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

Jennifer Hunt†, Pia Orrenius‡ and Madeline Zavodny§

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Abstract

In the wake of the pandemic, the United States experienced an unprecedented increase in migration, with many migrants either entering undetected or outside of usual legal pathways, often to pursue an asylum claim or via humanitarian parole, or overstaying a visa. The surge in those groups totaled over 6.5 million but was undercounted in household surveys. Hence, we use immigration court data to estimate surge migration to U.S. metros and quantify its impact. We find that over the 2021-2024 period, the surge boosted employment by 0.4 jobs for each surge migrant and raised average metropolitan area GDP by about 1.8%. Although the surge may have reduced average wages due in part to compositional effects, it raised U.S. natives' wages 0.9-1.1% over that period. The surge left natives' employment rates unaffected, but increased natives' rent by 1.3%. We calculate that native renters' wage net of rent rose 2.3%. There was little or no offsetting response of net domestic in-migration. The year-over-year responsiveness of natives' wages and rents declined over 2021-2024, which could reflect the economy adjusting to the ongoing inflows or the cooling of the economy after the post-pandemic recovery.

Keywords: immigration, migration, asylum seekers, buslift, labor markets, housing markets

JEL Classification: J15, J21, J61, F22, R31

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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. Over 2021-2024, there was an increase of at least 6.5 million in the stock of migrants who had entered undetected, overstayed a visa, or been allowed to enter outside of usual legal pathways, often to pursue an asylum claim or via humanitarian parole (Congressional Budget Office 2026, Edelberg et al. 2026). Many of the roughly 5.5 million migrants in the third category 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 added at least 2% to the 2021 population, and its effects on macroeconomic aggregates were sizeable and significant, although hard to initially detect given the U.S. 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 increased 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 the surge in recorded entries affected labor and housing markets in major metro areas across the United States in 2021-2024. Our contributions relative to two earlier working papers on the same topic (Wilson and Zhou 2026, followed by Clemens et al. 2026) are the use of migrant busing initiatives conducted by Texas and Arizona as a source of exogenous variation in migration destinations; a careful analysis of the timing and evolution of migration's impacts over 2021-2024; and a study of domestic migration as an adjustment mechanism. The two earlier papers examine a more extensive set of outcomes than we do.[2]

[1] The exception was CBO (2024), who incorporated Department of Homeland Security (DHS) data on border encounters, asylum seekers, parolees, and entries without inspection in developing their estimates of 2023 net immigration.

[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).

In addition to using the busing instrument, we follow the lead of Wilson and Zhou (2026) and use a shift-share instrument. The two instruments should provide us with two different local average treatment effects (LATE) of the surge impact: the impact of surge migration caused by the "buslift," which is mostly 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 migrant networks and 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) and the Department of Homeland Security's Customs and Border Protection (CBP) agency.[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 jobs over 2021-2024, with the greatest increases in leisure and hospitality and professional and business services, as measured in shares. The additional workers and consumers increased GDP by 1.6% for each one percentage point increase in surge migrants as a share of the population, implying an increase in GDP per capita. 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 1.8%. A weakly statistically significant estimate indicates that a one percentage point increase in the surge migrant share may have reduced average hourly wages by about 1.7%. This response is too large to be fully accounted for by compositional effects, and we cannot replicate the negative effect in the ACS.

The part of the decline in the average wage not explained by composition cannot be explained by a decline in U.S. natives' wages. On the contrary, natives' wages rose by 0.9-1.1% over 2021-2024 in response to the surge. Although a 2017 consensus view was that immigration has little or no effect on the wages of U.S.-born workers (National Academies 2017), our results

[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.

are consistent with those of Caiumi and Peri (2026), who find that immigration over 2000-2023 raised native wages by 0.6-0.7%.[5]

Offsetting the positive wage effects, metropolitan area rents measured either for all renters or U.S. natives rose 1.3-1.6%. We calculate that the average native renter's wage net of rent rose 2.3%, helped by a more positive effect on renters' wages than on all native wages. Meanwhile, native employment rates did not change as a result of the surge, consistent with the pre-surge U.S. literature.[6]

The impact of the surge migration to particular metros was not moderated by an offsetting response of domestic migration. An apparently strong offset based on the shift-share instrument simply reflects the fact that surge migrants are predicted to locate in metros experiencing a long-term decline in net domestic in-migration. Although some studies of earlier periods find little internal offset of international migration to the United States (e.g., Peri and Zaiour 2023), those studies typically examine longer-run effects of inter-state migration, while Wozniak and Murray (2012), using inter-metro migration, show longer-run analysis obscures a short-run offset of about 50%.[7]

Our coefficients are generally similar to those from comparable analysis in Wilson and Zhou (2026) and Clemens et al. (2026), except for the much larger positive impact on GDP found by Clemens et al. (20026) and the negative, marginally statistically significant effect on native wages found by Wilson and Zhou (2026). However, Wilson and Zhou (2026) emphasize the impacts of migrants who are likely to be workers rather than the impact of all migrants, leading to their main finding of a one-for-one increase in employment. Clemens et al. (2026) emphasize results for the 2021-2023 period rather than 2021-2024, leading to their main finding of an increase in the native employment rate; we find no effect for that period. Wilson and Zhou

[5] Their paper is identified by comparing workers in different cells defined by workers' combination of skills. Other papers finding positive effects of immigrants on natives examine particular immigrant and native types, e.g., Mahajan et al. (2025) find that H-1B workers may raise the wages of their colleagues.

