turnbacks_fy19-fy24.xlsx. Edelberg et al. (2026) estimate that the surge totaled about 6.9 million over 2022-2024, including gotaways.

valid visa encountered by CBP between or at ports of entry (online appendix figure A1).[11] During 2021-2024, the share of migrants from Mexico and the Northern Triangle was below one-half, and the share of migrants from other countries in Latin America and the Caribbean had increased considerably, most notably for Venezuela, Colombia, and Haiti (online appendix figure A1).

I.A. Where Did the Surge Migrants Go?

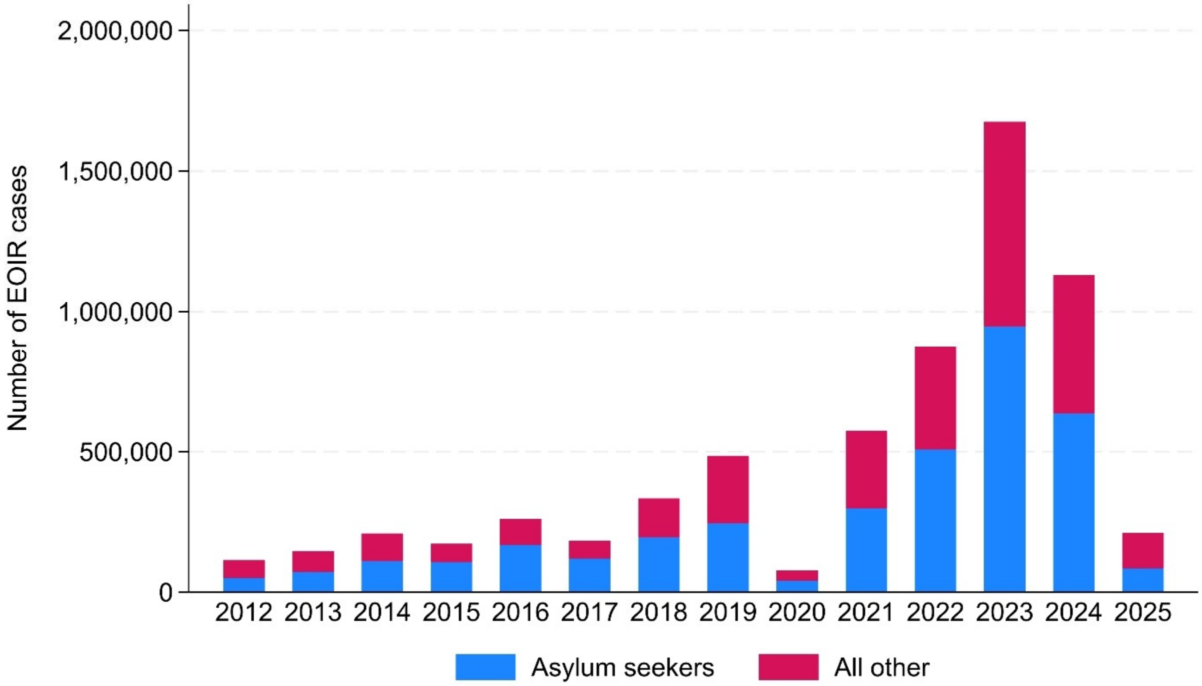
The CBP data give an idea of the size of the surge and migrants' origin countries, but those data do not indicate where migrants went within the United States. For that, we first turn to administrative data from the U.S. Department of Justice's Executive Office for Immigration Review (EOIR). Migrants who were given an NTA and then released or granted entry are included in the EOIR data, as are migrants who are in removal proceedings, some of whom have filed for asylum. Figure 3 shows the number of cases in the EOIR data for migrants by their arrival year over 2012-2025.[12] The data are further disaggregated by whether the migrant has filed an asylum application, according to the EOIR case record.[13] The surge is again evident in the data, with almost 2 million EOIR cases for migrants who arrived in 2023, and over 1 million in 2024. For 2021-2024 as a whole, the EOIR data include over 4.2 million cases.

[11] Data on individual encounters at ports of entry that include migrants' characteristics and disposition are only publicly available beginning in 2014.

[12] We use the date of entry recorded in the EOIR file. When the date of entry is not recorded, we use the date of the NTA. Surge migrants typically received an NTA when they were released by U.S. Border Patrol or were granted entry into the country after presenting themselves at a port of entry without a visa. We only include non-detained cases since the EOIR data report the place of detention, not the pre-detention place of residence, for detained migrants. About 94 percent of EOIR cases during 2021-2024 involve non-detained migrants.

[13] The EOIR data do not include successful affirmative asylum claims submitted to U.S. Citizenship and Immigration Services (USCIS). Affirmative claims that are turned down by USCIS with an applicant who lacks lawful status and defensive asylum claims by migrants in removal proceedings are included in the EOIR data.

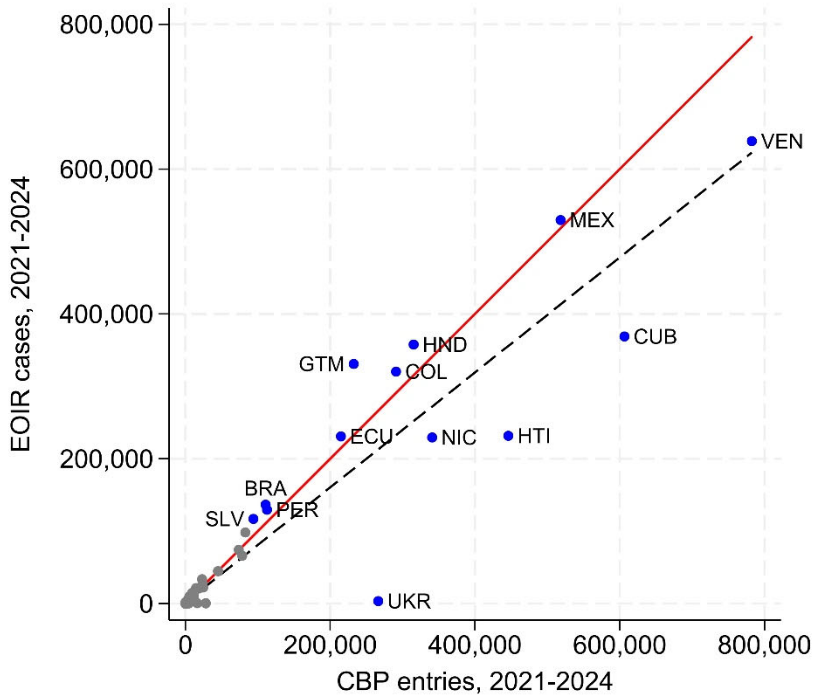
Figure 3: Number of Executive Office for Immigration Review cases, 2012-2025



Source: Authors' calculations using data from the U.S. Department of Justice, Executive Office for Immigration Review. Only non-detained cases are included. Migrants are categorized by their year of entry or, when that is not recorded, year of their notice to appear.

EOIR cases are not a count of unique surge migrants, since an EOIR case can include more than one person (like a family unit) and conversely we are unable to identify when individuals or families have more than one EOIR case. Figure 4's plot of 2021-2024 EOIR cases against CBP entries for each origin country shows that most countries lie along the 45-degree line. However, some countries are well below the 45-degree line, most notably Ukraine, whose nationals have a large number of CBP entries but are almost entirely absent from the EOIR data. The (dashed) regression line has a slope of 0.8, the factor by which the EOIR undercounts entries. This should be kept in mind for the descriptive statistics, and below we discuss the issue further in the context of interpretation of regression results.

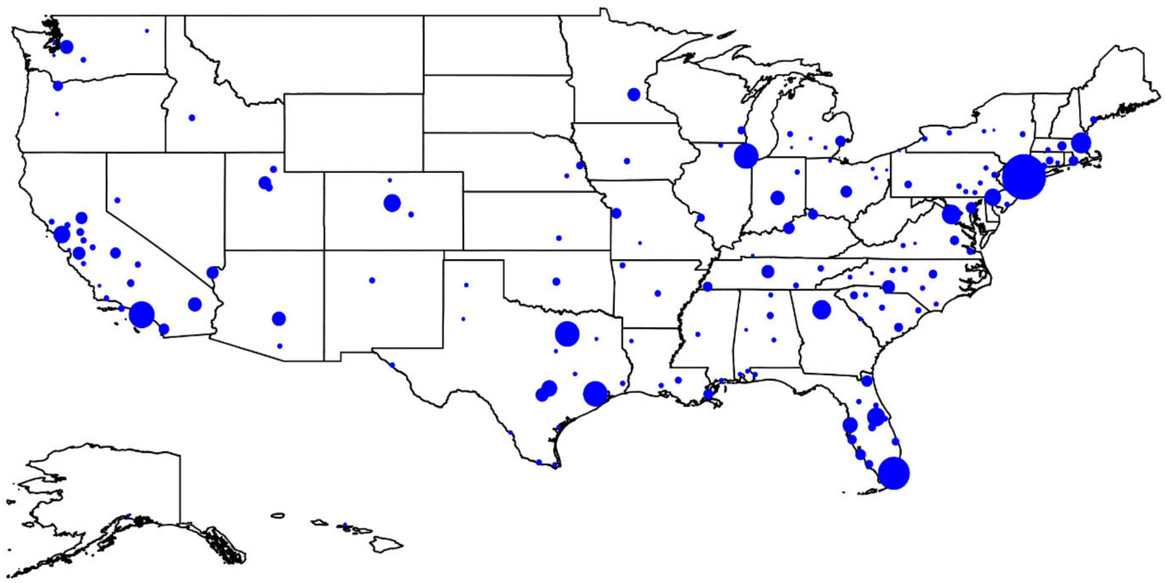
Figure 4: CBP entries and EOIR cases, 2021-2024, by country



Note: Authors' calculations using data from the U.S. Department of Homeland Security, Customs and Border Protection, and the U.S. Department of Justice, Executive Office for Immigration Review. EOIR cases include non-detained migrants who entered in 2021-2024 or, if an entry date is not recorded, received a notice to appear in 2021-2024. The red line is the 45° line. The dashed black line is the best linear fit. Its slope is 0.79 (0.02). The 13 labeled countries have the most CBP entries during 2021-2024.

Most of the surge migrants entered the United States along the U.S.-Mexico border, but they dispersed across the country. Figure 5 shows the relative distribution across major metro areas of surge migrants who arrived in 2021-2024, with larger bubbles corresponding to larger numbers of migrants. These 164 metro areas, which are the major metro areas that are identifiable in the ACS, accounted for 88 percent of surge migrants. The New York City metro area tops the list of surge migrant destinations, with almost 575,000 EOIR cases during 2021-2024. Miami is second, followed by Los Angeles, Dallas-Fort Worth, Chicago, and Houston.

Figure 5: Distribution of surge migrants as measured by EOIR cases across major metro areas



Source: Authors' calculations using data from the U.S. Department of Justice, Executive Office for Immigration Review on non-detained cases that entered or received a notice to appear during 2021-2024. The top 164 metro areas by total population over 2021-2024 that are identified in the American Community Survey are indicated.

The top destinations of surge migrants are among the traditional destinations of new immigrants. Miami being second on the list reflects the large number of surge migrants from countries that are geographically proximate to south Florida or have large diaspora communities there, most notably Cuba and Haiti. Among the major metro areas, Miami received the most surge migrants relative to its population size (online appendix figure A2).

Although many of the surge migrants went to traditional migrant destinations, there was an unusual twist during the surge: the "buslift."[14] The governors of Texas and, to a lesser extent, Arizona organized high-profile busing of migrants from border towns far into the U.S. interior. The bus trips were free and voluntary for migrants and may have been particularly attractive to migrants who did not have strong networks in the country, such as Venezuelans and Colombians.

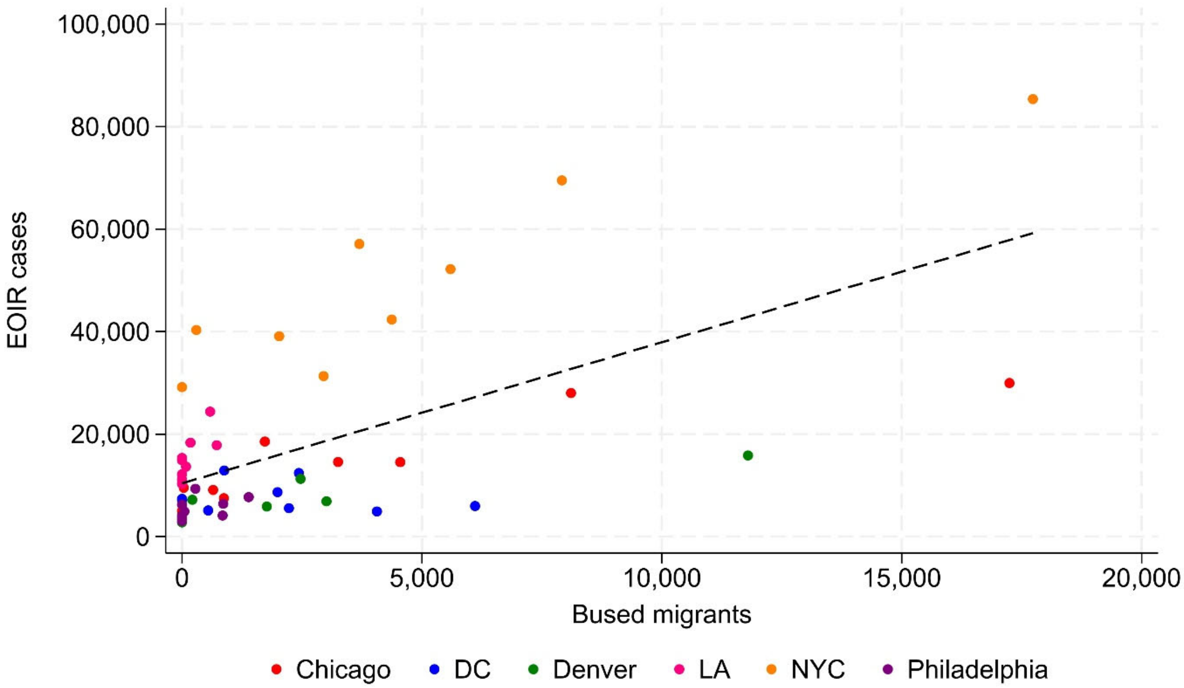
[14] Ozdogan and Shih (2025) coined the term "buslift" in the vein of the Mariel boatlift. They examine the effects on educational outcomes in New York City. Other research on surge migrants includes Meyer et al. (2025), who examine the impact on homelessness in NYC, Boston, Chicago, and Denver; all except Boston were buslift destinations. Scarborough et al. (2026) likewise examine the impact on homelessness, focusing on the impact of the Texas buses. See also Chishti and Gelatt (2022) and Goodman et al. (2024) for background on the buslift and the ensuing political controversy.

Many of the migrants who took the buses in Texas are believed to be from Venezuela (Goodman et al. 2024), while many of the early bus riders from Arizona were from Colombia (Chishti and Gelatt 2022). The combination of free transportation and lack of networks likely influenced migrants' decision to accept a bus ride. In total, almost 125,000 migrants were bused from Texas or Arizona to one of six cities: Chicago, Denver, Los Angeles, New York City, Philadelphia, and Washington, DC. As a result, those metro areas arguably received more surge migrants than they would have otherwise.[15] In particular, Denver and Philadelphia are not traditional destinations for new migrants. The choice of destinations was politically motivated – they were large sanctuary cities with Democratic mayors. As discussed more below, we use the buslift as an exogenous source of variation in surge migrants' destinations in addition to a traditional shift-share instrument when evaluating impacts on labor and housing markets.

The buslift began in April 2022 and ended in June 2024. Figure 6 shows the relationship between the numbers of bused migrants and surge migrants by quarter and metro area during the buslift period. There is a strong positive relationship between the quarterly number of bused migrants and the number of surge migrants, as proxied by EOIR cases for newly arrived migrants, across the six metro areas during that period. Although presumably not all of the bused migrants appear in the EOIR data, the number of bused migrants is equivalent to about 14 percent of all EOIR cases in those six metro areas during that period, and almost 60 percent of EOIR cases involving Venezuelans and Colombians (see online appendix figures A3 and A4). In short, the buslift plausibly caused a sizable increase in the number of surge migrants in the destination cities.

[15] In addition to the buslift organized by the Texas and Arizona governors' offices, some border cities and non-profit organizations organized buses using their own funds or FEMA funds. For example, as of mid-September 2022, the city of El Paso had sent over 80 buses of surge migrants to New York City and Chicago (Chishti and Gelatt 2022). We do not include those buses because of data limitations. Additionally, the governor of Florida used state funds to pay to fly from Texas 48 migrants to Martha's Vineyard, Massachusetts, and 36 migrants to Sacramento, California. We do not include those flights since they involve a very small number of migrants.

Figure 6: Bused migrants and EOIR cases, 2022Q2-2024Q2



Source: Authors' calculations using data from the U.S. Department of Justice, Executive Office for Immigration Review (EOIR), and the Texas and Arizona governors' offices. Only non-detained EOIR cases are included. Each point is the number of bused migrants and EOIR cases for a given metro area and quarter over 2022Q2 – 2024Q2. The linear regression line has a slope of 2.766 (0.880) and an R-squared of 0.377.

More broadly, surge migrants were a large share of population growth in many metro areas in the post-pandemic era. The Census Bureau estimates that net international migration totaled about 6.67 million between July 2021 and June 2024, and U.S. population growth, which includes the difference between births and deaths in addition to net international migration, was 7.9 million.[16] The total number of EOIR cases involving migrants who arrived during that period – our main measure of surge migrants – is 3.68 million. For the 164 major metro areas we examine, as a whole, the number of EOIR cases is equivalent to about 55 percent of the Census Bureau's estimate of net international migration to those areas between July 2021 and June 2024 (online appendix figure A5), and about one-half of the total change in their population; meanwhile, net domestic migration was negative for those major metro areas as a whole during

[16] Based on the 2025 vintage estimates available at https://www2.census.gov/programs-surveys/popest/datasets/2020-2025/state/totals/NST-EST2025-ALLDATA.csv.

that period, an issue we return to below.[17] Given the sizable share of population growth it accounted for, the surge would be expected to have effects at both the macro- and microeconomic levels, impacting labor and housing markets overall as well as U.S. natives' outcomes. As noted above, many surge migrants were eligible for a work permit, either immediately or, for most asylum seekers, after about six months, and, as legal workers, they would be expected to show up in traditional measures of economic activity.[18]

I.B. How Well Do Census Surveys Capture Surge Migrants?