[6] See Cabral and Steingress (2026), National Academies (2017), and Saiz (2007). Howard et al. (2025) find that facilitating deportation raises the price of newly constructed (but not existing) units due to a decline in labor supply. 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).

[7] Amior (2025) finds that in response to a demand shock, international immigration influenced by ethnic enclaves crowds out the net migration response within the United States. Haslag and Weagley (2023) examine changing reasons for U.S. internal migration over 2017-2021.

(2026) multiply their coefficients by the mean change in the surge migration share to find the overall effect, as we do, but multiply by a much larger number, leading to larger overall effects.[8]

Our examination of year-to-year changes reveals the positive effects on GDP and U.S. natives' wages and rents were higher in 2021-2023 than in 2023-2024, when the effects fell to zero or even reversed (based on the buslift instrument), thus pulling down the magnitude of the longer-run effects. This could reflect the economy adjusting to the ongoing inflows or the cooling of the economy after the post-pandemic recovery. 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 necessarily mean that there were no impacts of the late stages of the surge, despite the lack of offsetting domestic migration, since other adjustments such as capital or trade flows could spread the impacts over a wider geography, preventing their detection via a comparison of 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. The post-pandemic recovery may also have been conducive to early wage increases. 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 could indicate that migrants whose location choice was influenced by the bus destination had more negative effects on natives than surge migrants more influenced by their networks. However, due to the influence of Denver in the buslift estimation, they could equally reflect that Denver was less able to adjust to inflows than other metros.

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.[9] Much like those earlier waves, surge migration was due to a combination of the pull of a strong

[8] The Literature Review Appendix outlines the differences across the three papers.
[9] 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).

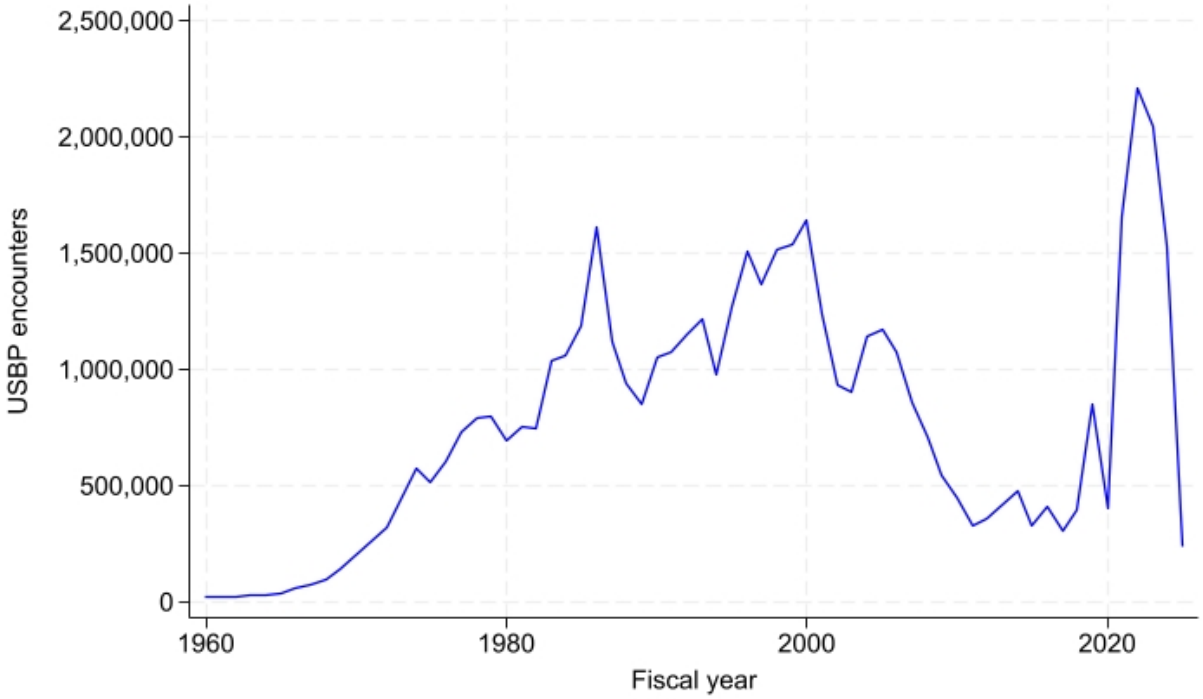
U.S. labor market, which attracted migrants as the economy rebounded from pandemic closures, and push factors in origin countries, including weak economic conditions, political instability, violence, and natural disasters. However, the surge differed notably from historical episodes by encompassing a 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.[10]

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.[11] 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.

[10] 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.

[11] 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 the late 2000s, migrants caught crossing the border illegally after their first time are more likely to be 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



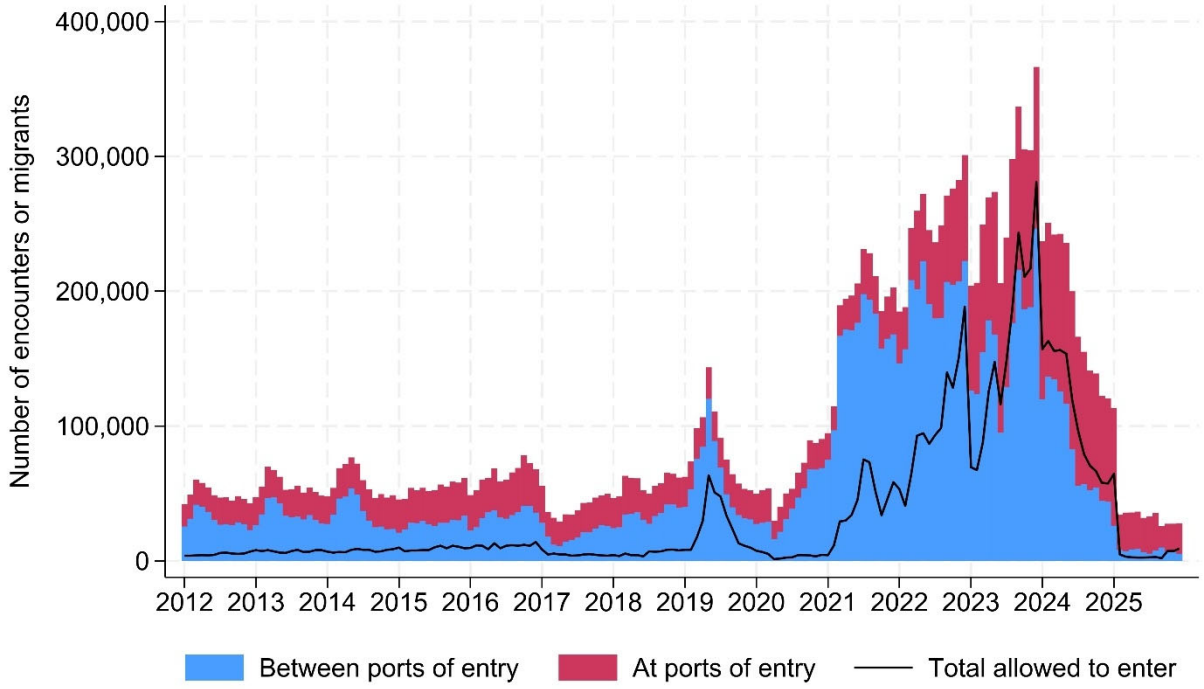
Source: U.S. Department of Homeland Security, Customs and Border Protection.

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.[12] 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 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 estimated number who were released or granted entry.[13] 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

[12] 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).
[13] Online appendix figure A1 adds an estimate of the number of migrants who were detained by USBP and then released to our estimate of how many migrants were allowed to enter. We discuss how including those migrants affects key estimates later in the paper.

encounters may reflect multiple interactions with the same people 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 entries without inspection during fiscal years 2021-2024.[14]

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,

[14] 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 entries without inspection.

Guatemala, and Honduras together accounted for almost three-quarters of migrants without a valid visa encountered by CBP between or at ports of entry (online appendix figure A2).[15] 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 A2).

I.A. Where Did the Surge Migrants Go?