An important question for researchers who rely on data from household-level Census surveys to examine questions related to immigration is how well those surveys captured the surge in the near term. We focus here on the American Community Survey (ACS) since it has the largest sample – 1% of the U.S. population – of public-use datasets.[19] The ACS has a sample of roughly 3.5 million households, and the sample is developed using the Census Bureau's Master Address File, its inventory of every known residential address. Weights are assigned to make the ACS data consistent with pre-determined fixed population controls from the Census Bureau Population Estimates Program (PEP) for that year by age, sex, race, and Hispanic origin at the county level.[20] The Census Bureau retrospectively revised its estimates of net international migration upwards for 2021-2022 and 2022-2023 by over 1.8 million migrants to better reflect the surge, which increased the PEP estimates for those periods (Gross et al. 2024). However, ACS weights were not retrospectively revised. As a result, the 2022 and 2023 ACS weights are too low for recently arrived migrants. The Census Bureau adjusted its estimate of net international migration for 2023-2024 upwards by about 1.5 million to reflect the shortfall

[17] Based on the 2025 vintage estimates available at https://www2.census.gov/programs-surveys/popest/datasets/2020-2025/metro/totals/cbsa-est2025-alldata.csv.

[18] One measure of how many surge migrants worked legally is the number of applications for a work permit by asylum applicants and parolees. Online appendix figures A6, A7, and A8 show those data.

[19] For discussion of how well the Current Population Survey (CPS) captured the surge, see, for example, Edelberg et al. (2026).

[20] It is well-known that the ACS and similar surveys underreport unauthorized immigrants even after taking the weights into consideration. That underreporting ("coverage error") is typically believed to range from about 5 to 25 percent but can reach as high as 40 percent for some groups (e.g., Van Hook et al. 2021). The weighting procedure does not incorporate immigrant status or characteristics typically associated with unauthorized immigrants, other than Hispanic ethnicity.

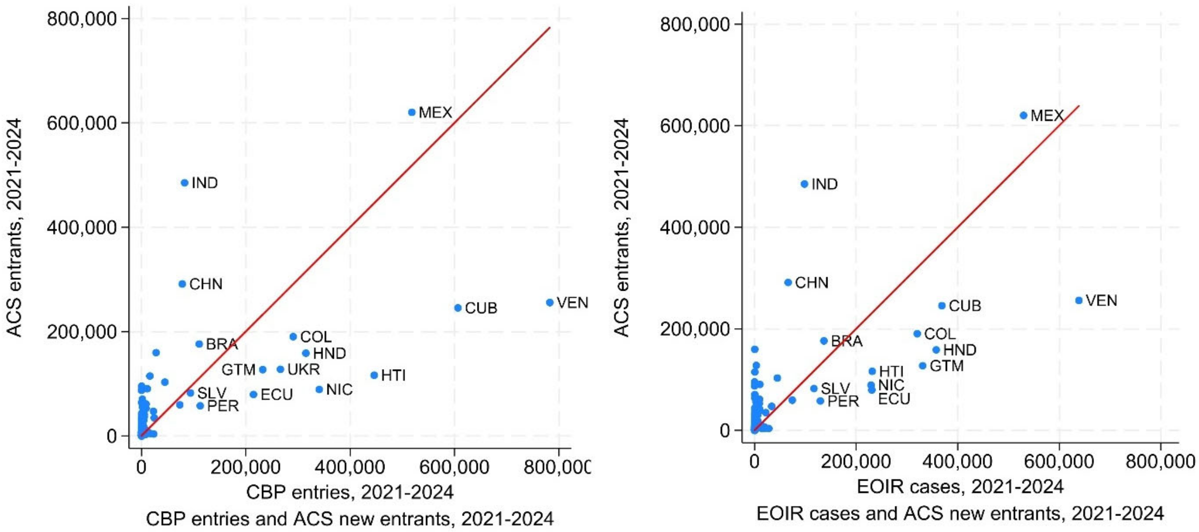
between the counts implied by the 2023 ACS and administrative data (Gross et al. 2024), and the 2024 ACS weights should reflect that adjustment, at least at the national level.[21]

Figure 7 compares the total number of non-citizens who reported that their residence one year ago was abroad in the 2021-2024 ACS with the number that CBP allowed to enter and the number of EOIR cases for 2021-2024 migrants, by country.[22] Figure 7 highlights the 12 Latin American and Caribbean countries that composed most of the surge (see online appendix figure A1) and a few other notable countries. Since the total number of non-citizens in the ACS who were abroad a year ago should, in theory, include immigrants who were formally admitted as well as migrants who were allowed to enter despite not being formally admitted (surge migrants), the ACS totals should exceed the CBP and EOIR totals if the ACS accurately measures all migrants. This is clearly not the case for most of the surge countries – most of them are well below the 45° line in both figures, indicating they were undercounted in the ACS. The undercount is particularly notable for Venezuelans, who were the largest source country of surge migrants. However, Mexico, another major surge country, is above the 45° line. Mexicans who entered irregularly as part of the surge may be more likely to be counted in the ACS since they have deeper networks in the United States, but there are also substantial numbers of Mexicans who enter the United States legally with a temporary or permanent resident visa. The two other countries that are well above the 45° line – India and China – receive large shares of temporary and permanent resident visas but account for small shares of surge migrants.

[21] The Census Bureau used EOIR data to distribute humanitarian migrants to specific states and counties in its vintage 2025 estimates, including retrospective changes for the sub-national 2022-2024 data (U.S. Census Bureau 2026). Although the Census PEP estimates were revised, the ACS weights were not. The weights in the 2024 (and earlier) ACS reflect historical geographic distributions of international migrants. If surge migrants went to different places than earlier migrants, then the 2024 ACS weights may be correct at the national level but not at the sub-national level.

[22] Since the 2024 ACS weights should better incorporate the surge, online appendix figure A9 shows a similar comparison using the 2024 ACS with non-citizens who reported entering the country in 2021 or later. Online appendix figure A10 compares the number of non-citizens who were abroad a year ago in the 2021-2024 ACS and the number of non-citizens in the 2024 ACS who reported entering the country in 2021 or later. Online appendix figure A11 compares the number of non-citizens who were abroad a year ago in the 2021-2024 ACS with the same in the 2021-2024 Current Population Survey (CPS) Annual Social and Economic Supplements. Like the ACS, the CPS uses pre-determined fixed population controls to weight observations, and those controls do not incorporate immigrant status.

Figure 7: Comparison of number of surge migrants in CBP and EOIR data with number of new entrants in ACS, by country, 2021-2024



Source: Authors' calculations using data from U.S. Department of Homeland Security, Customs and Border Protection (CBP) on the number of irregular migrants who were released or granted entry; data from the U.S. Department of Justice, Executive Office for Immigration Review (EOIR) on the number of non-detained cases involving migrants who entered the country during 2021-2024; and data from the 2021-2024 American Community Survey (ACS) on non-U.S. citizens whose residence one year ago was abroad. The person weights are used to create the ACS counts of entrants. The red line is the 45° line.

The ACS thus appears to capture surge migrants only partially. There are several reasons beyond the weights why the ACS appears to miss many surge migrants. Many of the surge migrants were initially homeless; lived in shelters, including non-traditional shelters, such as hotels, sports arenas, and huge tents, in some cities; or stayed with friends and family (Meyer et al. 2025; Scarborough et al. 2026). The ACS does not include the homeless, and Census surveys undercount people in non-traditional or crowded housing (Kissam 2017). Among surge migrants with traditional housing, uncertainty about their legal status may have made them reluctant to complete a government survey, particularly soon after arrival, and friends and family who hosted them may have been reluctant to participate for similar reasons. Frequent moves across housing units may also make some surge migrants hard to capture.[23] Lack of English or Spanish literacy – the two languages in which the ACS is primarily conducted – may also hamper responses.

[23] The ACS instructions say to include anyone living or staying at the address for more than two months, or anyone staying at the address who has no other permanent place to stay even if they have been there for less than two months, but respondents may not have complied. Research indicates that respondents' social construct of a household trumps the instructions (Kissam 2017).

The ACS undercount of surge migrants has several implications. Population growth and demographics are mismeasured, affecting economic and fiscal projections. Discrepancies across data sources that arise as a result of mismeasurement erode trust in government data and inject uncertainty into organizational decision-making at the federal, state, and local levels. Moreover, the Census Bureau's population estimates are used to help determine the allocation of federal funds to states and localities as well as to various nationwide programs. Since ACS data inform the Census Bureau's population estimates, an undercount of surge migrants has the potential to reduce funding to areas that disproportionately received surge migrants.[24] The population estimates and data from surveys like the ACS are also widely used by researchers around the world.

II. Theoretical Framework

Although there are some similarities between surge migrants and unauthorized migrants in earlier periods who crossed the southern border with no intent to claim asylum, the labor market impact of the surge migrants may differ from previous flows of unauthorized migrants for several reasons. First, the "great reshuffling" of workers across industries and occupations during the pandemic combined with the re-opening of businesses whose operations were curtailed during the pandemic resulted in tremendous demand for workers in sectors that required little formal education or training, such as leisure and hospitality—primarily restaurant and hotel jobs. Second, many of the surge migrants received legal permission to work, as noted above. Third, the education profile of surge migrants differed from traditional unauthorized immigrants, who tend to have relatively low levels of formal education; as many as one-half of surge migrants have at least some college education, according to the CBO (2024). The surge migrants may more closely resemble visa overstayers along some dimensions.

II.A. Aggregate Local Economy Effects

At the aggregate level, we expect surge migrants to boost employment and output, and hence GDP, while their effect on the average wage is ambiguous. The magnitude of the impact

[24] Further, ACS data inform Census Bureau population estimates with a lag since a given year's ACS is still in the field while the Census Bureau produces its population estimates for that year.

on total employment is more interesting than the direction. Estimates from Clemens et al. (2026) are consistent with positive spillovers of asylum seekers onto other workers, or a greater than one-for-one effect, whereas Wilson and Zhou (2026) estimate a roughly one-for-one increase in employment with working-age surge migrants. The impact on the average wage is ambiguous since surge migrants had a compositional effect on the labor force in addition to increasing both labor supply and labor demand, the latter through their impact on aggregate demand. The increase in labor supply should put downward pressure on wages, an effect that is offset at least somewhat by the increase in labor demand due to complementarities with native-born workers, a boost to demand from higher consumer and business spending, and possibly increased productivity due to increased specialization and, over longer periods, more capital and higher innovation. If surge migrants earned below-average wages, as might be expected given their lack of familiarity with U.S. labor markets, relatively low educational attainment, and limited English fluency, their presence might mechanically reduce median wages. This effect is likely to ameliorate over time as migrants assimilate.

II.B. U.S. Natives' Labor Market Outcomes

Effects on U.S. natives' employment and wages depend on the aggregate demand effect, as well as the degree of substitutability versus complementarity between surge migrants and U.S.-born workers. Demand effects should have positive wage and employment effects on natives. By education levels, substitutability is more likely for less-educated U.S. natives, especially in the short run. Delayed or no work authorization is likely to have forced many surge migrants to initially take low-skilled jobs regardless of their education level. In addition, it will take many surge migrants time to acquire English language fluency and professional networks. This suggests that surge migration would have had negative effects on employment and wages among U.S. natives who have relatively low levels of education. Over time, negative effects may have ameliorated as surge migrants and U.S. natives moved or switched jobs and as consumption and any productivity effects grew. Meanwhile, more educated U.S. natives may have experienced positive wage and employment effects due to complementarities with surge migrants. The surge coincided with major shifts in the wake of the pandemic, including initially the rise in work from home and eventually children returning to in-person school. An influx of workers willing to produce household services, from childcare to gardening, may have boosted labor supply in higher-income households. Wage effects among highly educated U.S. natives therefore may reflect compositional shifts in the U.S.-born workforce that are due in part to the surge.

III.C. Housing Markets

In the very short run, the surge may have had little effect on rental markets if surge migrants were living in shelters or with friends or relatives, but after some time they likely sought rental housing. The U.S. housing stock is inelastic, particularly in urban areas, so housing costs are likely to rise as a result of surge migration. Studies using pre-surge data typically find that immigration raises rents and housing prices, as any population growth would (Saiz 2007, Howard et al. 2025, Cabral and Steingress 2026). A rise in rents could also push some U.S. natives and earlier immigrants into lower-quality, more-crowded housing or even homelessness (Greulich et al. 2024).

III. Data and Methods

We use data at the local level from a variety of sources to examine the effects of surge migrants. Since endogenous location choice is a major concern when evaluating the impact of immigration at the local level, we focus on instrumental variable regressions when estimating impacts. We also look at several different time periods, including differences for the entire period of surge migration (2021-2024) and short-run year-over-year changes for 2021-2022, 2022-2023, and 2023-2024.

III.A. Data and Samples

We perform our analysis at the metropolitan area level (using core-based statistical areas, or CBSAs) because we believe it best captures the geographic dimensions of local labor and housing markets. It also has the advantage of allowing us to use variables computed by the Census Bureau and other federal agencies at that level. For most of our analysis, we use the 164 largest metros that are identifiable in the ACS throughout 2021-2024. Those metros comprise more than 85% of the U.S. population. We examine the impact of surge migration on outcomes based on aggregate measures that include the entire labor or housing market at the metro level, as well as outcomes based on measures constructed from the ACS that include only U.S. natives. Before detailing those measures, we briefly describe the variables we use to measure surge migration.

Surge migration: Our primary measures of surge migration are the number of EOIR cases involving non-detained migrants, as described above, and this number divided by the metro area population.[25] We include migrants of all ages in order to better capture aggregate demand effects. The number of surge migrants increased over the four-year period we examine, as shown in Table 1 panel A. To create our instruments, we use CBP entries, as described above, which is a better national-level measure of surge migration than EOIR cases, together with ACS data, and the number of buslift migrants along with an indicator for buslift metros. For the surge variable and its instruments, which are monthly, years are defined from July of the previous year through June of a given ACS year, and we use the population in July of the prior year when dividing by population. Since the aggregate measures of employment, average wage, GDP, and average rent are on a calendar year basis, our approach implicitly builds in a six month lag for surge migration to affect those variables.[26]

Aggregate measures: We examine metro labor market outcomes using employment counts and average hourly earnings from the Current Employment Statistics (CES) data produced by the Bureau of Labor Statistics (BLS). These data, commonly known as the payroll survey data, are based on monthly surveys of non-farm employers and are benchmarked annually to the BLS's Quarterly Census of Employment and Wages (QCEW). The CES survey covers all non-farm wage and salary jobs, so the data include surge migrants who have work authorization as well as any unauthorized immigrants working on the books for an employer. The data do not capture self-employed workers or gig economy workers who are not formal employees. We also examine effects on output (Gross Domestic Product), using Bureau of Economic Analysis data.[27] The GDP data only capture formal-sector activity, but they should

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[25] The EOIR data include migrants' zip code, which we map into metro areas using a crosswalk from the U.S. Department of Housing and Urban Development. We examine gross, not net, surge migration. We do not believe it is possible to accurately measure net surge migration, and it is unlikely a large number of surge migrants left before mid-2024. Clemens et al. (2026) and Wilson and Zhou (2026) examine net migration.

[26] Another reason for this approach is that we do not know when in the calendar year a respondent answered the ACS question about previous twelve month's wage and salary income, which we use to construct hourly wages.

[27] The GDP data are published at the county level, which we aggregate into metro areas. We can construct GDP data for only 160 of the 164 major metro areas we examine in our labor market analysis. We use inflation-adjusted GDP data expressed in 2024 dollars.

include the value of output produced by gig-economy, contract workers, or off-the-books employees at formal-sector establishments. Finally, we examine average rents at the metro level using data on multifamily rents produced by Zillow.[28] U.S. natives cannot be distinguished from immigrants in these data, and the effects of surge migration we estimate with these outcomes will include any compositional effect of migrant outcomes on mean values. Summary statistics are reported in panel B of Table 1.

ACS-constructed and domestic migration measures: In order to examine the impact of the surge on U.S. natives' outcomes, we primarily use variables we construct from the ACS: the employment rate among U.S. natives ages 16-64 (as of the recent reference week), average hourly wages among the subset who are working and not self-employed (based on the 12 months prior to the interview), and average gross rent including utilities for all U.S. natives (so as not to give equal weight to small and large households).[29] We also use published net domestic in-migration data from the Census Bureau (vintage 2025), which measures net inflows from July in the prior year to June of the current year. Where the sample size permits, we consider subsamples of U.S. natives by education groups.

Panel C of Table 1 reports sample means for those variables. The 2.4 percentage point increase in the native employment rate and the \$2.40 decline in the real average native wage between 2021 and 2022 reflect the recovery from the pandemic recession and accompanying inflation and compositional changes in the U.S.-born labor force (Howard et al. 2022). The decline appears later than in Current Population Survey data because the ACS wage is calculated from income over the 12 months preceding a survey. In the regressions using ACS wage data, we therefore adjust wages and other outcomes (except domestic migration) for gender, age, and education.[30] However, the means of the adjusted outcomes, reported in online appendix Table A1, jump almost as much in 2021-2022 as the unadjusted outcomes, showing that the adjustment is not very effective.

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[28] The Zillow rent data are available for only 92 of the 164 major metro areas we examine in our labor market analysis. The underlying unit is a residence.

[29] As with the aggregate variables, wages are deflated using the CPI-U. Rents are deflated using the CPI-ex-shelter.

[30] We estimate one regression per outcome pooled for 2021-2024 and weighted by ACS sample weights, and control for gender, gender × age, 3 education dummies, 4 age dummies, and education dummies × age. We then use the average residuals by metro and year as the dependent variable.