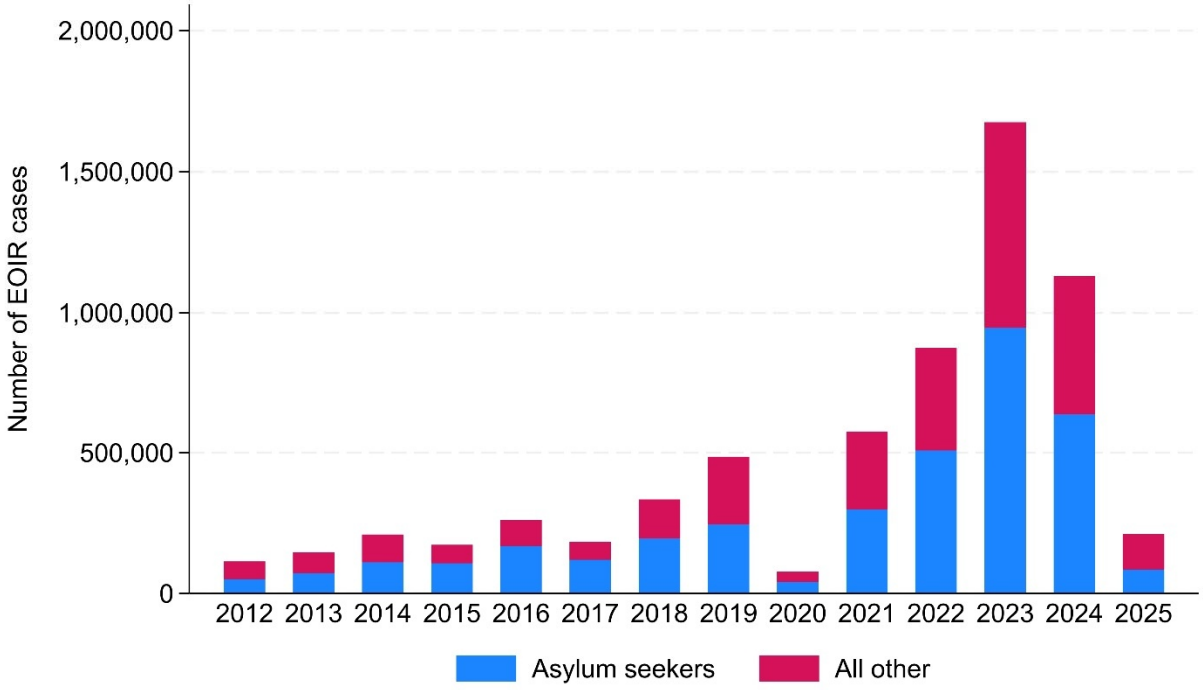
The CBP data give an idea of the size of the surge and migrants' origin countries, but those data do not indicate where migrants went within the United States. For that, we first turn to administrative data from the EOIR. Migrants who were given an NTA and then released or granted entry are included 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.[16] The data are further disaggregated by whether the migrant has filed an asylum application, according to the EOIR case record.[17] 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.

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

[16] Surge migrants typically received an NTA when they were released by U.S. Border Patrol or were granted entry into the country despite being inadmissible. The major exception is migrants who entered via humanitarian parole; as discussed later, these surge migrants are underrepresented in the EOIR data. 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.

[17] 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

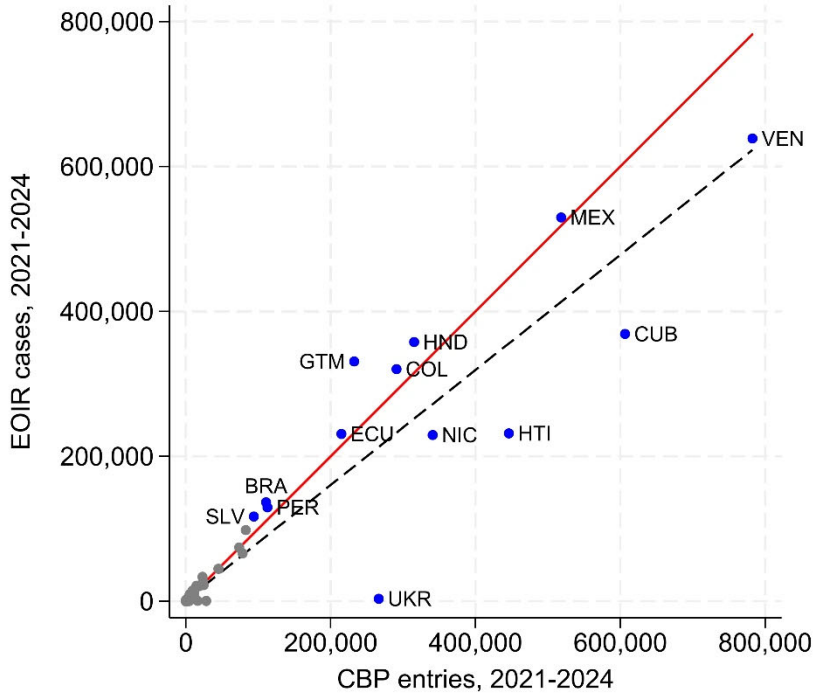


Source: Authors' calculations using data from the U.S. Department of Justice, Executive Office for Immigration Review. Only non-detained cases are included. Migrants are categorized by their year of entry or, when that is not recorded, year of their notice to appear.

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; those countries had a large number of surge migrants who entered via humanitarian parole and therefore were not issued an NTA and are less likely to appear in the EOIR data. The (dashed) regression line has a slope of 0.8, the factor by which the EOIR undercounts CBP entries.[18] 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.

[18] Online appendix figure A3 reproduces figure 4 with a broader estimate of migrants allowed to enter that includes USBP encounters with migrants who were detained and eventually released. The linear regression line in that figure has a slope of 0.73.

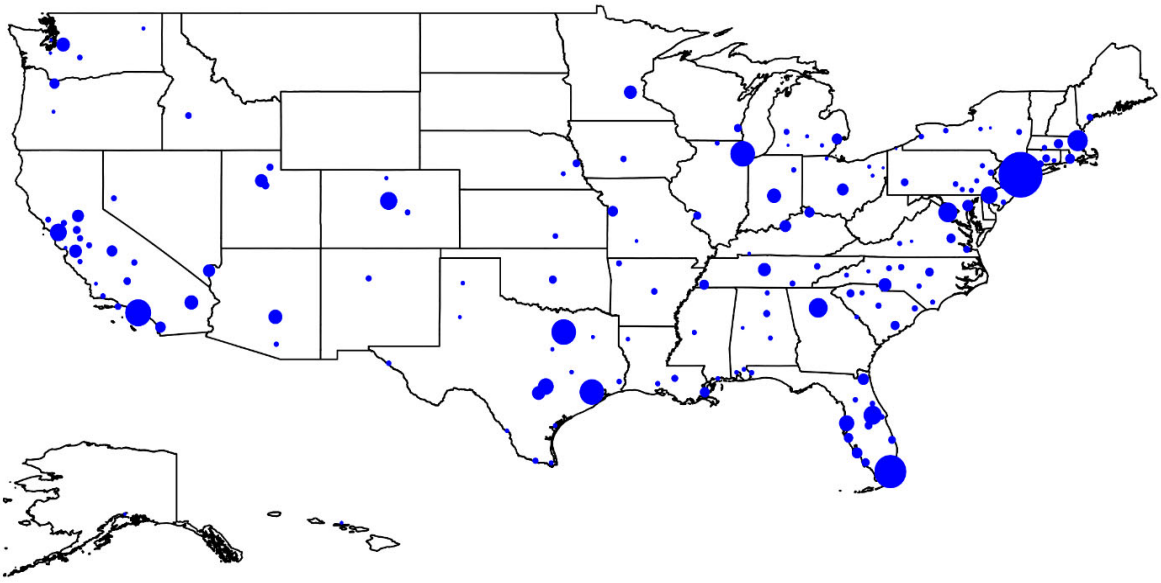
Figure 4: CBP entries and EOIR cases, 2021-2024, by country



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. 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 A4).

Although many of the surge migrants went to traditional migrant destinations, there was an unusual twist during the surge: the "buslift."[19] Texas and, to a lesser extent, Arizona organized high-profile busing of migrants from border towns far into the U.S. interior, beginning in April 2022 and ending in June 2024. The choice of destinations was politically motivated – they were large sanctuary cities with Democratic mayors. The bus trips were free and voluntary for

[19] 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.

migrants and may have been particularly attractive to migrants who did not have strong networks in the country. 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). 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.[20] In particular, Denver and Philadelphia are not traditional destinations for new migrants. There is a strong positive relationship between the quarterly number of bused migrants and the number of surge migrants (see online appendix figure A5). 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 A6 and A7).

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.[21] 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 A8), 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 that period, an issue we return to below.[22] As noted above, many surge migrants were eligible for

[20] 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.

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

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

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.[23]

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.[24] 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 predetermined 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.[25] 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 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.[26]

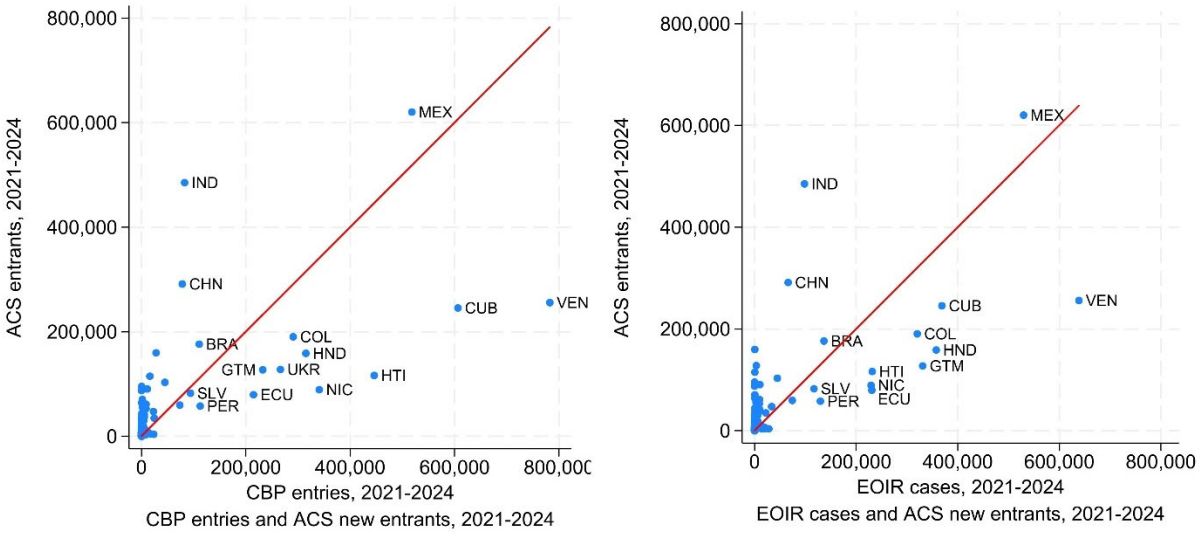
Figure 6 compares the total number of non-citizens who reported that their residence one year ago was abroad in the 2021-2024 ACS with the number that CBP allowed to enter and the

[23] 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 A7, A8, and A9 show those data.
[24] For discussion of how well the Current Population Survey (CPS) captured the surge, see, for example, Edelberg et al. (2026).
[25] 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.
[26] 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.

number of EOIR cases for 2021-2024 migrants, by country.[27] Figure 6 highlights the 12 Latin American and Caribbean countries that composed most of the surge (see online appendix figure A2) 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, suggesting they were undercounted in the ACS. The likely 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.