Table 1: Summary statistics for analysis of impact of surge migrants

	2021	2022	2023	2024
A. Surge migration variables:				
EOIR cases	1176	4240	5754	9817
	(2948)	(11544)	(15839)	(24357)
EOIR cases/population (%)	0.06	0.19	0.25	0.47
	(0.05)	(0.19)	(0.19)	(0.30)
Shift-share using CBP entries	988	4375	8055	12357
	(3142)	(18107)	(33727)	(46217)
Shift-share using CBP entries/population (%)	0.04	0.16	0.29	0.47
	(0.04)	(0.28)	(0.50)	(0.66)
Buslift city indicator variable	--	0.2	0.2	0.2
Buslift migrants	--	135	839	3143
		(742)	(2569)	(9193)
Buslift migrants/population (% × 100)	--	0.22	0.97	4.18
		(0.12)	(3.29)	(13.19)
B. Aggregate variables:				
Employment (in thousands)	680	711	726	735
	(1085)	(1143)	(1165)	(1179)
Hourly wage ($2024)	29.56	26.50	25.36	24.74
	(4.55)	(3.97)	(3.75)	(3.61)
GDP (in millions $2024)	122.2	116.0	114.3	114.1
	(235.3)	(222.4)	(217.2)	(216.4)
Multifamily rent ($2024)	1574	1616	1621	1624
	(457)	(475)	(475)	(476)
C. ACS-constructed and domestic migration variables:				
Hourly wage for U.S. natives ($2024)	33.4	31.0	32.3	32.7
	(6.0)	(6.2)	(5.8)	(6.1)
Employment rate for U.S. natives (%)	69.6	72.0	72.4	72.4
	(4.4)	(4.2)	(4.0)	(4.0)
Net domestic in-migration in the last year/population (%)	0.4	0.4	0.2	0.2
	(1.3)	(1.2)	(1.0)	(0.8)
Gross rent for U.S. natives ($2024)	1168	1397	1544	1650
	(323)	(365)	(401)	(420)

Sources: EOIR; CBP; Arizona Department of Emergency and Military Affairs and Texas Division of Emergency Management (buslift migrants); Bureau of Labor Statistics Current Employment Statistics (employment, hourly wage); Bureau of Economic Analysis (GDP); Zillow (multifamily rent); ACS for U.S. native wages, employment, and rent; Census Population Vintage 2025 for population and net domestic in-migration; CPI-U-RS for deflating wages, CPI excluding shelter for deflating rents.

Notes: Unweighted sample means (and standard deviations) for 164 major metro areas, except for the three buslift variables, which are for the top 30 metro areas. Gross rents include utilities. The 2021 surge statistics are not included in the regressions. The buslift began in 2022.

III.B. Empirical Methods

To capture short- and longer-run effects of migration and in order to eliminate metro-specific fixed effects, we examine one- and three-year (2021-2024) differences in GDP, wages, employment, and rents. Surge migration and domestic migration are flows, so they are not expressed in differences but rather are simply totaled over either the one- or three-year period under examination. There are three specifications for our outcome variables. The first specification measures the effect of surge migration on overall employment. In this case, we estimate:

$$\Delta^k y_{mt} = \alpha + \varphi\, Surge_{mt}^k + \beta_1 Pop_{m,\,2019} + \beta_2(y_{m,\,2019} - y_{m,\,2016}) + \beta_3(y_{m,\,2021} - y_{m,\,2019}) + \sum_{\tau=1}^{J} \beta_{4,\,\tau}\, s_{m,\,\tau} + \Delta^k \varepsilon_{mt}$$

where the outcome *y* is total employment in metro area *m, t* indexes calendar years, *k* indicates the length of the difference, and $Surge^k$ is the cumulative number of surge migrants over the *k* years covered by the difference. Since employment is generally trending up and differs in scale across areas, we control for the pre-pandemic population in each metro area ($Pop_{m,2019}$) and the pre-pandemic change in total metro employment. To allow for post-recession reversion to the mean, we also control for the depth of the pandemic recession with the 2019-2021 change in metro employment.[31] Finally, we control for the shares $s_{m\tau}$ of metro employment in each major sector, indexed by τ, in the pooled years 2016-2019.

The second regression model, which we use to examine all other outcomes except domestic migration, is an equation similar to (1), but with surge migration scaled by population and without the controls for the 2019 population and the 2016-2019 employment shares. We estimate:

$$\Delta^k y_{mt} = \delta + \rho\left(\frac{Surge_{mt}^k}{Pop_{m,\,t-k}}\right) + \gamma_1(y_{m,\,2019} - y_{m,\,2016}) + \gamma_2(y_{m,\,2021} - y_{m,\,2019}) + \Delta^k \nu_{mt}$$

The coefficient of interest is ρ. Although we adjust natives' outcomes for individual characteristics, a control for mean-reversion is necessary, since these adjustments did not successfully capture composition changes (however, the control itself is unadjusted).

[31] We do not control for the more natural 2019-2020 change because many outcomes are poorly measured in the 2020 ACS.

In the case where *y* is itself a flow, as in the domestic migration regressions, y's are summed over the k years, rather than differenced:

$$\sum_{u=t-k+1}^{t} y_{mu} = \theta + \lambda\left(\frac{Surge_{mt}^{k}}{Pop_{m,t-k}}\right) + \mu_1 \sum_{u=2016}^{2019} y_{mu} + \mu_2 \sum_{u=2019}^{2021} y_{mu} + \omega_{mt}$$

Finally, for the outcomes where we are interested in differential effects by education, we calculate and analyze means by native education group.

For GDP and average wages and rent, we examine changes in logs. For natives' wages and rents, we examine changes in the metro averages of logs of (adjusted) individual outcomes. The net domestic in-migration rate and the native employment rate, however, are simply differences and not logged.

We estimate equations (1) and (2) for the longer-run difference over 2021-2024 as well as for the three short-run differences over adjacent years in that period, using OLS and 2SLS. The publicly available ACS micro-data reflect responses given throughout a calendar year, while for the surge variable and its instruments (and net domestic migration), we define years to be from July of the previous year through June of a given ACS year, as noted earlier.[32]

III.C. Instrumental Variables

Because of the possibility that surge migrants went to areas with better economic opportunities or lower rents, causing φ, ρ, and θ to be biased – upwards for wages, employment, and GDP, and downwards for rents – in the equations above, we use one of two instruments.

Our first instrument is a shift-share instrument following Card (2001), which is particularly suited to the surge migration event when the mix of origin countries changed suddenly, since the instruments are unlikely to reflect serial correlation (Jaeger, Ruist, and Stuhler 2018). The construction of the shift-share instrument requires distributing inflows of immigrants from various origin countries or groups of countries across the United States based on historical settlement patterns of earlier immigrants from that origin.[33] The local area treatment

[32] It may seem natural to align a certain ACS year with a calendar year of surge migration, but this would mean that a large share of the surge migrants arrived after the interviews of a large share of ACS respondents. To mitigate this problem, we lagged the surge migrants by six months. We were reluctant to lag by 12 months, as this would imply that the surge migrants of year t arrived two years before the last ACS interviews of year t.

[33] We calculate the shares for the top 12 surge countries and individual countries with more than 2% of immigrants, and otherwise calculate them for regions, yielding 25 origin countries or regions. See online appendix table A2 for the 25 groups and their number and share of CBP entries during 2021-2024.

effect (LATE) interpretation of the IV coefficient is the impact of surge migrants whose destination choice was influenced by ethnic networks.

We use the 2013-2017 ACS to measure earlier immigrants' distribution across areas. To calculate the shifts, we use CBP entries by origin. There are not enough origin countries with big shifts to precisely identify our coefficients under the assumption that the shifts are exogenous rather than the shares (Adão et al. 2019, Borusyak et al. 2022), so we rely instead on the assumption that the shares are exogenous (Goldsmith-Pinkham et al. 2020) and report Rotemberg weights. Cubans typically have the largest weight. We believe Cubans' location choices are exogenous to metro economic conditions since half of them go to Florida – and almost one-third to Miami alone – because of the Cuban diaspora located there. Our instrument does not use EOIR data and therefore is uncorrelated with its measurement error. A disadvantage is that we cannot generate a "leave-one-metro-out" instrument as others have done (Clemens et al. 2026) except by subtracting EOIR data from CBP data. Doing so would reduce one source of correlation with the error term while increasing another.

Our second instrument is the number of surge migrants bused to a metro area by Texas and Arizona, a politically motivated action as explained above. When estimating the first-stage model for equation (1), we include an indicator variable for the six metro areas that received buses in addition to the number of migrants who were bused from Texas or Arizona during the indicated time period.[34] Because so few cities received buses, for regressions involving this instrument we calculate wild bootstrapped standard errors using Roodman et al. (2019) with Webb weights in addition to robust standard errors. Because all six metros are large, it is likely that the best comparison metros are also large. We therefore restrict the sample for the buslift analysis to the 30 largest metros.[35] The LATE interpretation of the 2SLS coefficient is the impact of surge migrants whose destination was influenced by the buslift. The buslift migrants were predominantly Venezuelans and Colombians who lacked U.S. networks.

A drawback of measuring surge migration with the EOIR data, as mentioned above, is that one EOIR case does not map cleanly onto one surge migrant, making interpretation of the EOIR coefficient's magnitude difficult. We address this in three ways. First, we present reduced-

[34] The joint statistical significance of these two variables is much higher than that of the number of migrants alone based on the joint wild bootstrap test (p=0.03), with the more flexible specification explaining more variation. This is not the case in regressions in which the surge migrants are scaled by metro population.
[35] Denver, the smallest of the six buslift destinations, is the 20th most populous metro.

form results, which do not use EOIR data; the coefficient here represents the effect of expected surge migration, or the effect of exposure to surge migration. Second, we use the adjustment factor described above based on figure 4: the graph's slope of 0.8 indicates that each EOIR case represents 1.25 CBP entries. Thus, coefficients on EOIR covariates should be deflated by a factor of 0.8 to find the effect of true entries. Third, we test sensitivity to removing the origin countries farthest from the 45-degree line: Ukraine, Cuba, and Haiti.

The first-stage coefficients in our preferred specifications differ slightly depending on the outcome because we control for historical changes (or summed flows) in the outcome. Table 2 shows the first-stage results for total employment (top panel) and the native employment rate (bottom panel), and for different time periods in each column. The coefficients on all shift-share instruments are statistically significant at the 1% level with F-statistics of at least 17, with one exception (columns 1-5). As column 1 in the top panel shows for 2021-2024, across our sample of 164 major metro areas, the shift-share instrument predicts that each additional surge migrant allowed into the country by CBP is associated with an additional 0.21 surge migrants in the EOIR data. If we instead measure surge migrants relative to the initial metro population, the corresponding number is 0.31.

Columns 2-4 show the short-run shift-share first stages for each pair of years. In both panels, the instrument is strongest at the start of the period, with both the coefficient and F-statistic being the highest. The coefficients and F-statistics are lower in the later pairs of years. The declining coefficient may reflect that early surge migrants were most likely to go to traditional destinations or dispersed from them most slowly. It may also reflect a higher share of surge migrants being given a notice to appear in immigration court early in the surge, when flows were smaller and more manageable. The declining statistical significance reflects that even as the number of surge migrant flows rose, the growth for the various surge countries became more similar, weakening the predictive power of the instrument. We report the 2023-2024 coefficients for the largest 30 metros in column 5, so as to compare with buslift results using this sample; the coefficients are smaller for this sample.

We find that the buslift is a sufficiently strong instrument only for 2023-2024, when the numbers increased greatly from small numbers (the initial stage of the buslift involved only Washington, DC, where many migrants disembarked in front of the vice president's residence), so we do not report the other buslift results. During the height of the buslift in 2023-2024 (column 6 top panel), each additional bused migrant is associated with an additional 1.6 surge migrants in the EOIR data. On a per capita basis, each additional bused migrant is associated with an additional 1.2 surge migrants in the EOIR data (bottom panel). The estimated coefficients exceeding 1 suggest that metros that received buses attracted "excess" surge migrants, perhaps because the social services that those cities provided to surge migrants received extensive news coverage, drawing other migrants. Alternatively, bused migrants could have quickly been joined by friends and family members who did not arrive via the buslift. If, however, the buses were sent to metros that were already receiving large numbers of non-bussed surge migrants that year, the instrument is invalid. In results not shown here, the buslift coefficient declines greatly in statistical significance if Denver, the metro with by far the most buslift migrants per capita in 2024, is dropped. This means that the buslift approach is essentially a case study of Denver, a point to which we return below.

Table 2: Impact of instruments on surge migration (first-stage results)

	2021-2024 (1)	2021-2022 (2)	2022-2023 (3)	2023-2024 (4)	2023-2024 (5)	2023-2024 (6)
A. In levels, with full controls:						
Instrument in levels	0.21^{***} (0.03)	0.42^{***} (0.02)	0.17^{***} (0.03)	0.15^{***} (0.03)	0.11^{***} (0.04)	1.56^{***} (0.42) [p=0.03]
Buslift metro indicator variable	--	--	--	--	--	-30.40^{***} (10.20) [p=0.03]
F-statistic	56	350	39	20	8	--
B. Relative to population, with full controls:						
Instrument/ population	0.31^{***} (0.08)	0.54^{***} (0.10)	0.24^{***} (0.06)	0.27^{***} (0.08)	0.16^{***} (0.03)	1.16^{***} (0.17) [p=0.06]
F-statistic	17	29	18	20	17	--
Observations	164	164	164	164	30	30
Instrument	Shift share	Shift share	Shift share	Shift share	Shift share	Buslift

Source: Authors' calculations.
Note: Unweighted OLS coefficients on the shift-share instrument using CBP cases or the buslift, in levels in the top panel and relative to the initial year population in the bottom panel. The outcome is the number of EOIR cases, in levels or relative to the initial population. Regressions in the top panel also control for the 2019 population, the 2016-2019 and 2019-2021 changes in employment, and the 2016-2019 share of employment in each supersector (excluding government); in the last column, the estimated coefficient and SE on the indicator variable are divided by 10,000. Regressions in the bottom panel control for the 2016-2019 and 2019-2021 change in the native employment rate. Robust standard errors in parentheses, with significance levels *** p < 0.01, ** p < 0.05, * p < 0.1. In column 6, the p-value based on the Webb wild bootstrap is in brackets.

IV. Results

We first present the results of estimating equation 1 for total employment and equation 2 for employment by industry and for GDP. Next, we present estimates of equation 2 for wages for all workers in the CES and for native-born workers in the ACS, before examining natives' employment rates in the ACS and domestic migration based on Census Bureau statistics. In our last regressions, we estimate equation 2 for natives' rents (plus utilities) from the ACS and Zillow multifamily rents. We conclude with a table summarizing the effects of the surge on natives by period, taking into account the change in the surge size over time.

IV.A. Total Employment and GDP

Surge migration boosted total employment in destination metros, particularly over the longer run (Table 3). OLS estimates indicate that each additional EOIR case is associated with 0.8 additional jobs over 2021-2024 (column 1, top row). Using the shift-share instrument, total employment increases by about 0.5 for each additional EOIR case when controlling for other variables (column 1, row 2); the estimated impact is much larger, at 1.6, when controls are not included (column 1, row 3). With each EOIR case equivalent to 0.8 surge migrants (figure 4), each surge migrant added about 0.4 jobs over 2021-2024. This estimate is smaller than that estimated by Wilson and Zhou (2026), who report that each working-age surge migrant increased employment roughly one-for-one; we include surge migrants of all ages, in contrast. Nonetheless, our estimate indicates that surge migration played an important role in employment growth over 2021-2024. Based on the shift-share IV result with controls and applying the adjustment factor of 0.8, the surge increased total employment in major metros by about 1.6 million.[36] The surge thus accounted for about 18% of the employment gains over 2021-2024 in those metros.

The year-to-year differences, reported in the remaining columns in Table 3, do not show a clear pattern over time, although most of the point estimates indicate a positive impact. Estimates are largest for 2022-2023 (column 3), when the shift-share IV with controls indicates that total employment increased by 0.8 for each additional EOIR case. When we use the buslift

[36] Based on a total of 3.25 million EOIR cases in the 164 major metro areas. Total employment gains across those metros were 9 million over 2021-2024.

instruments and examine 2023-2024 (column 6), the coefficients are imprecisely estimated despite a reasonably strong first stage (Table 2, top panel, column 6).

Table 3: Effect of surge migrants on total employment

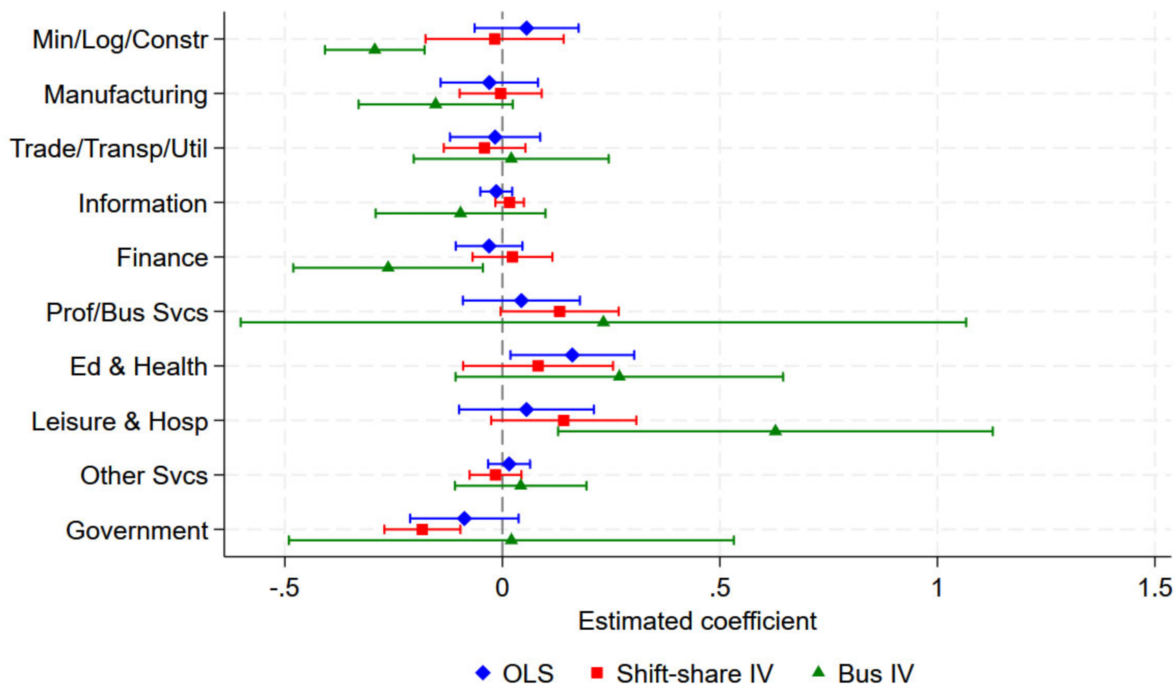
	2021-2024	2021-2022	2022-2023	2023-2024	2023-2024	2023-2024
	(1)	(2)	(3)	(4)	(5)	(6)
OLS	0.79^{***}	0.47	1.08^{***}	0.45^{***}	0.15	—
Full controls	(0.20)	(0.27)	(0.29)	(0.12)	(0.20)	
IV	0.49^{***}	0.23	0.84^{***}	0.48^{***}	0.43	0.15
Full controls	(0.13)	(0.13)	(0.18)	(0.14)	(0.33)	(0.22)
						[p=0.67]
IV	1.61^{***}	3.50^{**}	1.28^{***}	0.62^{***}	0.59^{***}	0.46^{**}
No controls	(0.20)	(1.28)	(0.09)	(0.03)	(0.04)	(0.15)
						[p=0.27]
Observations	164	164	164	164	30	30
Instrument	Shift share	Shift share	Shift share	Shift share	Shift share	Buslift

Source: Authors' calculations.
Note: Unweighted OLS or IV coefficients on surge migrant flows as measured by the number of EOIR cases. The outcome is the difference in employment in the payroll survey over the period indicated. Robust standard errors in parentheses, with significance levels *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. In column 6, the p-value based on the Webb wild bootstrap is in brackets. The regressions in the first two rows also control for the 2019 population, the 2016-2019 and 2019-2021 changes in employment, and the 2016-2019 share of employment in each supersector (excluding government).