[27] 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 A11 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 A14 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 predetermined fixed population controls to weight observations, and those controls do not incorporate immigrant status.

Figure 6: 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 unsheltered, 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.[28] Lack of English or

[28] The ACS instructions say to include anyone living or staying at the address for more than two months, and 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).

Spanish literacy – the two languages in which the ACS is primarily conducted – may also hamper responses.

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.[29] 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).

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

[29] 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. 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 mean 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.

II.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.

III. Data and Methods

We use data 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 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. 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, 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.[30]

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 (BEA) data.[31] The GDP data only capture formal-sector activity, but they should 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 produced by Zillow.[32] 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 variables 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.[33]

[30] Another reason for this approach is that we do not know when in the calendar year a respondent answered the ACS question about previous 12 months' wage and salary income, which we use to construct hourly wages.
[31] 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.
[32] 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.
[33] Wages are deflated using the CPI-U. Rents are deflated using the CPI-ex-shelter.

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 between 2021 and 2022 reflects the ongoing recovery from the pandemic recession, while the simultaneous $2.40 decline in the real average native wage reflects accompanying inflation (the rise and fall in nominal wages due to education and experience compositional effects happened prior to our data). 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 data, we adjust wages and other outcomes (except domestic migration) for gender, age, and education, to adjust for compositional effects that vary by metro.[34]

[34] 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, as well as metro × year dummies. We then use the coefficients on the metro × year dummies 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
		(1.18)	(3.29)	(13.19)
B. Aggregate variables:				
Employment (in thousands)	680	711	726	735
	(1085)	(1143)	(1165)	(1179)
Hourly wage ($2024)	32.84	31.92	31.72	31.83
	(5.06)	(4.78)	(4.69)	(4.65)
GDP (in billions $2024)	120.3	115.1	113.9	114.1
	(231.8)	(220.8)	(216.4)	(216.4)
Rent ($2024)	1682	1718	1744	1766
	(461)	(475)	(477)	(481)
C. ACS-constructed and domestic migration variables:				
Hourly wage for U.S. natives ($2024)	34.3	31.7	33.0	33.4
	(6.1)	(6.3)	(6.0)	(6.3)
Hourly wage for U.S. natives ($2024) National-level trimming	32.8	30.3	31.3	32.0
	(5.0)	(5.0)	(4.8)	(4.8)
Hourly wage for U.S. natives ($2024) Metro-level trimming	32.7	30.0	30.9	31.7
	(5.7)	(5.3)	(5.5)	(5.6)
Weekly wage for U.S. native ($2024)	1319	1218	1252	1277
	(235)	(224)	(230)	(233)
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)	1510	1518	1597	1650
	(417)	(397)	(415)	(420)

Sources: EOIR; CBP; Arizona Department of Emergency and Military Affairs and Texas Division of Emergency Management (buslift migrants); BLS CES (employment, hourly wage); BEA (GDP); Zillow; ACS for U.S. native wages, employment, and rent; Census Bureau for population and net domestic in-migration (vintage 2025); 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; GDP, which is for 160 metro areas; and average rent, which is for 92 metro areas. Gross rents include utilities. The 2021 surge statistics are not included in the regressions. The buslift began in 2022. Trimming involves dropping the bottom 2%, top 0.5% and part-time workers in the next 0.5%, at the worker level. "National-level trimming" indicates that the trimming is applied to pooled metros for each year, while "Metro-level trimming" indicates that it is applied separately by metro and year.

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:

$$y_{mt} - y_{m,\,t-k} = \alpha + \varphi\, Surge^{k}_{mt} + \beta_1 Pop_{m,\,2019} + \beta_2 (y_{m,\,2019} - y_{m,\,2016}) + \sum_{\tau} \beta_{3,\,\tau}\, s_{m,\,\tau} + \Delta^{k} \varepsilon_{mt}, \tag{1}$$

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. 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:

$$y_{mt} - y_{m,\,t-k} = \delta + \rho\left(\frac{Surge^{k}_{mt}}{Pop_{m,\,t-k}}\right) + \gamma(y_{m,\,2019} - y_{m,\,2016}) + \Delta^{k}\nu_{mt}. \quad (2)$$

The coefficient of interest is ρ. Although the dependent variable is sometimes adjusted at the micro level for individual characteristics, the control for the 2016-2019 trend is always based on the unadjusted *y*.[35] Also controlling for a pandemic-period change in the dependent variable has little effect on the coefficient of interest, so we do not report results with that control.

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:

$$\frac{n^{k}_{mt}}{Pop_{m,\,t-k}} = \theta + \lambda\left(\frac{Surge^{k}_{mt}}{Pop_{m,\,t-k}}\right) + \mu\left(\frac{n^{3}_{m,\,2019}}{Pop_{m,\,2016}}\right) + \omega_{mt}, \quad (3)$$

where n^{k}_{mt} is the cumulative net migration into metro area m over the *k* years ending in year *t*.

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 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.[36]

[35] A complete analysis of the response of non-surge migration to surge migration would examine the impact of surge migration on other international migration. However, although the shift-share instrument is a very poor predictor of non-surge international migration in this period, this need not imply that the shift-share instrument is completely uncorrelated with the error term in a regression examining non-surge international migration, so we do not examine this adjustment dimension.

[36] 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 up to two years before the last ACS interviews of year t.

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 λ 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), whose construction 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.[37] The local area treatment 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 have a very large weight in 2021-2022, though it falls over time and is slightly lower than Venezuela's for the period as a whole. 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.[38]

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 (e.g., 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.

[37] 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 A1 for the 25 groups and their number and share of CBP entries during 2021-2024.

[38] Jaeger et al. (2018) observe that unless the mix of origin countries changes suddenly, estimates based on shift-share instruments will reflect a mix of short-run effects (presumed negative) and long-run effects (presumed positive) due to serial correlation in the instruments. We do have sudden changes: the correlations of our origin country composition shares in national inflows are only 0.34 between 2021 and 2022 and 0.40 between 2021 and 2024. Despite this, we are not able to separately identify the contemporaneous and lagged inflows as recommended, but our results and the issues involved in a short annual panel differ from those of Jaeger et al. (2018) and their decadal panel.

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 as an additional instrument 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.[39] 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.[40] 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. With the exogeneity of this instrument in mind, we have experimented with controlling for there having been any Republican mayor over the period in the core municipality and whether the core municipality is a "sanctuary city." Because these do not affect the coefficient of interest, we do not report these results.

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-form results, which do not use EOIR data, in the online appendix; 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 total entries. Third, we test sensitivity to removing the origin countries farthest from the 45-degree line in figure 4: 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

[39] 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.
[40] Denver, the smallest of the six buslift destinations, is the 20th most populous metro.

(bottom panel), and for different time periods in each column. The coefficients on all shift-share instruments are statistically significant at the 5% level with F-statistics of at least 10 (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 (bottom panel, column 1). Rotemberg weights are tabulated in online appendix tables A2 and A3.

Table 2 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 the slowest. 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; in the bottom panel, the coefficients are smaller for this sample, whereas results in the top panel are similar for both samples.

We find that the buslift is a sufficiently strong instrument only for 2023-2024, when the numbers increased greatly from low levels (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.[41] During the height of the buslift in 2023-2024 (column 6 top panel), each additional bused migrant is associated with an additional 1.8 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

[41] We determine that the buslift instrument is weak for the early surge years not principally because it has low statistical significance in the first stage, but because the second-stage coefficients on surge migration are not close to statistically significant for any outcome for these years. Angrist and Kolesár (2024) caution against selecting instruments based on the first stage F-statistic for just-identified IV, which is approximately median-unbiased.

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	2021-2022	2022-2023	2023-2024	2023-2024	2023-2024
	(1)	(2)	(3)	(4)	(5)	(6)
A. In levels, with full controls:						
Instrument in levels	0.21***	0.42***	0.18***	0.15**	0.16**	1.84***
	(0.04)	(0.03)	(0.05)	(0.05)	(0.05)	(0.42)
						[p=0.05]
Buslift metro indicator variable	--	--	--	--	--	-3.57***
						(0.98)
						[p=0.09]
F-statistic	25	176	14	10	12	--
B. Relative to population, controlling for 2016-2019 employment change:						
Instrument/ population	0.31***	0.54***	0.24***	0.27***	0.16***	1.18***
	(0.07)	(0.10)	(0.06)	(0.08)	(0.03)	(0.19)
						[p=0.07]
F-statistic	17	29	18	11	23	--
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 change 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 change in the native employment rate. Parentheses contain robust standard errors (and asterisks refer to these); brackets contain Webb wild bootstrap p-values based on 9,999 repetitions (column 6). *** p<0.01, ** p<0.05, * p<0.10.

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. We also estimate equation 2 for natives' rent (plus utilities) from the ACS, plus rent from Zillow. In our last regressions, we estimate equation 3 to examine domestic migration based on Census Bureau statistics. 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 0.7 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 (column 1, rows 2 and 3). With each EOIR case equivalent to 0.8 surge migrants (figure 4), each surge migrant added about 0.4 jobs over 2021-2024.[42] Wilson and Zhou (2026) report a similar estimate, 0.5, in their appendix Table B2. 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, the surge increased total employment in major metros by about 1.6 million.[43] The surge thus accounted for about 14% 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 instruments and examine 2023-2024 (column 6), the coefficients are imprecisely estimated despite a reasonably strong first stage (Table 2, top panel, column 6).

[42] If we instead apply the adjustment factor 0.73 from appendix figure A3, each surge migrant added about 0.35 jobs.