The increase in employment was largest in the leisure and hospitality and professional and business services sectors. Figure 8 shows OLS and IV estimates of the change in the share of employment in each supersector over 2021-2024. The OLS estimates suggest that education and health services experienced relative growth in metros that received more surge migrants. The shift-share IV indicates relative growth in professional and business services, a sector that includes temp agencies, janitorial services, and landscaping services. The buslift IV indicates strong relative growth in leisure and hospitality; it also suggests relative growth in education and health services and business and professional services, but the point estimates are imprecise. Relative migrant-spurred growth in those three sectors is not surprising since they include many jobs that require little formal education or training, such as home health care aides.

Surprisingly, the shift-share IV indicates a relative decrease in government, a sector that might be expected to experience relative growth given the need to provide government services to surge migrants, and the buslift IV indicates a relative decrease in construction (together with mining and logging) and in finance. None of the models suggests that surge migration led to an increase in construction employment, although that may simply reflect that migrants working in construction were either self-employed, contract or day laborers, or paid off-the-books. The payroll data do not include two sectors that may have seen an uptick due to surge migration: gig workers, who may have worked in ride-sharing and delivery services, and private households, who may have hired migrants as childcare providers, house cleaners, and the like. The payroll data also do not include agricultural employment, but that would occur largely outside the metro areas we examine.

Figure 8: Longer-run effect of surge migrants on share of employment, by sector



Source: Authors' calculations.
Notes: Unweighted OLS or IV coefficients on surge migrant flows. The outcome is the 2021-2024 difference in the sector's share of employment in the payroll survey. Regressions also control for the change in the share of employment in that sector over 2016-2019 and over 2019-2021. Robust 95% confidence intervals shown.

Since the surge migrants boosted employment, we would expect them to increase GDP, a result we find in Table 4 for 2021-2024. Shift-share IV results for 2021-2024 (column 1) indicate that a one percentage point increase in the surge migrant population share as measured by EOIR cases raised metro GDP about 1.7% over 2021-2024. Adjusting for the EOIR undercount implies that a one percentage point increase in surge migration increased GDP by 1.4%. Given their share of the population (0.9), this implies an aggregate GDP effect of 1.5%. That estimate is larger than the population increase due to surge migration, indicating they boosted GDP per capita. The point estimates indicate that the surge can account for about one-fifth of real GDP growth during 2021-2024 in the 164 major metros we examine.[37]

The impact of surge migrants on GDP falls over time and is no longer significantly positive in 2023-2024 (columns 4-5). The buslift IV estimates for 2023-2024 (column 6) are negative but imprecisely estimated.

Table 4: Effect of surge migrants on GDP

	2021-2024 (1)	2021-2022 (2)	2022-2023 (3)	2023-2024 (4)	2023-2024 (5)	2023-2024 (6)
OLS	1.22*	3.40***	0.61	-0.37	-0.22	—
Full controls	(0.53)	(0.93)	(0.84)	(0.55)	(0.47)	
IV	1.70**	3.07***	2.08*	0.09	-0.05	-1.66**
Full controls	(0.42)	(0.65)	(0.85)	(0.51)	(0.37)	(0.62)
						[p=0.39]
IV	2.33***	3.87***	2.42**	0.69	0.40	-1.95**
No controls	(0.52)	(0.77)	(0.91)	(0.51)	(0.45)	(0.72)
						[p=0.21]
Observations	164	164	164	164	30	30
Instrument	Shift share	Shift share	Shift share	Shift share	Shift share	Buslift

Source: Authors' calculations.

Note: Unweighted OLS or IV coefficients on surge migrant flows as measured by the number of EOIR cases relative to the initial population (in %). The outcome is the difference in the log of real GDP over the period indicated (×100). Robust standard errors in parentheses, with significance levels *** p < 0.01, ** p < 0.05, * p < 0.1. In column 6, the p-value based on the Webb wild bootstrap is in brackets. The regressions in the first two rows also control for the 2016-2019 and 2019-2021 differences in log GDP.

IV.B. Wages for All workers and U.S. Natives

Turning to average wages in the payroll survey data, we find some evidence that wages may have fallen as a result of surge migration (Table 5). The longer-run shift-share IV estimate with controls (column 1, row 2) indicates that a one percentage point increase in surge migrants as measured by EOIR cases as a share of the metro population reduced average real hourly wages by about 1.7%, albeit statistically significant only at the 10% level. Adjusting for the EOIR undercount, the implied effect is about -1.4%. The corresponding effect on average weekly wages in Wilson and Zhou (2026) is -0.9% and is not statistically significant. Average real hourly wages fell by 3.1% during our sample period, so the point estimate indicates that

[37] GDP increased by 6.5%, on average, in the 164 major metros we examine.

surge migration can account for 49% of that drop by reducing wages by 1.5%. The estimates with the shift-share instrument are also negative for most year-to-year differences (columns 2-5), and the bus instrument coefficients (column 6) are again imprecise. Given the standard errors, it is difficult to rank the short-run versus longer-run and early surge versus late surge aggregate wage impacts.

Table 5: Effect of surge migrants on average wage

	2021-2024	2021-2022	2022-2023	2023-2024	2023-2024	2023-2024
	(1)	(2)	(3)	(4)	(5)	(6)
OLS	-1.18	-1.88	-1.42	0.03	-0.10	—
Full controls	(0.63)	(1.39)	(1.24)	(0.72)	(1.28)	
IV	-1.74*	-1.62	-2.45*	-1.14	-2.58***	2.13
Full controls	(0.80)	(1.42)	(1.08)	(0.78)	(0.73)	(2.21)
						[p=0.87]
IV	-1.98	-1.53	-3.21*	-1.34	-2.20**	2.06
No controls	(1.03)	(1.49)	(1.60)	(0.81)	(0.77)	(2.50)
						[p=0.87]
Observations	164	164	164	164	30	30
Instrument	Shift share	Shift share	Shift share	Shift share	Shift share	Buslift

Source: Authors' calculations.
Note: Unweighted OLS or IV coefficients on surge migrant flows as measured by the number of EOIR cases relative to the initial population (in %). The outcome is the difference in the log of the average real hourly wage in the payroll survey over the period indicated. Robust standard errors in parentheses, with significance levels *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. In column 6, the p-value based on the Webb wild bootstrap is in brackets. The regressions in the first two rows also control for the 2016-2019 and 2019-2021 differences in log wages.

Compositional changes in the workforce as a direct result of surge migration likely contributed to the decrease in the average hourly wage. Of course, we cannot directly estimate the contribution, as we know neither the wages nor the employment rate of surge migrants. However, compositional changes could explain only a fraction of the effects implied by our point estimates. Even if surge migrants worked for nothing, a composition effect would imply that a one percentage point increase in their population share, were their employment rate the same as that of others, would reduce wages by only 1%. A -1.7% coefficent (Table 5, column 1, row 2) would require in addition that the surge migrant employment rate be 70% higher than that of others. On the other hand, given the standard errors, we cannot rule out there being no effect on overall wages at all. We can test whether the negative point estimate is due in part to a decline in the wages of other low-education workers or there were other compositional changes that coincided with, or were caused by, surge migration; we cannot test whether later surge migrants reduced the wages of earlier surge migrants.

For native workers, the coefficients in Table 6 column 1 show instead that a one percentage point increase in surge migrants over 2021-2024 has a statistically significant positive effect on (adjusted) native wages of 1.3% in OLS and 1.0% with the shift-share instrument, in both cases controlling for wage changes in 2019-2021 and 2016-2019. The unreported coefficient on the 2019-2021 wage change is large and statistically significantly negative, suggesting it captures post-COVID reversion to the mean. Without the past wage change covariates, the corresponding IV coefficient is similar at 1.1%, suggesting it is not greatly biased upward.[38] The IV coefficient is only slightly lower than the corresponding 1.3% coefficient of Clemens et al. (2026) (their Appendix Table 1). To account for the EOIR case undercount, all coefficients (including those of Clemens et al. 2026) should be multiplied by 0.8.[39]

The positive wage effect could be due to demand factors that cushion the impact of greater labor supply, such as an economic boost caused by the arrival of immigrants who could initially consume but not yet work. Clemens et al. (2026) instead suggest complementarity between surge migrants and the average U.S. native as the cause of the positive wage effect. Earlier immigrants from the surge countries (other than Colombia, Venezuela, and Ukraine) had low education, making them particularly substitutable with native high school dropouts and to a lesser degree with native high school graduates, and potentially complementary with more educated natives. A positive overall wage effect would result if complementarity outweighed substitution.

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[38] Our standard errors are similar with IV and OLS estimation, which may reflect the removal of measurement error from the EOIR-based endogenous variable offsetting factors which usually lead to larger standard errors with IV.

[39] However, they report a 3.1% coefficient for the 2021-2023 period, whereas our unreported coefficient for this period is only 1.8%. A number of differences could account for the 2021-2023 discrepancy. Clemens et al. (2026) weight regressions, do not adjust outcomes for individual characteristics, use EOIR data to construct the shift-share instrument, leave out one metro from their instrument (as is best practice), use commuting zones rather than metros, extend the analysis to smaller localities, drop observations with outlier instrument values, and use net (not gross) surge immigration. Weighting our regressions with the 2019 population makes our coefficients more different, reducing them to 0.8% for 2021-2024 (online appendix table A5) and 1.6% for 2021-2023 (unreported). Using unadjusted wages raises our coefficients marginally to 1.1% for 2021-2024 (online appendix table A6) and 1.9% for 2021-2023 (unreported). The analysis of Wilson and Zhou (2024) appears to be similar to that of Clemens et al. (2026), but uses weekly wages and a novel method and slightly different data to construct the shift-share instrument. They find an entirely different IV coefficient of -0.7% for 2021-2024, statistically significant at the 10% level, and do not report the OLS coefficient.

The economy should adjust to an initial boom in demand either by local responses such as the adoption of different technologies or generation of more capital, or the dispersal of the local effect across metros through flows of people, goods, or capital. The same could be true for adjustment to increased complementarity, as native workers enter the most complementary occupations, for example. Such adjustment is consistent with our finding that the estimated wage impacts are considerably more positive for the beginning of the surge in 2021-2022 than in the longer run (Table 6, column 2): an increase in surge migrants of one percentage point increases wages statistically significantly by 3.3% with OLS and 2.3% with shift share IV; the IV coefficient is 2.8% without past wage change covariates. Using IV and adding covariates thus reduces the estimated effect considerably.[40]

Table 6: Effect of surge migrants on native log wages adjusted for gender, age, and education

	2021-2024 (1)	2021-2022 (2)	2022-2023 (3)	2023-2024 (4)	2023-2024 (5)	2023-2024 (6)
OLS	1.3***	3.3***	1.0	-0.8	-1.0	—
Full controls	(0.3)	(1.0)	(0.9)	(0.6)	(0.7)	
IV	1.0***	2.3***	1.3	-0.4	-0.5	-2.6***
Full controls	(0.3)	(0.7)	(0.9)	(0.7)	(0.5)	(0.6)
						[p=0.04]
IV	1.1***	2.8***	1.3	-0.6	-0.7	-2.4**
No controls	(0.3)	(0.8)	(0.9)	(0.6)	(0.4)	(0.5)
						[p=0.05]
Observations	164	164	164	164	30	30
Instrument	Shift share	Shift share	Shift share	Shift share	Shift share	Buslift

Source: Authors' calculations.
Notes: Unweighted OLS or IV coefficients on surge migrant flows/population (in %), multiplied by 100. The outcome is the differenced average native adjusted log real hourly wage in the ACS. Full controls refers to covariates for the 2019-2021 and 2016-2019 changes in (unadjusted) native log wage. Parentheses contain robust standard errors (and asterisks refer to these); brackets contain Webb wild bootstrap p-values (column 6).
*** p<0.01, ** p<0.05, * p<0.10

Furthermore, examination of columns 2-4 in Table 6 shows that the short-run shift-share effects decline with time. Our preferred coefficient of 2.3% for 2021-2022 (column 2) falls to a statistically insignificant 1.3% for 2022-2023 and to a statistically insignificant -0.4% for 2023-2024. The fact that the short-run reduced-form effects also decline over time (online appendix

[40] Despite the stronger first stage for 2021-2022 than for 2021-2024, the standard errors in the second stage are larger for 2021-2022.

Table A7) supports the interpretation of the Table 6 temporal pattern as a declining causal effect. Because later surge immigration was expected and could be planned for, the short-run coefficient pattern is consistent with long-run adjustment of the economy.

In column 5, we restrict the shift-share sample to the largest 30 metros and find this to have little effect on the 2023-2024 coefficient in column 4. Here and in later tables, the aim of this restriction is to verify that any different result with the bus IV does not simply reflect the smaller sample.

The bus IV coefficient based on the specification with full controls (Table 6, column 6, row 2), suggests that a one percentage point increase in surge migrants due to the buslift reduces native wages by 2.6%. The coefficient is statistically significant from zero, but is unlikely to be statistically significantly different from the column 5 coefficient of -0.5, meaning we cannot rule out that it reflects the reduction of the sample to 30 metros rather than the use of the bus instrument. The difference is nevertheless suggestive of different LATE effects, either because bus passengers and surge migrants choosing destinations based on ethnic networks have different characteristics, or because the impact of migrants with no choice of destination is more negative than the impact of migrants availing themselves of networks.

It is also possible that the effect was different because bus migrants went to different places from surge migrants generally, notably (in terms of population share) to Denver. The bus instrument is strong for 2023-2024 precisely because 2024 is when the number of passengers bused to Denver soared from 207 to the surely unimagined height of 19407: if the Denver economy offered less migrant-native complementarity, or if the time and place were less propitious for a consumption-driven boom, the short-run wage effect could be negative in response to an unexpected shock. The magnitude is nevertheless large compared to those in the general immigration literature, even if adjusted by the EOIR case undercount factor of 0.8.

Although we have already taken steps to control for metro area variation in the rebound from the COVID recession, there is still some concern that the regressions may be picking up post-pandemic wage effects. We investigate this issue further by adding 2019 and 2020 to the regressions we use to adjust wages for gender, age and education. We note that the unreported residuals jump up in 2020 and remain constant in 2021, before (as already shown in online appendix Table A1) jumping down in 2022, with all jumps particularly pronounced at higher residual percentiles (not shown). If we add a control for the 2019-2021 change in these residuals to the Table 6 regressions for 2021-2022, its coefficient is statistically significant and very negative, but does not affect the surge migration coefficients. If we also control for the change in the 90th percentile residuals, the coefficient on surge migration is reduced somewhat but remains statistically significant positive. We therefore believe our positive wage impacts for regressions involving 2021 are not an artefact of the recovery from the COVID recession.

Different concerns lead us to re-estimate the shift-share regressions omitting Ukraine, Cuba and Haiti from the construction of the instrument. For these three countries, the difference between total counts in the EOIR data versus the CBP counts is particularly large, affecting the interpretation of the IV coefficient on surge migration. Furthermore, Cuba has a large Rotemberg weight share of 48% in the 2021-2022 first stage (online appendix table A4). Omitting these origins renders the coefficient more clearly the effect of a one percentage point increase in the surge migrant share, but also changes the LATE interpretation since the immigrants in question are from fewer origin countries. The new coefficients are smaller (online appendix Table A8), at 70-90% of the magnitudes of the positive coefficients in Table 6 columns 1-3 (supporting the use of an adjustment factor of 0.8 above) and with a statistically significant coefficient of -0.8 compared to -0.5 in column 4. Another way of addressing the mismatch between EOIR and CBP counts is to focus on the reduced form (online appendix Table A7; like the IV results, the short-run effects fall with time). A different robustness check shows the coefficients from regressions weighted by the 2019 metro population are slightly less positive, or more negative, than those in Table 6 (online appendix table A5).

Estimation using native samples split by education should shed light on the hypothesis that positive wage effects are caused by surge migrant complementarity with natives. We therefore present results based on these samples and for our preferred IV specification controlling for past wage changes in Table 7.[41] However, due to large standard errors, no coefficient is statistically significant and the test is inconclusive. Almost all point estimates using the shift share instrument are positive. However, even based on the point estimates the only pattern that emerges is a possibly particularly large positive effect for high school dropouts,

[41] Although the samples are based on education, we retain the 2019-2021 log wage change control for natives of all education levels. The weighted average of the coefficients by education does not correspond exactly to the coefficients for all education groups in Table 6 because the coefficients on the wage change covariates vary by education.

contrary to the theory. Over 2021-2024 (column 1), the positive coefficient of 0.9% for college graduates is statistically significant at the 10% level.

Table 7: Effect of surge migrants on native log wages by education of natives

	2021-2024	2021-2022	2022-2023	2023-2024	2023-2024	2023-2024
	(1)	(2)	(3)	(4)	(5)	(6)
High school dropouts	2.1*	4.0	-2.1	3.2	5.3*	2.1
	(1.1)	(3.6)	(2.7)	(3.0)	(2.8)	(5.6)
						[p=0.74]
High school graduates	0.9	0.4	2.3	0.1	0.3	-4.0
	(1.2)	(3.0)	(1.5)	(1.9)	(1.6)	(2.5)
						[p=0.21]
Some college	0.7	3.2	1.0	-2.4	-0.4	-5.1**
	(0.8)	(2.2)	(1.6)	(1.5)	(1.0)	(2.5)
						[p=0.57]
College graduates	0.9*	2.2	1.2	0.0	-1.5	-1.9*
	(0.5)	(1.7)	(1.6)	(1.3)	(1.0)	(1.1)
						[p=0.19]
Observations	164	164	164	164	30	30
Instrument	Shift share	Shift share	Shift share	Shift share	Shift share	Buslift

Source: Authors' calculations.
Notes: Unweighted IV coefficients on surge migrant flows/population (in %), multiplied by 100. The outcome is the differenced average native adjusted log real hourly wage in the ACS. All regressions include the two-year 2019-2021 and three-year 2016-2019 changes (unadjusted) in the native log wage. Parentheses contain robust standard errors (and asterisks refer to these); brackets contain Webb wild bootstrap p-values (rows involving the bus instrument). *** p<0.01, ** p<0.05, * p<0.10.

The positive effect of the surge migrants on native wages over 2021-2014 and the early surge years cannot explain why aggregate wages (immigrants and natives together) generally fall in these periods by more than a composition effect could explain. Some of the aggregate decline could be caused by composition effects among natives that we have removed before performing the above wage regressions. However, using unadjusted wages does not flip any of the positive coefficients' signs (online appendix table A6). Some of the aggregate decline could be due to a decline in wages of non-surge immigrants, and we therefore repeat all the Table 6 regressions for a sample of immigrants (those present in the ACS). The coefficients are very imprecisely estimated and have varying signs, but the IV point estimate for 2021-2024 is approximately zero (online appendix table A9). This is consistent with Wilson and Zhou (2026) and Clemens et al (2026). The disparity between the imprecise negative effect of the surge migrants on the aggregate wage and our positive effects for natives' wages in the ACS thus remains unresolved.

IV.C. Native Employment Rates

We investigate the impact of surge migrants on the (adjusted) native employment rate in Table 8. None of the coefficients is statistically significant, even at the 10% level. While we cannot rule out large effects (of varying signs) in columns 2-6, the smaller standard errors and small coefficients for 2021-2024 (0.0 in our preferred specification, column 1) allow us to rule out moderate longer-run effects.

Our preferred shift-share coefficient of 0.0% (Table 8, column 1) is similar to the corresponding statistically insignificant coefficient of 0.2% in Clemens et al. (2026) (their Appendix Table 1). The Clemens et al. (2026) statistically significant coefficient of 1.7% for 2021-2023 is much larger, however and much larger than both our unreported coefficient of 0.1% and the Wilson and Zhou (2026) coefficient of 0.2% for 2021-2023. The point estimates using the bus instrument are larger in absolute value than the shift-share coefficients, implying that a one percentage point increase in the surge migrant share reduces employment by a large 0.8-0.9 percentage points. The coefficients are similar if the employment rate is not adjusted at the individual level, if Ukraine, Cuba, and Haiti are excluded from the shift-share instrument and if the regressions are weighted by 2019 population (online appendix table A8); the reduced form coefficients are also similar (online appendix table A7).

Our finding of no effect on the native employment rate is surprising if the positive wage effects noted above for the whole period and the early surge are caused by a rise in aggregate demand, but the analysis might obscure differences by native education. In Table 9, we therefore present the results of regressions based on four samples of natives, focusing on our preferred IV specifications (those with controls for past employment changes). Most of the coefficients are statistically insignificant, although the initial 2021-2022 response (column 2) seems to have different effects by education: in response to a one percentage point increase in surge migrants, the high school graduate employment rate fell by 2.8 percentage points (significant at the 10% level) while college graduate rose by a statistically significant 2.3 percentage points. These are surprisingly consequential magnitudes, but the fact that this pattern reverses in 2022-2023 (column 3) leaves the interpretation unclear.

Table 8: Effect of surge migrants on the native employment rate (%) adjusted for gender, age, and education

	2021-2024 (1)	2021-2022 (2)	2022-2023 (3)	2023-2024 (4)	2023-2024 (5)	2023-2024 (6)
OLS	-0.1	0.4	-0.3	-0.4	-0.2	—
Full controls	(0.1)	(0.4)	(0.5)	(0.4)	(0.3)	
IV	0.0	0.3	-0.2	-0.3	0.0	-0.8**
Full controls	(0.1)	(0.3)	(0.4)	(0.4)	(0.2)	(0.3)
						[p=0.23]
IV	0.2	0.9	-0.3	-0.4	-0.0	-0.9**
No controls	(0.1)	(0.6)	(0.4)	(0.5)	(0.3)	(0.4)
						[p=0.12]
Observations	164	164	164	164	30	30
Instrument	Shift share	Shift share	Shift share	Shift share	Shift share	Buslift

Source: Authors' calculations.
Notes: Unweighted OLS or IV coefficients on surge migrant flows/population (in %). The outcome is the differenced average native adjusted employment rate from the ACS. "Full controls" refers to the two-year 2019-2021 and three-year 2016-2019 changes in the (unadjusted) native employment rate. Parentheses contain robust standard errors (and asterisks refer to these); brackets contain Webb wild bootstrap p-values (column 6).
*** p<0.01, ** p<0.05, * p<0.10.

Table 9: Effect of surge migrants on native employment rate by education of natives

	2021-2024 (1)	2021-2022 (2)	2022-2023 (3)	2023-2024 (4)	2023-2024 (5)	2023-2024 (6)
High school	-0.8	-2.8*	2.6**	-1.0	0.5	-4.7***
dropouts	(0.8)	(1.7)	(1.2)	(1.2)	(0.6)	(1.0)
						[p=0.08]
High school	0.1	-0.3	-0.4	0.6	0.6	-1.3
graduates	(0.3)	(0.8)	(1.0)	(0.7)	(0.5)	(1.0)
						[p=0.14]
Some college	0.1	0.3	-0.2	-0.2	0.6	0.0
	(0.2)	(0.9)	(0.9)	(0.8)	(0.7)	(0.6)
						[p=0.98]
College	0.3	2.3***	-1.2*	-0.6	-0.9***	-0.3
graduates	(0.2)	(0.8)	(0.6)	(0.6)	(0.2)	(0.3)
						[p=0.34]
Observations	164	164	164	164	30	30
Instrument	Shift share	Shift share	Shift share	Shift share	Shift share	Buslift

Source: Authors' calculations.
Notes: Unweighted IV coefficients on surge migrant flows/population (in %). All regressions include the 2019-2021 change in the native employment rate (unadjusted). Parentheses contain robust standard errors (and asterisks refer to these); brackets contain Webb wild bootstrap p-values (column 6). *** p<0.01, ** p<0.05, * p<0.10.

IV.D. Domestic Migration

Local short-run impacts of immigration may be absorbed not only by local responses in the long run, but also by national-level responses such as domestic migration that spread the local effects to other geographies. To assess whether this is occurring in our context, we analyze the impact of surge migration on net domestic in-migration (of all persons, unadjusted) in Table 10. In specifications including the sums of net domestic in-migration for 2016-2019 and 2020-2021, the 2021-2024 effect of a one percent point increase in the surge migration share is -0.6 percentage points with OLS and -0.9 percentage points with IV, both statistically significant (column 1). Thus, the increase in surge migration over 2021-2024 was almost entirely offset by reduced domestic in-migration. This would seem to imply there should be no detectable effect on any outcome, including wages, with all effects of the surge migration spread to larger regions. However, there could still be detectable effects if the international and internal migrants have different skills. Unfortunately, our efforts to examine the internal migration effects by internal migrant education foundered upon the problem of small sample sizes.

In contrast to the results for other outcomes, the short-run shift-share effects of surge migration on domestic in-migration strengthen as the surge progresses. All are statistically significant in the preferred IV regression of Table 10, row 2: the 2021-2022 coefficient is -0.5 percentage point (column 2); the 2022-2023 coefficient is -0.9 percentage point (column 3); and the 2023-2024 coefficient is -1.2 percentage points (column 4), the latter implying an offset of more than 100% (although the coefficient is not statistically significantly different from -1). The shift-share coefficients are slightly less negative when Ukraine, Cuba, and Haiti are excluded from the instrument, and the increasing short-run response over time is less marked (online appendix table A8).

The temporal pattern of increasingly negative effects on net in-migration does not appear in the reduced form results, however (online appendix table A7). This is important for the interpretation, indicating that the IV pattern is entirely due to the declining first stage coefficient. One interpretation is that as the share of immigrants choosing their location based on ethnic networks (“compliers”) declines, their characteristics change in a way that leads to more negative LATE (treatment on the treated) effects while the equivalent of the intent to treat effect is stable. The steady cumulation of the reduced form (intent to treat) effects explains why, contrary to the case for wages (and rents below), the long run 2021-2024 coefficient of -0.9 percentage points is stronger than the -0.5 coefficient for 2021.[42]

By contrast to these shift-share IV results, the Table 10 preferred column 5 coefficient using the buslift instrument is 0.1 percentage point, statistically insignificant mainly due to its small magnitude. Net migration is a form of adjustment, so any surprise element in the buslift to Denver would not have increased short-run net migration. The small coefficient suggests instead that Venezuelans and Colombians stimulated less response in domestic in-migration than did surge migrants generally.

Table 10: Effect of surge migrants on net domestic in-migration

	2021-2024 (1)	2021-2022 (2)	2022-2023 (3)	2023-2024 (4)	2023-2024 (5)	2023-2024 (6)
OLS	-0.6***	-0.6*	-0.7***	-0.6***	-0.3	—
Full controls	(0.2)	(0.3)	(0.2)	(0.1)	(0.3)	
IV	-0.9***	-0.5**	-0.9***	-1.2***	-1.3***	0.1
Full controls	(0.2)	(0.2)	(0.2)	(0.2)	(0.3)	(0.1)
						[p=0.61]
IV	-0.6	-0.1	-0.7	-1.1***	-1.5***	-0.4
No controls	(0.4)	(0.6)	(0.5)	(0.3)	(0.4)	(0.4)
						[p=0.21]
Observations	164	164	164	164	30	30
Instrument	Shift share	Shift share	Shift share	Shift share	Shift share	Buslift

Source: Authors' calculations.
Notes: Unweighted OLS or IV coefficients on surge migrant flows/population (in %). The outcomes are domestic in-migration rates (in %) in columns 2-6 and the three-year sum of domestic in-migration divided by the 2021 population in column 1. "Full controls" refers to the sums of domestic in-migration (divided by initial population) for 2016-2019 and 2020-2021. Parentheses contain robust standard errors (and asterisks refer to these); brackets contain Webb wild bootstrap p-values (column 6). *** p<0.01, ** p<0.05, * p<0.10

IV.E. Rents for Natives and for Multifamily Residences

Our focus thus far has been the labor market, but the U.S. housing market has been shown to be affected by immigration in other contexts and, in this section, we examine rents, first of U.S. natives (with the unit of observation at the micro level being a person), then of multifamily residences (with the unit of observation at the micro level being a residence). The surge increased long-run native real monthly rents (including utilities) statistically significantly

[42] This line of reasoning applied to native wage (and rent) outcomes suggests that the economy may adjust even faster than the temporal decay of the IV coefficients suggests, since the decay is happening despite the complier effect operating in the opposite direction.

over 2021-2024, based on OLS and the shift-share instrument (Table 11, column 1). In the IV specification controlling for past rent changes, a one percentage point increase in surge migrants increased rents by 1.8% for the sample of 164 metros in 2021-2024. This modest increase (modest particularly when adjusted to 1.4% to reflect the EOIR case undercount) is consistent with the 90% offset of the surge migration by domestic net in-migration.

Table 11: Effect of surge migrants on native log real gross rent, adjusted for gender, age, and education

	2021-2024 (1)	2021-2022 (2)	2022-2023 (3)	2023-2024 (4)	2023-2024 (5)	2023-2024 (6)
OLS	1.9***	3.0**	3.3	-0.7	-0.8	—
Full controls	(0.6)	(1.5)	(2.0)	(1.3)	(1.2)	
IV	1.8***	2.9**	6.2**	-2.8	0.2	-0.7
Full controls	(0.6)	(1.3)	(2.5)	(2.2)	(1.0)	(1.2) [p=0.62]
IV	1.6**	2.4**	6.0***	-2.5	-0.1	-1.0
No controls	(0.7)	(1.2)	(2.3)	(2.1)	(1.2)	(1.4) [p=0.68]
Observations	164	164	164	164	30	30
Instrument	Shift share	Shift share	Shift share	Shift share	Shift share	Buslift

Source: Authors' calculations.
Notes: Unweighted OLS or IV coefficients on surge migrant flows/population (in %). The outcome is differenced adjusted native log rent from the ACS. Rents include utilities and are deflated by the CPI excluding shelter (2024 $). "Full controls" refers to the two-year 2019-2021 and three-year 2016-2019 changes in the (unadjusted) log rent. Parentheses contain robust standard errors (and asterisks refer to these); brackets contain Webb wild bootstrap p-values (column 6). *** p<0.01, ** p<0.05, * p<0.10.

The short-run shift-share results (Table 11, columns 2-4) show that, as with wages, the effects fade over time. The 2021-2022 coefficient of 2.9% and the 2022-2023 coefficient of 6.2% (both statistically significant) are larger than the longer-run 2021-2024 coefficient in column 1, indicating that the housing market is adjusting over time. The coefficients should be multiplied by the EOIR case undercount factor of 0.8. The peak adjustment in 2022-2023 is later than for wages, which may reflect the confluence of new arrivals with many surge migrants from the previous year leaving shelters or accommodation with family and friends to seek their own rental housing. When the sample is restricted to the largest 30 metros for 2023-2024 (column 5), the IV coefficient is very small (0.2%), and for these same metros using the bus instrument in column 6, the negative coefficient of -0.7% is not close to statistically significant.[43]

When we repeat the regressions omitting Ukraine, Cuba, and Haiti from the construction of the instrument (online appendix table A8), the coefficients in columns 1-3 are 5082% of those in Table 11, suggesting that the 0.8 adjustment factor for the EOIR case undercount may be too high for rents. The short-run reduced-form coefficients fall over time as the IV coefficients do (online appendix Table A7), while coefficients from weighted regressions confirm the considerable and statistically significant rent response in 2022-2023, while suggesting the response in other years was minor.

The aggregate results, i.e., effects for all people in multifamily housing, confirm the results for natives: surge migration pushed up rents, especially early in the surge. The 2021-2024 shift-share IV coefficient of 1.6 (Table 12, column 1, row 2) is similar to the corresponding coefficient of 1.8 in Table 11. The results are also similar to the preferred coefficient of 1.4 for Zillow rents found by Wilson and Zhou (2026); a comparison with papers not using EOIR to measure immigration would require multiplying our coefficient by 0.8.[44] The coefficient implies an increase in rent over 2021-2024 of a modest 1.4%. The short-run impacts diminish over time (columns 2-4), more quickly than in the natives' rent results reported in Table 11: 2021-2022 is the only period in which aggregate rents rise, with the statistically significant shift-share IV coefficient of 6.7% in 2021-2022 falling to a weakly statistically significant -1.8% in 2023-2024 for our full sample (row 2). The difference reflects the fact that Zillow data refer to new rental contracts, which respond quickly to changes in supply and demand, while ACS data reflect the stock of contracts, which typically change only annually. The buslift IV suggests an increase in 2023-2024 but is imprecisely estimated (column 6).

[43] We have experimented with using the sample of the 44 metros for which Gyourko et al. (2021) provide a measure of housing supply inflexibility and interacting this measure with the surge migrant share. The point estimate on the interaction is consistent with surge migrants raising rents more in inflexible housing markets, but it is not statistically significant.

[44] However, their construction of the surge variable differs from ours (theirs is based on working-age net surge migration, with employment – not population – as the denominator), and to find the total effect, they multiply 1.4 by a 3.1 percentage point average increase in their sample, whereas our average increase is 0.9 percentage points.

Table 12: Effect of surge migrants on average rent

	2021-2024 (1)	2021-2022 (2)	2022-2023 (3)	2023-2024 (4)	2023-2024 (5)	2023-2024 (6)
OLS	0.6	6.1***	-0.7	-2.6**	-2.8*	—
Full controls	(0.6)	(1.2)	(1.0)	(0.9)	(1.3)	
IV	1.6***	6.7***	-0.8	-1.8*	0.2	1.5
Full controls	(0.4)	(1.0)	(0.7)	(0.8)	(1.5)	(2.8)
						[p=0.62]
IV	1.7***	6.5***	-0.3	-1.5	-0.3	2.5
No controls	(0.5)	(0.9)	(0.7)	(0.9)	(1.0)	(3.0)
						[p=0.46]
Observations	92	92	92	92	30	30
Instrument	Shift share	Shift share	Shift share	Shift share	Shift share	Buslift

Source: Authors' calculations.

Note: Unweighted OLS or IV coefficients on surge migrant flows as proxied by the number of EOIR cases relative to the initial population (in %). The outcome is the difference in the log of average real multifamily rent over the period indicated (×100). Robust standard errors in parentheses, with significance levels *** p < 0.01, ** p < 0.05, * p < 0.1. In column 6, the p-value based on the Webb wild bootstrap is in brackets. The regressions in the first two rows also control for the 2016-2019 and 2019-2021 differences in log rent.

IV.F. Summary of Surge Impact on U.S. Natives

To this point, we have concentrated on the coefficients and thus the response to a given influx of surge migrants. To find the effect of the surge, we must multiply the coefficients by the actual change in the surge migrant share as measured by EOIR cases. We lay out the results for each time horizon in Table 13, based on coefficients for our preferred IV specifications for each native outcome in Tables 6, 8, 9, and 11.

Table 13: The impact of the surge on native labor market outcomes and rent

	2021-2024 (1)	2021-2022 (2)	2022-2023 (3)	2023-2024 (4)	2023-2024 (5)
Wages (%)	0.9***	0.4***	0.4	-0.3	-1.8***
Employment rate (percentage points)	0.0	0.0	0.0	-0.1	-0.5**
Rent (%)	1.6***	0.5**	1.8**	-1.4	-0.5
Surge migrants/population (percentage points)	0.9	0.2	0.3	0.5	0.7
Observations	164	164	164	164	30
Instrument	Shift share	Shift share	Shift share	Shift share	Buslift

Source: Authors' calculations.

Notes: Impacts of surge migration on native outcomes: average change in surge migrant share for each period, multiplied by IV coefficients (Row 2, "IV, Full controls") from table 6 (wages), table 8 (employment rate), and table 11 (rent). The "Surge migrants / population" row is a descriptive figure (not a regression coefficient) and is not drawn from any of the tables above. Asterisks indicate whether the underlying coefficient was statistically significant, based on robust standard errors in columns 1-4 and the Webb wild bootstrap p-value in column 5 — not the significance of the multiplied impact figure shown here, which has no standard error of its own.

Based on the shift-share instrument, the surge raised native wages by 0.9% over the whole 2021-2024 period (first row, column 1), a small effect. The timing of gains within that period reflects the competing effects of falling responsiveness to surge migration, as the economy adjusts, and increasing surge migration. The net effect is that native wages rose by equal amounts in 2021-2022 and 2022-2023 (0.4%, columns 2 and 3), and turned slightly negative (although statistically insignificantly so) in 2023-2024 (-0.3% in column 4). Based on the bus instrument, the surge reduced 2023-2024 native wages by a moderate 1.8% (column 5). The bus instrument calculations yield a moderate 0.5 percentage point decline in the employment rate for 2023-2024 (second row, column 5), while the impact is zero throughout based on the shift-share instrument.

The surge pushed up rents by a moderate 1.6% over the whole 2021-2024 period (third row, column 1). Because rent's responsiveness and surge migration both rose in the early stages of the surge, the small 2021-2022 rent increase of 0.5% (column 2) rose to 1.8% in 2022-2023 (column 3). The effect on rents apparently became negative in 2023-2024, though this -1.4% is imprecisely estimated. Since renters spend approximately 30% of their income on rent, we can calculate (assuming each worker pays all the rent and initially pays exactly 30% of income in rent) that over 2021-2024, the surge meant natives' wage net of rent rose (0.9-0.3*1.6)/0.7=0.6%.[45]

V. Conclusion

We find that surge migrants boosted employment, GDP, and GDP per capita over 2021-2024, while leaving the native employment rate unchanged. Although surge migrants may have reduced overall wages, in part due to their own wages depressing the average, they boosted native wages, likely by stimulating consumption while work restrictions initially constrained surge migrant labor supply. We cannot rule out that the wage boost was the result of complementarity between surge migrants and natives, however. Meanwhile, rent increases offset the benefit of higher wages to natives, but we calculate that the average U.S. native renter's wage net of rent rose at least 1.5%.

We find interesting dynamics when looking at year-to-year changes during the three-year period. The short-run responses of native wages and rents to surge migration declined over time and even reversed in the case of surge migrants who were bused. Changes in domestic migration that offset the surge immigration played a key role in dampening these responses over time. They also imply that the impacts of the surge migration on affected metros came to be diffused across larger geographies over time and were no longer detectable in cross-metro comparisons. Although the magnitude of surge migration was increasing over 2021-2024, this was not enough to keep the total impact of the surge as measured across metros from falling, eventually to zero.

One important takeaway from our results is that the U.S. economy appears to have largely absorbed an unprecedented influx of migrants with few adverse economic effects. One of the lessons, however, is that flexibility of labor markets is key when shocks like these occur. The response of domestic migration was a crucial mechanism in attenuating the impact of the migration surge on destination cities. Of course, it is also likely that the unique circumstances of the pandemic recovery, marked by the inability of employers to hire at the existing wage, trillions of dollars in government stimulus and historically low interest rates, contributed to the overall lack of negative effects of the migration surge.

[45] Unreported results show that a one percentage point surge increase raised native renters' wages by 2.2% over 2021-2024, translating into a 0.9*2.2=2.0% increase and hence a 2.2% increase in post-rent wage. We have not yet checked for employment effects for native renters and assume them to be zero as for all natives.

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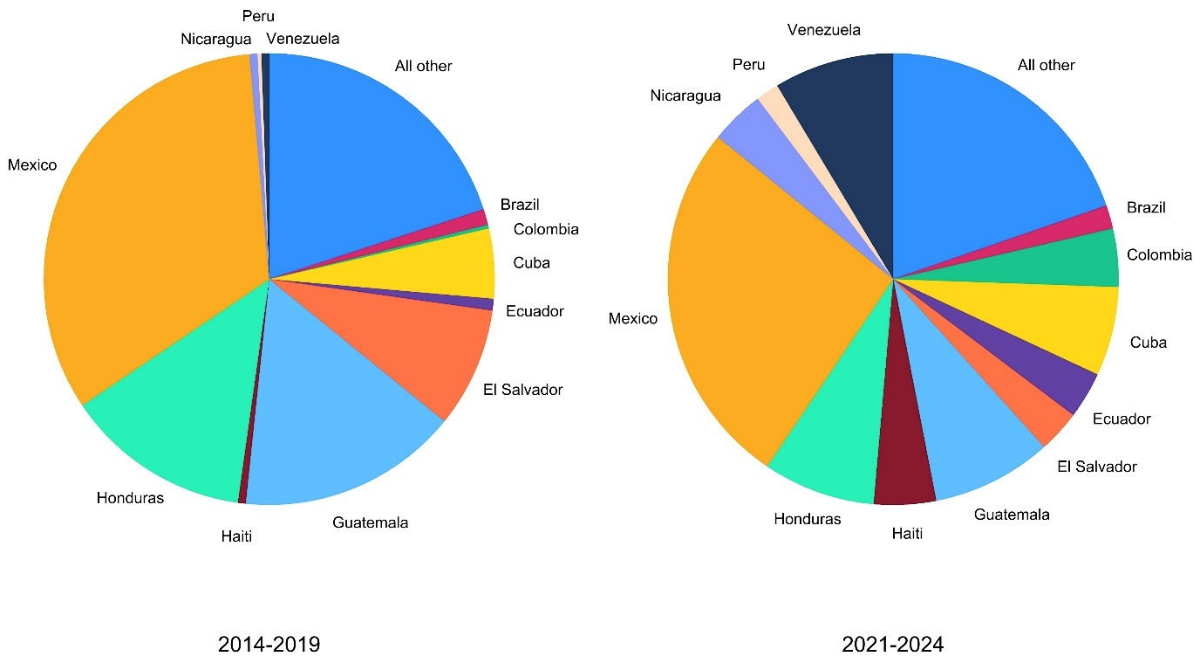
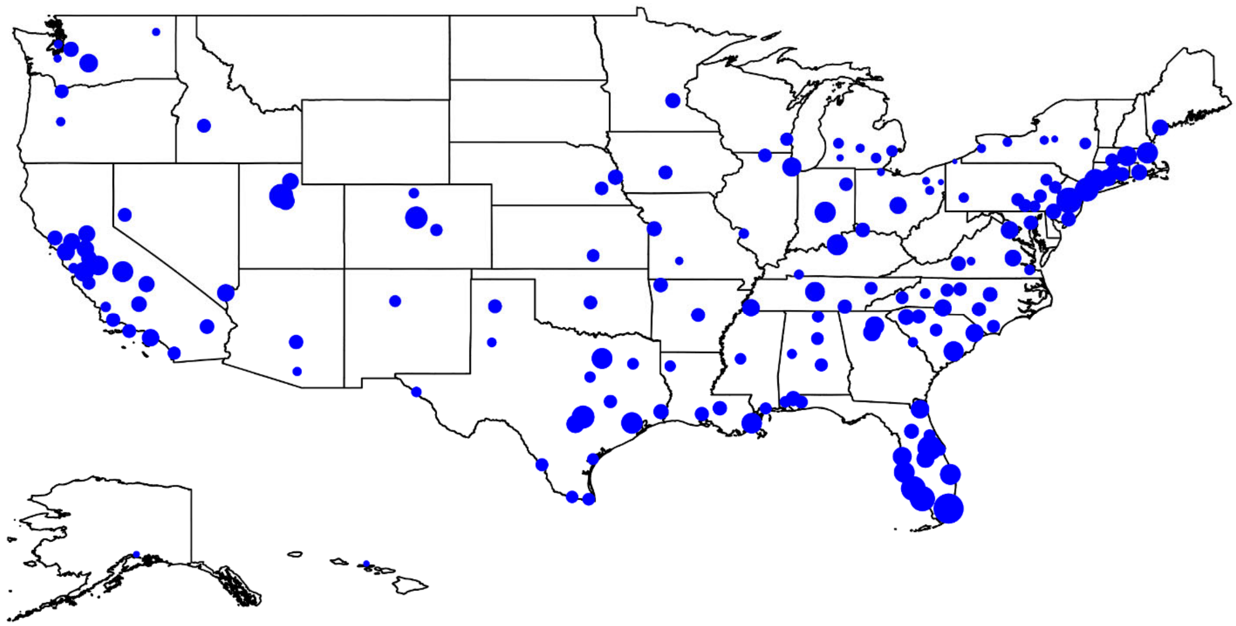
Online appendix

Figure A1: Distribution of CBP encounters by country, 2014-2019 and 2021-2024



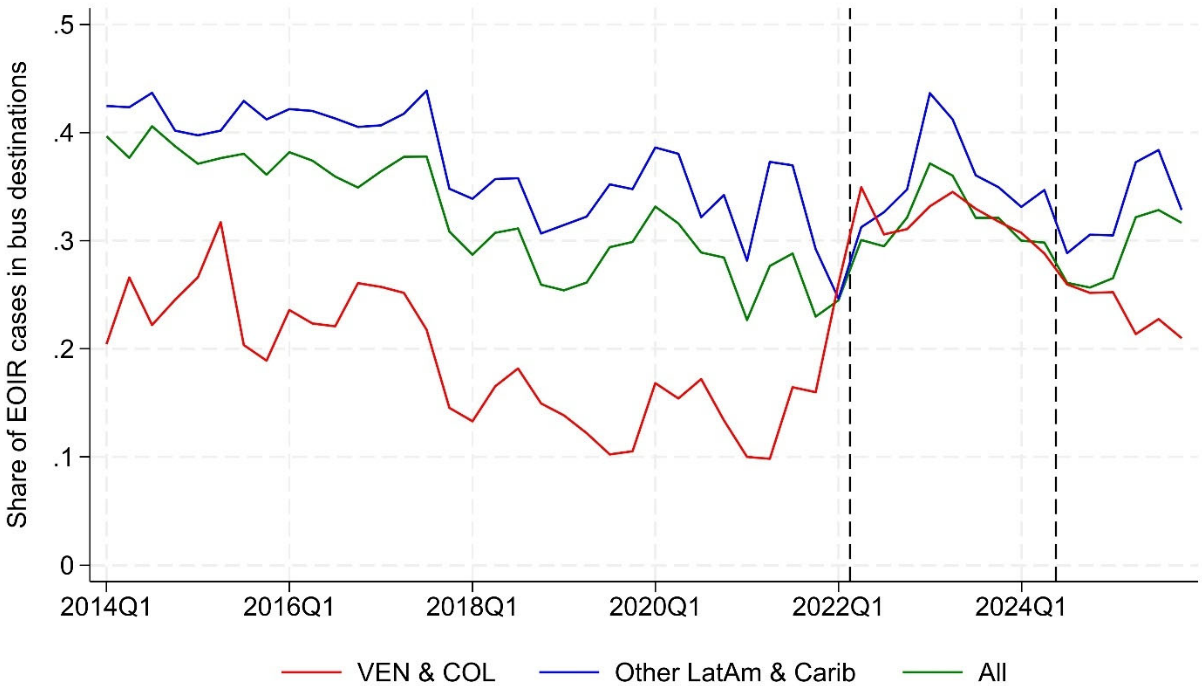
Note: Authors' calculations using data from the U.S. Department of Homeland Security, Customs and Border Protection.

Figure A2: Distribution of surge migrants across major metro areas, relative to population



The ranking of the metro areas changes considerably from Figure 5 if areas are ranked by EOIR cases relative to the total metro population instead of the absolute number of EOIR cases. The metro area with the highest number of EOIR cases relative to its population is the Miami area, at over 48 cases per 1000 people, followed by Orlando, Salt Lake City, and New York City. There is also considerably less dispersion across the major metro areas in EOIR cases relative to the population than in the absolute number of cases.

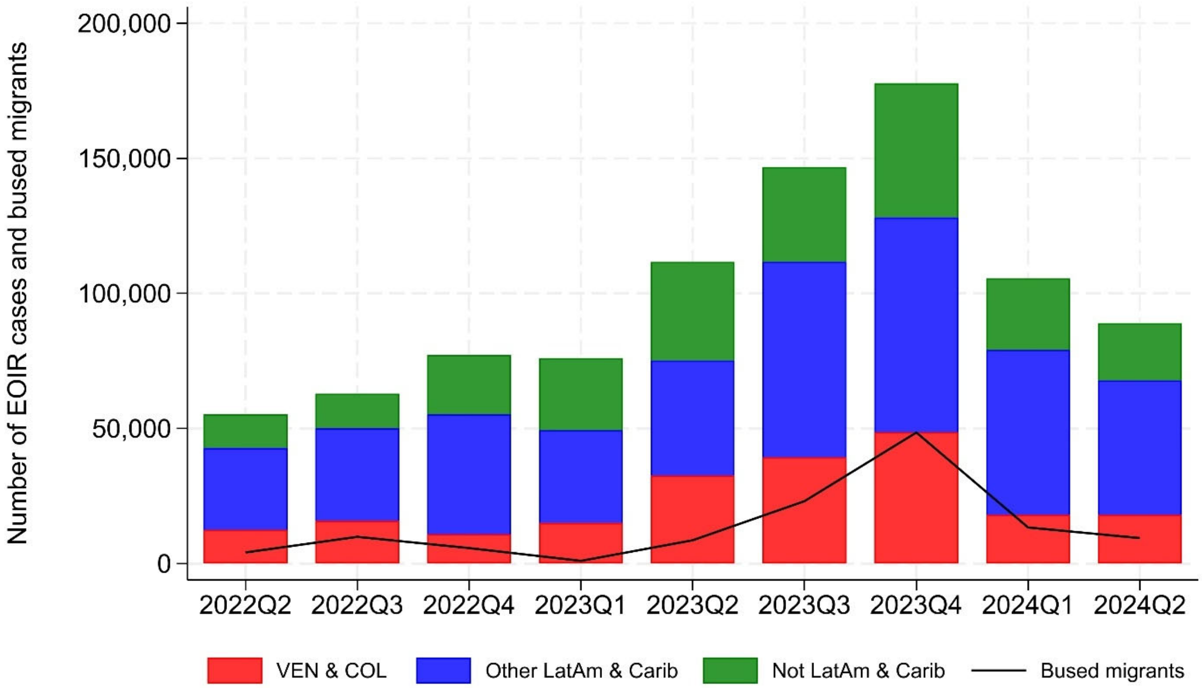
Figure A3: Share of surge migrants in buslift destination metro areas, 2014-2025



Source: Authors' calculations using data from the U.S. Department of Justice, Executive Office for Immigration Review, and the Texas and Arizona governors' offices. Only non-detained cases are included. The vertical lines indicate the beginning and end of the buslift (2022Q2 and 2024Q2, respectively).

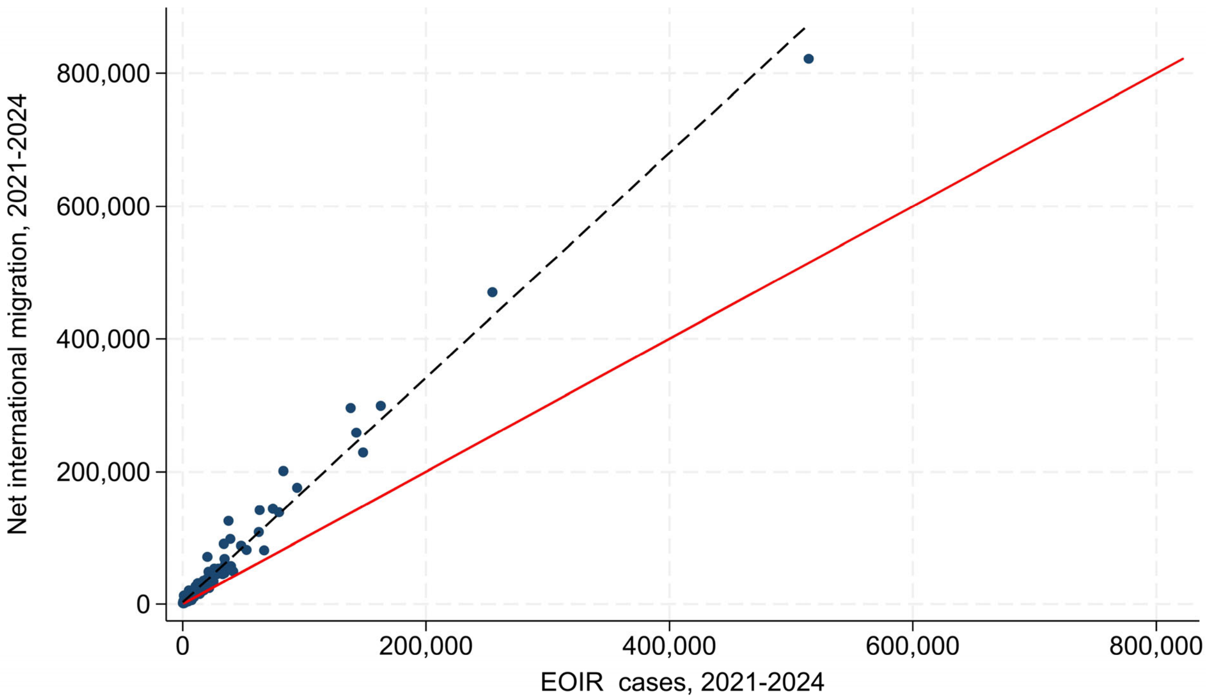
The share of EOIR cases among migrants from Venezuela and Colombia in the six buslift destinations (Chicago, Denver, Los Angeles, New York, Philadelphia, and Washington, DC) rose considerably when the buslift began. The share of EOIR cases in those six destinations for migrants from other Latin American and Caribbean countries as well as overall rose too, but those increases are smaller.

Figure A4: Number of EOIR cases and bused migrants in bus destination metro areas, 2022Q2-2024Q2



Source: Authors' calculations using data from the U.S. Department of Justice, Executive Office for Immigration Review, and the Texas and Arizona governors' offices. Only non-detained cases in the six bus destination metro areas (Chicago, Denver, Los Angeles, New York, Philadelphia, and Washington, DC) are included.

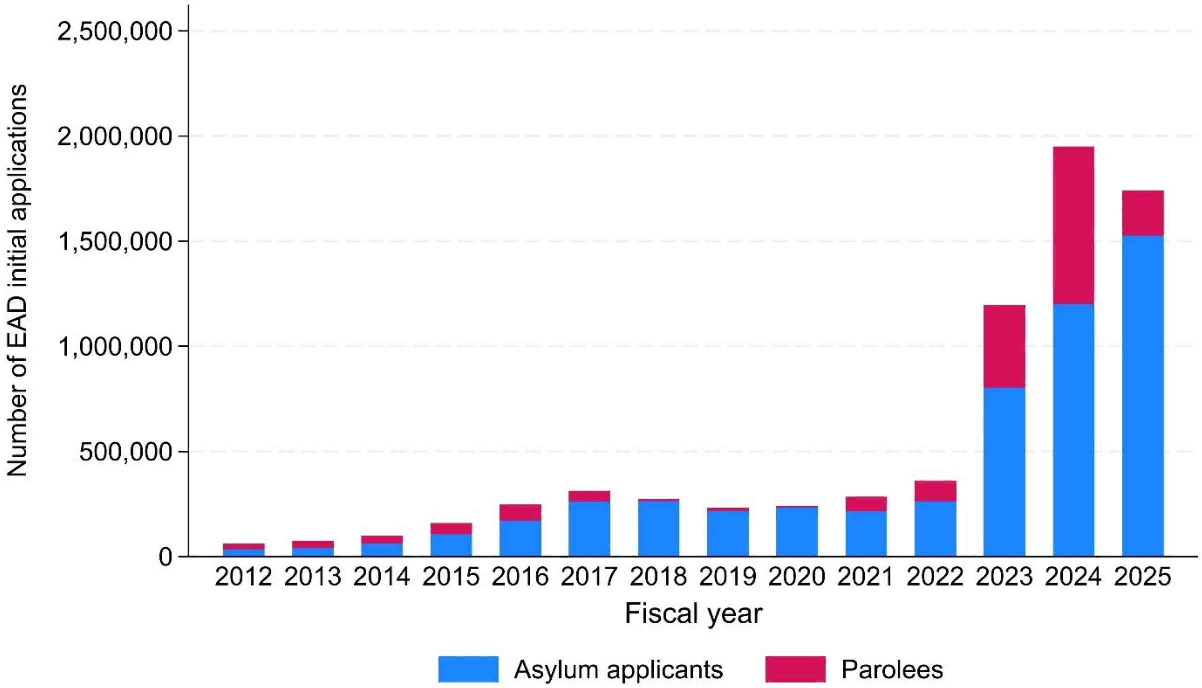
Figure A5: Number of EOIR cases and Census Bureau estimate of net international migration for major metro areas, 2021-2024



Note: Authors' calculations using data from the U.S. Department of Justice, Executive Office for Immigration Review and 2025 vintage Census Bureau estimated components of resident population change. EOIR cases include non-detained migrants who entered in July 2021- June 2024 or, if an entry date is not recorded, received a notice to appear during that period. The net international migration estimates are the sum of 2022, 2023, and 2024 estimates. The red line is the 45° line. The dashed black line is the linear regression line. It has a slope of 1.69 (0.07) and an R-squared of 0.98.

The two top points in Figure A5 are the New York City and Miami areas.

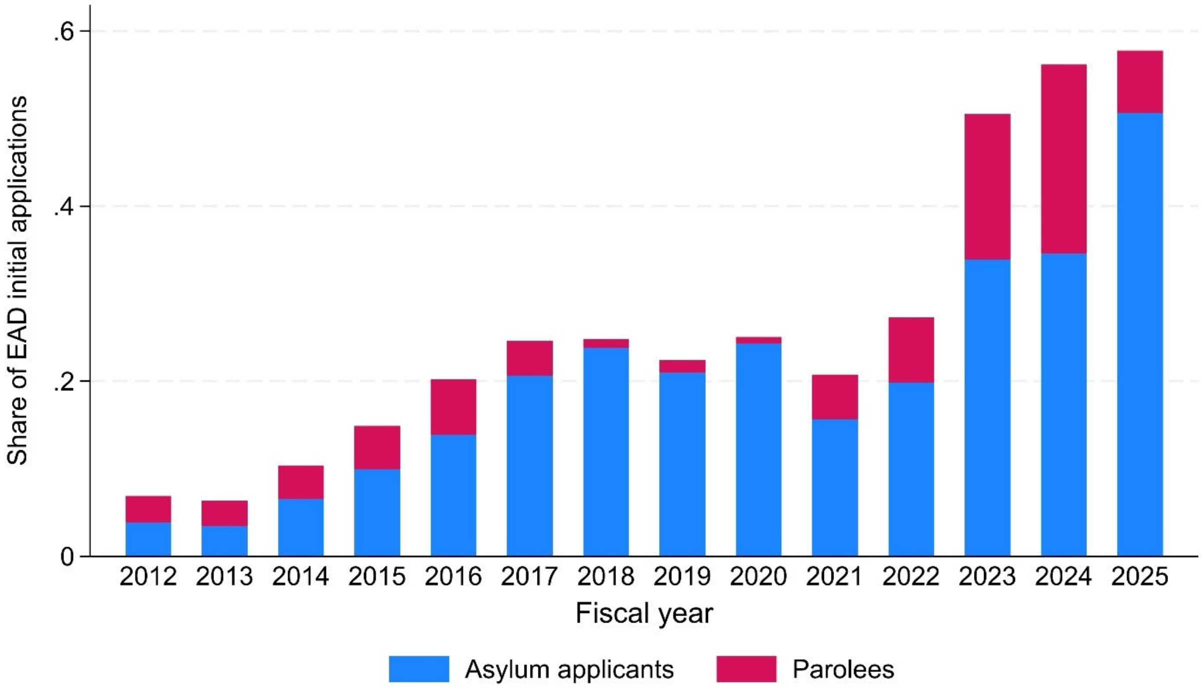
Figure A6: EAD applications by asylum applicants and parolees, FY 2012-2025



Source: Authors' calculations using data from the U.S. Department of Homeland Security, U.S. Citizenship and Immigration Services.

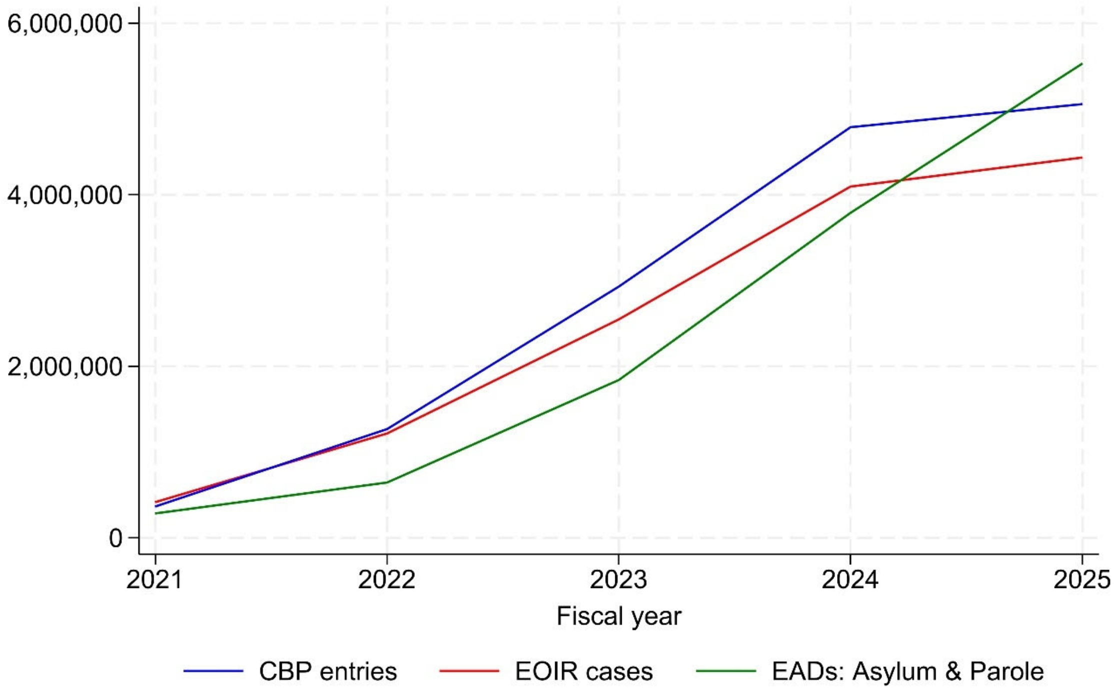
The number of initial applications for an Employment Authorization Document (EAD, or Form I-765) filed by applicants for asylum and migrants admitted to the United States with parole soared as a result of the surge. The share of EAD applications in those two categories rose as well (next figure). For more discussion of EADs, see Foote (2024).

Figure A7: Share of EAD applications by asylum applicants and parolees, FY 2012-2025



Source: Authors' calculations using data from the U.S. Department of Homeland Security, U.S. Citizenship and Immigration Services.

Figure A8: Cumulative number of surge migrants in CBP and EOIR data and EAD applications by asylum applicants and parolees, FY 2021-2025



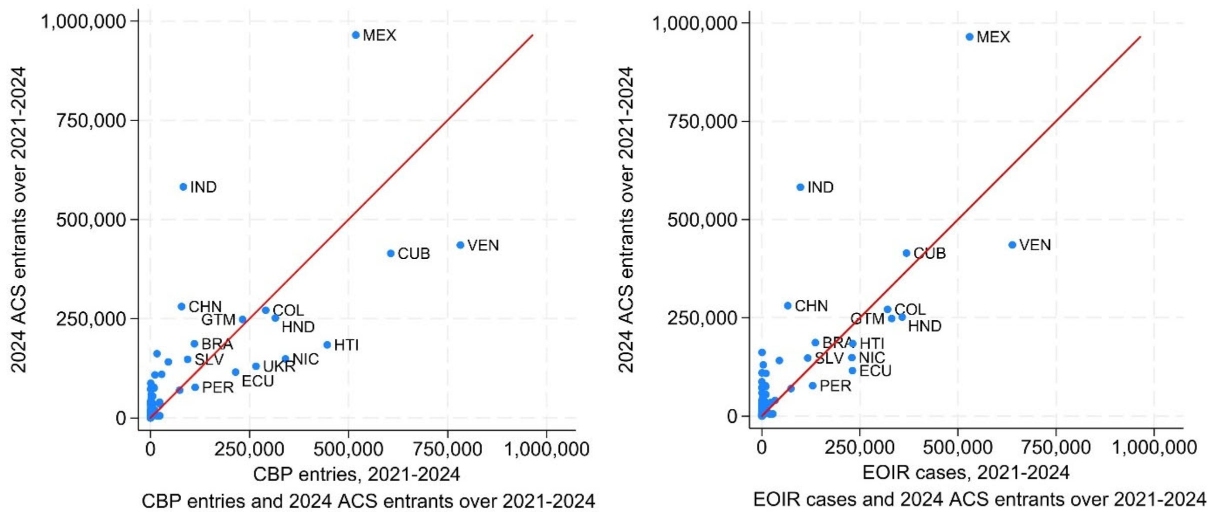
Source: Authors' calculations using data from the U.S. Department of Homeland Security, Customs and Border Protection and U.S. Citizenship and Immigration Services, and U.S. Department of Justice, Executive Office for Immigration Review

The number of migrants who applied for an employment authorization document (EAD, or Form I-765) because they applied for asylum or were admitted with parole closely tracks the total number of migrants without a valid visa released or granted entry by CBP and the number of EOIR cases during the surge. Asylum seekers were eligible to apply for an EAD once their asylum application had been pending for at least 150 days, and they could be approved for an EAD once their asylum application had been pending for at least 180 days. Given this lag in the EAD process, it makes sense that the EAD applications appear to lag the CBP and EOIR data.

At the end of fiscal year 2025, the cumulative number of EAD initial applications filed by asylum seekers and parolees exceeds the cumulative number of CBP entries and EOIR cases. How can this happen? One contributing factor is that the EAD data are the total number of EAD applications by asylum seekers and parolees, not the number of unique individuals. Some migrants may have first applied for an EAD as a parolee, and then applied again as an asylum applicant after the Biden administration announced in October 2024 that it would not extend parole status for many migrants who were paroled into the country or after Trump won the election in November 2024.

None of the three data series in Figure A8 is a perfect indicator of the increase in labor supply due to the surge, but they all point to a large increase in labor supply due to surge migrants.

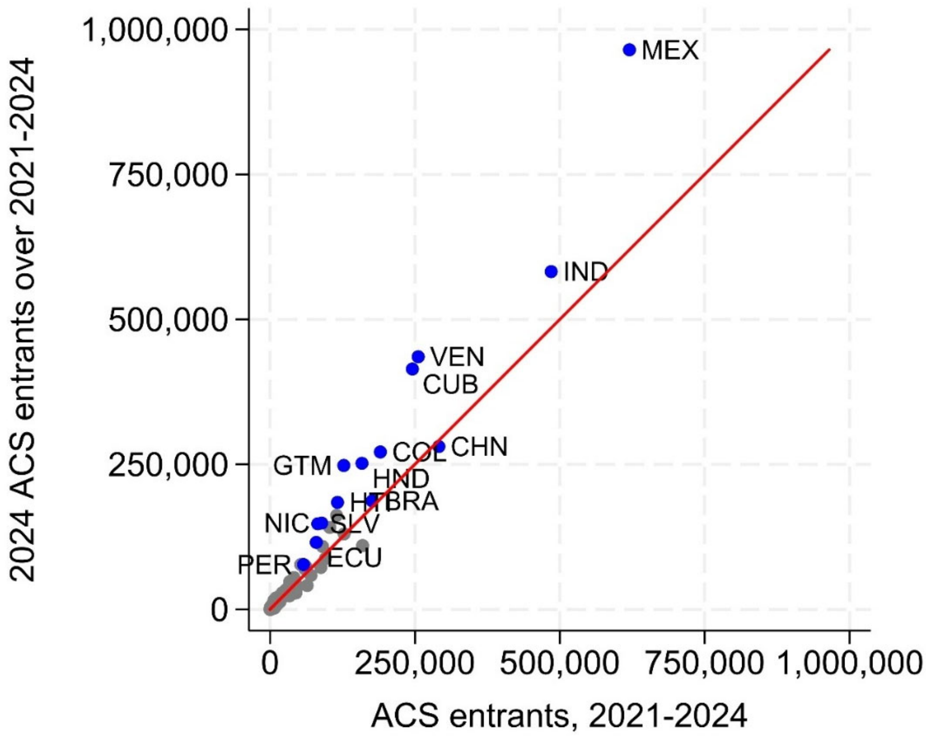
Figure A9: Comparison of number of surge migrants in CBP and EOIR data with number of 2021-2024 entrants in 2024 ACS, by country



Source: Authors' calculations using data from U.S. Department of Homeland Security, Customs and Border Protection on the number of migrants who were released or granted entry into the United States despite not having a visa during 2021-2024; data from the U.S. Department of Justice, Executive Office for Immigration Review on the number of non-detained cases involving migrants who entered the country during 2021-2024; and data from the 2024 American Community Survey (ACS) on non-U.S. citizens who reported arriving in the United States in 2021 or later. The person weights are used to create the ACS counts of entrants. The red line is the 45° line.

Figure A9 is the equivalent of Figure 7 but using only the 2024 ACS and including all non-citizens who reported entering in 2021 or later (versus being abroad a year ago in Figure 7). The main disadvantage of the 2024 ACS is that it will not capture any surge immigrants who have already left. In addition, the first ACS after surge migrants entered may better reflect where they initially went. The higher numbers in the 2024 ACS reflect the increase in the population estimates to better incorporate the surge but may also reflect better incorporation over time of surge migrants who arrived more than a year ago. It is not unheard of for unauthorized immigrant cohort sizes to increase over time in surveys even though this is mathematically impossible (see Van Hook et al. 2014).

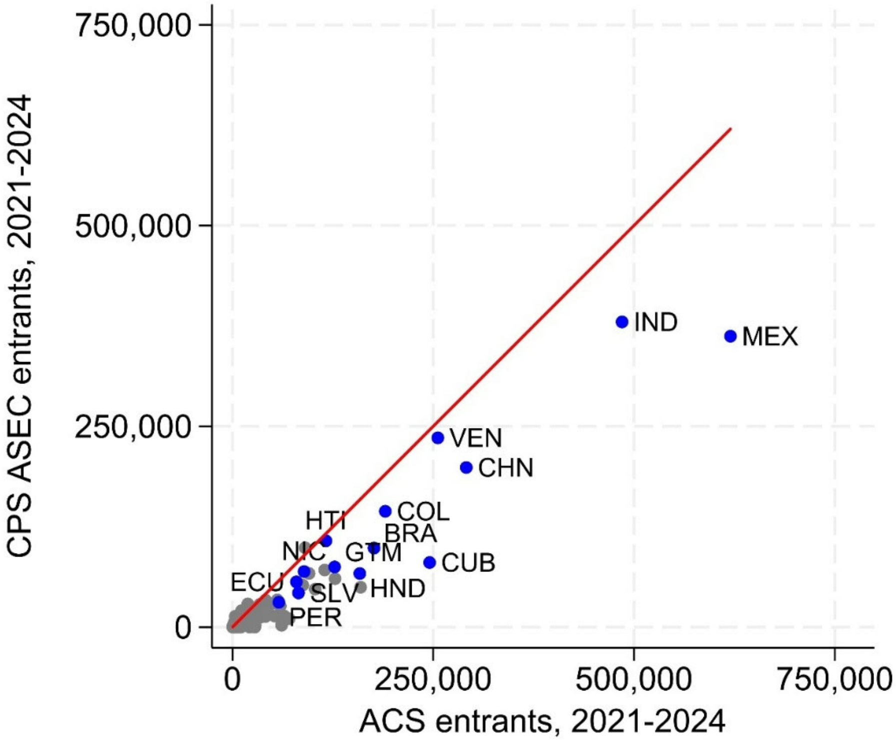
Figure A10: Comparison of number of new entrants in 2021-2024 ACS and number of 2021-2024 entrants in 2024 ACS, by country



Source: Authors' calculations using data from the 2021-2024 American Community Survey (ACS) on non-U.S. citizens whose residence one year ago was abroad and from the 2024 ACS on non-U.S. citizens who reported arriving in the United States in 2021 or later. The person weights are used to create the ACS counts of entrants. The red line is the 45° line. Blue dots are labeled points.

Figure A10 compares the number of non-citizens who were abroad a year ago in the 2021-2024 ACS and the number of non-citizens in the 2024 ACS who reported entering the country in 2021 or later. The 2024 ACS has higher numbers of surge migrants, as all top 12 surge countries are above the 45° line. The figure also calls out India and China since they were major sources of new immigrants – although not surge migrants – during 2021-2024. Our analysis of labor market and housing impacts does not use the ACS data for either of these periods – we rely on the EOIR and CBP data to measure surge migrants.

Figure A11: Comparison of number of new entrants in 2021-2024 ACS and the 2021-2024 CPS ASEC, by country



Source: Authors' calculations using data from the 2021-2024 American Community Survey (ACS) on non-U.S. citizens whose residence one year ago was abroad and from the same using data from the 2021-2024 Current Population Survey Annual Social and Economic Supplements (CPS ASEC). The ACS and CPS ASEC person weights are used to create the counts of entrants. The red line is the 45° line. Blue dots are labeled points.

Like the ACS, the CPS ASEC includes a question about place of residence one year ago. The basic monthly data from the CPS does not include a question about residence one year ago. Unlike the ACS, the CPS reports year of arrival in intervals, making it less useful for examining the surge.

Figure A11 compares the number of non-citizens who were abroad a year ago in the 2021-2024 ACS with a similar calculation for the CPS ASEC. The ACS has higher numbers of surge migrants, with all top 12 surge countries are below the 45° line, but it also has more entrants from most non-surge countries as well, including India and China (which were major sources of new immigrants – although not surge migrants – during 2021-2024).

Table A1: Descriptive statistics for ACS-constructed outcomes for U.S. natives, adjusted for gender, age, and education

	2021	2022	2023	2024
Log hourly wage	-2.8	-7.9	-4.9	-3.0
($2024, × 100)	(1.1)	(11.0)	(10.7)	(10.8)
Employment rate	-1.9	0.3	0.6	0.5
(percentage points)	(3.4)	(3.1)	(3.0)	(2.9)
Log gross rent (including utilities)	-30.1	-13.3	-3.2	-3.0
($2024, × 100)	(23.3)	(23.2)	(23.1)	(22.8)

Source: Authors' calculations.
Notes: Means of residuals from one regression per outcome pooled for 2021-2024 and weighted by ACS sample weights, and controlling for gender, gender × age, 3 education dummies, 4 age dummies, and education dummies × age. The samples are for natives: workers aged 16-64 in the first row; respondents aged 16-64 in the second row; all respondents in the third row. The residuals do not average to zero across the years because the yearly means of the residuals are unweighted. Standard deviations are in parentheses.

Table A2: Distribution of surge migrants in CBP data across country groups

Country group	*Number allowed to enter, July 2021-June 2024*	*Share*
Venezuela	678,228	15.5%
Cuba	542,083	12.4%
Mexico	451,268	10.3%
Haiti	397,431	9.1%
Nicaragua	325,653	7.4%
Colombia	267,392	6.1%
Honduras	241,547	5.5%
Ukraine	233,759	5.3%
Guatemala	200,961	4.6%
Ecuador	196,806	4.5%
Other Europe, Australia, New Zealand	130,486	3.0%
Peru	109,515	2.5%
Africa	97,535	2.2%
Brazil	95,926	2.2%
El Salvador	73,871	1.7%
India	71,440	1.6%
China	65,108	1.5%
Other Asia	60,133	1.4%
Other Americas	52,166	1.2%
Dominican Republic	43,403	1.0%
Canada	21,506	0.5%
Philippines	13,600	0.3%
Vietnam	6,593	0.2%
South Korea	343	0.0%
Germany	274	0.0%
Total	4,377,027	100%

Source: Authors' calculations using data from the U.S. Department of Homeland Security, Customs and Border Protection.

Country groupings used to create the shift-share instrument and the number of surge migrants that CBP released or granted entry into the United States between July 2021 and June 2024. We classify USBP encounters between ports of entry with a disposition of NTA, P, REL, or I as released into the United States, and CBP Office of Field Operations encounters with inadmissible migrants at ports of entry with a disposition of NTA, NTA/WA, NTA-Person Released, or any form of parole as granted entry.

Table A3: Rotemberg weights for first stage of shift-share IV wage regressions not scaled by population (%)

Country group	*2021-2024*	*2021-2022*	*2022-2023*	*2023-2024*
Canada	0.0	0.0	0.0	0.0
Mexico	4.5	0.4	1.7	12.3
Other Americas	0.1	0.3	0.3	0.2
Germany	0.0	0.0	0.0	0.0
Other Europe, Australia, New Zealand	0.3	0.0	2.5	0.3
China	0.2	1.1	0.1	0.7
Korea	0.0	0.0	0.0	0.0
Philippines	0.0	0.0	0.0	0.0
Vietnam	0.0	0.0	0.0	0.0
India	0.4	0.0	0.3	0.4
Other Asia	0.1	0.0	0.0	0.1
Africa	0.5	0.0	0.2	0.8
El Salvador	0.3	0.5	0.1	0.3
Guatemala	2.1	1.0	0.1	4.4
Honduras	3.5	6.2	1.0	3.4
Nicaragua	6.2	14.7	10.8	1.6
Cuba	20.7	41.4	25.3	11.7
Dominican Republic	0.3	0.0	0.7	0.2
Haiti	15.1	2.6	11.5	20.9
Brazil	0.5	2.4	0.2	0.2
Colombia	6.4	5.1	7.5	4.3
Ecuador	5.9	1.4	3.4	8.1
Peru	1.0	0.4	2.5	0.4
Venezuela	27.8	20.0	23.5	28.2
Ukraine	4.0	2.4	8.4	1.3
Total	100.0	100.0	100.0	100.0

Source: Authors' calculations.

Notes: The first stage outcome is the number of surge migrants while controls in addition to the number of surge migrants predicted with the shift share are the 2016-2019 and 2019-2021 changes in log native wages.

Table A4: Rotemberg weights for first stage of shift-share IV wage regressions scaled by population (%)

Country group	*2021-2024*	*2021-2022*	*2022-2023*	*2023-2024*
Canada	0.0	0.0	0.0	0.0
Mexico	3.2	-0.1	0.3	9.2
Other Americas	0.0	0.1	0.1	0.1
Germany	0.0	0.0	0.0	0.0
Other Europe, Australia, New Zealand	0.1	0.4	0.9	0.1
China	0.0	0.0	0.0	0.1
Korea	0.0	-0.0	0.0	0.0
Philippines	0.0	-0.0	0.0	-0.0
Vietnam	0.0	0.0	0.0	-0.0
India	0.1	0.0	0.1	0.2
Other Asia	0.0	0.0	0.0	0.0
Africa	0.2	0.0	0.1	0.3
El Salvador	0.1	0.2	0.0	0.2
Guatemala	2.0	0.8	0.1	4.4
Honduras	2.4	5.1	0.7	2.5
Nicaragua	5.8	13.4	11.5	1.6
Cuba	29.8	48.1	36.0	17.6
Dominican Republic	0.1	0.0	0.2	0.0
Haiti	15.8	2.8	11.8	22.8
Brazil	0.5	2.7	0.2	0.2
Colombia	4.3	3.8	5.1	3.0
Ecuador	1.9	0.5	1.1	2.8
Peru	0.5	0.2	1.3	0.2
Venezuela	32.8	21.7	28.8	34.7
Ukraine	0.2	0.2	1.5	0.0
Total	100.0	100.0	100.0	100.0

Notes: The first stage outcome is the number of surge migrants/population while controls in addition to the number of surge migrants predicted with the shift share/population are the 2016-2019 and 2019-2021 changes in log native wages.

Table A5: Weighted results (using 2019 population), IV with full controls

	2021–2024 (1)	2021–2022 (2)	2022–2023 (3)	2023–2024 (4)	2023–2024 (5)	2023–2024 (6)
A. Aggregate variables:						
Total employment	0.6***	0.2*	1.2***	0.5***	0.7	0.0
	(0.1)	(0.1)	(0.2)	(0.1)	(0.6)	(0.2)
						[p=0.98]
GDP	1.1*	2.3***	1.1	-0.2	-0.1	-1.0
	(0.5)	(0.5)	(1.1)	(0.2)	(0.3)	(0.7)
						[p=0.42]
Average wage	-1.5*	-0.6	-2.5***	-1.6***	-2.1***	0.0
	(0.6)	(1.0)	(0.5)	(0.3)	(0.6)	(2.2)
						[p=1.00]
Multifamily rent	2.7***	7.5***	0.2	-0.7	0.7	4.3
	(0.6)	(1.1)	(0.7)	(0.7)	(1.3)	(4.0)
						[p=0.39]
B. ACS-constructed and domestic migration variables:						
Native wage	0.8*	1.9***	1.1	-0.6*	-0.4	-2.3**
	(0.5)	(0.5)	(0.7)	(0.3)	(0.5)	(1.1)
						[p=0.06]
Native employment rate	0.1	0.2	-0.1	-0.0	0.1	-1.0**
	(0.0)	(0.1)	(0.2)	(0.2)	(0.2)	(0.5)
						[p=0.15]
Native rent	1.4	0.5	4.2**	0.3	0.6	0.0
	(1.0)	(1.1)	(1.9)	(0.7)	(0.7)	(1.1)
						[p=0.97]
Net domestic in-migration	-0.7***	-0.2**	-0.7**	-1.2***	-1.3***	0.2
	(0.2)	(0.1)	(0.3)	(0.4)	(0.4)	(0.1)
						[p=0.15]
Observations	164	164	164	164	30	30
Instrument	Shift share	Shift share	Shift share	Shift share	Shift share	Buslift

Source: Authors' calculations.

Notes: Coefficients are from the "IV, Full controls" specification only, collected by outcome, reweighted by 2019 population instead of unweighted. Coefficients are ×100 except for total employment, native employment rate, and net domestic in-migration. Standard errors in parentheses below. *** p<0.01, ** p<0.05, * p<0.10. For column 6 (buslift instrument), the bracketed value below the standard error is the p-value from a wild cluster bootstrap-t test (boottest, Webb weights, 999 replications, null imposed).

Table A6: Unadjusted outcomes, IV with full controls

	2021–2024	2021–2022	2022–2023	2023–2024	2023–2024	2023–2024
	(1)	(2)	(3)	(4)	(5)	(6)
Native wage	1.1***	2.8***	1.3	-0.7	0.0	-0.7
	(0.3)	(0.7)	(0.9)	(0.6)	(0.5)	(0.8)
						[p=0.47]
Native employment rate	-0.0	0.4	-0.2	-0.4	0.1	-0.3
	(0.1)	(0.3)	(0.4)	(0.5)	(0.2)	(0.3)
						[p=0.40]
Native rent	1.9***	2.7*	5.3***	-1.5	1.1	-2.8*
	(0.5)	(1.6)	(1.7)	(1.7)	(1.0)	(1.7)
						[p=0.57]
Observations	164	164	164	164	30	30
Instrument	Shift share	Shift share	Shift share	Shift share	Shift share	Buslift

Source: Authors' calculations.

Notes: Coefficients are from the "IV, Full controls" specification only (the middle row of the corresponding unadjusted table in the earlier, longer version of this appendix), collected by outcome. Wage is dlnwageh / d3lnwageh (unadjusted log wage change, native workers — not residualized for age, education, or gender); mirrors Table 1's layout. Employment rate is dnempl / d3nempl (unadjusted native employment rate change, raw units — not ×100 scaled, not residualized at the micro level); mirrors Table 3's layout. Rent is dlnrentgrs / d3lnrentgrs (unadjusted log real rent change); mirrors Table 5's layout. Coefficients ×100 (log wage / log rent) except employment rate, which is in raw units — not ×100 scaled. Standard errors in parentheses below. *** $p<0.01$, ** $p<0.05$, * $p<0.10$. The Wage and Rent column 1 figures use the unlabeled full-sample no-covariate regression in the log where applicable, since the labeled regression at that position was restricted to the top-30 sample — the same labeling issue found and corrected in Table 1 previously. For column 6 (buslift instrument), the bracketed value below the standard error is the p-value from a wild cluster bootstrap-t test (boottest, Webb weights, 999 replications, null imposed).

Table A7: Reduced-form results

	2021-2024 (1)	2021-2022 (2)	2022-2023 (3)	2023-2024 (4)	2023-2024 (5)	2023-2024 (6)
A. Aggregate variables:						
Total employment: number of surge migrants	0.10* (0.04)	0.10 (0.06)	0.14** (0.05)	0.07* (0.03)	0.05 (0.04)	-0.10 (0.48) [p=0.88]
Total employment: buslift metro indicator	--	--	--	--	--	-13.93 (8.20) [p=0.09]
GDP	0.47* (0.18)	1.55** (0.49)	0.45* (0.22)	0.02 (0.13)	-0.01 (0.06)	-2.05*** (0.22) [p=0.67]
Average wage	-0.53 (0.33)	-0.87 (0.89)	-0.58 (0.33)	-0.30 (0.22)	-0.40* (0.16)	2.57 (2.95) [p=0.86]
Multifamily rent	0.43*** (0.12)	3.32*** (0.91)	-0.17 (0.14)	-0.40 (0.25)	0.04 (0.25)	1.71 (3.32) [p=0.63]
B. ACS-constructed and domestic migration variables:						
Native wage	0.3*** (0.1)	1.2** (0.5)	0.3 (0.2)	-0.1 (0.2)	-0.1 (0.1)	-3.3*** (0.8) [p=0.09]
Native employment rate	0.0 (0.0)	0.2 (0.2)	-0.0 (0.1)	-0.1 (0.1)	0.0 (0.0)	-0.9** (0.3) [p=0.20]
Native rent	0.5*** (0.2)	1.5* (0.9)	1.4* (0.7)	-0.7 (0.7)	0.0 (0.2)	-0.9 (1.6) [p=0.61]
Net domestic in-migration	-0.2*** (0.1)	-0.3** (0.1)	-0.2*** (0.1)	-0.3*** (0.1)	-0.2*** (0.0)	0.1 (0.2) [p=0.60]
Observations	164	164	164	164	30	30
Measure of surge migration	Shift share	Shift share	Shift share	Shift share	Shift share	Buslift

Source: Authors' calculations.

Note: Unweighted OLS coefficients on surge migrant flows as measured by the shift-share variable using CBP entries (columns 1-5) or buslift migrants (column 6). The outcome is the change in the variable indicated, in levels for total employment, native employment rate, and net domestic in-migration, and in logs for all other outcomes. Within each column, the pair of results in rows 1-2 are from the same regression. In row 1, the measures of surge migration are in levels; in row 2, the coefficient and SE on the buslift dummy are divided by 10,000; in all other rows, the measures of surge migration are relative to the initial population. Coefficients ×100 except total employment, native employment rate, and net migration. Robust standard errors in parentheses, with significance levels *** p < 0.01, ** p < 0.05, * p < 0.1. In column 6, the p-value based on the Webb wild bootstrap is in brackets. The regressions in rows 1-2 also control for the 2019 population, the 2016-2019 and 2019-2021 changes in employment, and the 2016-2019 share of employment in each supersector (excluding government). The regressions in all other rows control for the change in the (log of the) variable indicated over 2016-2019 and 2019-2021.

Table A8: IV results without Ukraine, Cuba, or Haiti in the shift-share instrument

	2021-2024 (1)	2021-2022 (2)	2022-2023 (3)	2023-2024 (4)
Total employment	0.52***	0.25	0.84***	0.56**
	(0.14)	(0.15)	(0.16)	(0.19)
GDP	1.30**	2.38**	1.58	0.266
	(0.45)	(0.82)	(0.85)	(0.56)
Average wage	-1.67**	-2.10	-2.31*	-0.62
	(0.64)	(1.52)	(1.00)	(0.90)
Multifamily rent	1.45**	6.54***	-1.02	-1.90*
	(0.46)	(1.02)	(0.76)	(0.87)
Native wage	0.7**	1.6**	1.1	-0.8**
	(0.3)	(0.7)	(0.7)	(0.4)
Native employment rate	0.0	0.2	0.1	-0.6
	(0.2)	(0.4)	(0.4)	(0.4)
Native rent	0.9	2.3*	3.7*	-2.5
	(1.0)	(1.2)	(2.0)	(1.8)
Net domestic in- migration	-0.8***	-0.7**	-0.8***	-1.0***
	(0.2)	(0.3)	(0.2)	(0.2)
Observations	164	164	164	164

Source: Authors' calculations.

Notes: Coefficients are from the same specification as the "IV, Full controls" row (row 2) of tables 3 (total employment), 4 (GDP), 5 (average wage), 12 (multifamily rent), 6 (native wage), 8 (native employment rate), 10, (net domestic in-migration), and 11 (native rent) except the shift-share instrument excludes Ukraine, Cuba, and Haiti. Columns 5 (top 30 for 2023-2024) and 6 (buslift IV) are omitted since this is a specification check on the shift-share instrument. Coefficients ×100 except total employment, native employment rate, and net migration. Standard errors in parentheses below. *** p<0.01, ** p<0.05, * p<0.10.

Table A9: Log immigrant wages (adjusted)

	2021–2024 (1)	2021–2022 (2)	2022–2023 (3)	2023–2024 (4)	2023–2024 (5)	2023–2024 (6)
OLS	0.6	-3.3	6.2*	-1.5	-1.1	—
Full controls	(1.0)	(2.8)	(3.5)	(2.3)	(2.1)	
IV	0.2	-1.6	3.4	-0.4	-2.0	-2.5
Full controls	(0.7)	(2.5)	(2.7)	(2.7)	(1.6)	(1.5)
						[p=0.62]
IV	0.1	-2.0	4.1	-1.1	-2.1	-2.2
No other controls	(0.8)	(2.5)	(2.7)	(2.8)	(1.5)	(1.7)
						[p=0.66]
Observations	164	164	164	164	30	30
Instrument	Shift share	Shift share	Shift share	Shift share	Shift share	Buslift

Source: Authors' calculations.

Notes: Outcome is dr_lnswageh / d3r_lnswageh (log wage change, immigrant workers). Mirrors Table 1's layout for comparison. Coefficients ×100, standard errors in parentheses below. *** $p<0.01$, ** $p<0.05$, * $p<0.10$. The OLS coefficient in column 6 (Bus instrument) is identical to column 5 (Top-30 sample) and is shown as —. The IV, no other controls, column 1 figure uses the unlabeled full-sample no-covariate regression in the log, since the labeled regression at that position was restricted to the top-30 sample — the same labeling issue found and corrected in Table 1. For column 6 (buslift instrument), the bracketed value below the standard error is the p-value from a wild cluster bootstrap-t test (boottest, Webb weights, 999 replications, null imposed).

Appendix-Only References

Van Hook J, Bean FD, Bachmeier JD, Tucker C. 2014. "Recent trends in coverage of the Mexican-born population of the United States: results from applying multiple methods across time." *Demography* 51(2): 699-726. https://doi.org/10.1007/s13524-014-0280-2.