[43] Based on a total of 3.25 million EOIR cases in the 164 major metro areas, multiplied by 0.5 jobs per EOIR case. Total employment gains across those metros were 9 million over 2021-2024.

Table 3: Effect of surge migrants on total employment

	2021-2024 (1)	2021-2022 (2)	2022-2023 (3)	2023-2024 (4)	2023-2024 (5)	2023-2024 (6)
OLS	0.7***	0.5	0.7**	0.4***	0.1	
Full controls	(0.1)	(0.4)	(0.3)	(0.1)	(0.1)	
IV	0.5***	0.2	0.8***	0.5***	0.2	0.1
Full controls	(0.1)	(0.2)	(0.2)	(0.1)	(0.2)	(0.1) [p=0.7]
IV	0.5***	0.3	0.7*	0.5***	0.5***	0.1
Population control only	(0.1)	(0.2)	(0.3)	(0.1)	(0.2)	(0.3) [p=0.9]
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. The regressions in the first two rows also control for the 2019 population, the 2016-2019 change in employment, and the 2016-2019 share of employment in each supersector (excluding government); the regressions in the third row control only for the 2019 population. Parentheses contain robust standard errors (and asterisks refer to these); brackets contain Webb wild bootstrap p-values based on 9,999 repetitions (column 6). *** p<0.01, ** p<0.05, * p<0.10.

The increase in employment tended to be largest in the leisure and hospitality and professional and business services sectors, based on an analysis of changes in employment shares (online appendix figure A15). Migrant-spurred relative growth in those sectors is not surprising since they include many jobs that require little formal education or training. We caution that the data likely undercount relative growth in construction, in which many migrants may be self-employed, contract or day laborers, or paid off-the-books. The payroll data also do not capture two other important groups: gig workers, who may have worked in ride-sharing and delivery services, and private household workers, such as nannies, housekeepers, and the like. The payroll data do not include agricultural employment, but that would occur largely outside the metro areas we examine.

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 2.0 % over 2021-2024.[44] Given surge migrants' share of the population

[44] Adjusting for the EOIR undercount by 0.8 implies that a one percentage point increase in surge migration increased GDP by 1.6%. Adjusting by 0.73 implies that a one percentage point increase in surge migration increased GDP by about 1.5%.

(0.9%), this implies an aggregate GDP effect of 1.8%. That estimate is larger than the population increase due to surge migration, indicating they boosted GDP per capita.

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 according to the wild bootstrap standard errors.

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	1.6**	4.0***	1.1	-0.3	0.1	
Trend control	(0.6)	(1.1)	(0.9)	(0.5)	(0.5)	
IV	2.0***	3.5***	2.6*	0.2	0.2	-1.9***
Trend control	(0.5)	(0.8)	(1.1)	(0.5)	(0.4)	(0.5)
						[p=0.3]
IV	2.3***	3.9***	2.4**	0.7	0.4	-2.0**
No trend	(0.5)	(0.8)	(0.9)	(0.5)	(0.5)	(0.7)
						[p=0.2]
Observations	160	160	160	160	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). The trend control is the 2016-2019 difference in log GDP. Parentheses contain robust standard errors (and asterisks refer to these); brackets contain Webb wild bootstrap p-values based on 9,999 repetitions (column 6). *** p<0.01, ** p<0.05, * p<0.10.

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 2.1%, albeit statistically significant only at the 10% level. Adjusting for the EOIR undercount, the implied effect is about -1.7%. The corresponding effect on average weekly wages in Wilson and Zhou (2026) is -0.9% and is not statistically significant. 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 very 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 (1)	2021-2022 (2)	2022-2023 (3)	2023-2024 (4)	2023-2024 (5)	2023-2024 (6)
OLS	-1.5	-2.0	-2.1	-0.1	-0.1	
Trend control	(0.8)	(1.4)	(1.4)	(0.7)	(1.3)	
IV	-2.1*	-1.8	-3.3*	-1.3	-2.4***	2.0
Trend control	(1.0)	(1.5)	(1.6)	(0.8)	(0.7)	(2.3)
						[p=0.9]
IV	-2.0	-1.5	-3.2*	-1.3	-2.2**	2.1
No trend	(1.0)	(1.5)	(1.6)	(0.8)	(0.8)	(2.5)
						[p=0.9]
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. The trend control is the 2016-2019 difference in log wages. Parentheses contain robust standard errors (and asterisks refer to these); brackets contain Webb wild bootstrap p-values based on 9,999 repetitions (column 6). *** p<0.01, ** p<0.05, * p<0.10.

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 compositional 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% (implying a coefficient of -1.0). A coefficient of -2.1 (Table 5, column 1, row 2) would require in addition that the surge migrant employment rate be more than twice that of others.[45]

We are unable to replicate the negative sign using ACS data on all workers, even calculating the wage as a ratio of the metro wage bill to metro weekly hours worked, based on private non-farm workers, to mimic the CES wage: the coefficient is 0.4 with a standard error of 0.7 (appendix table A4). Although we cannot claim that the ACS coefficient contradicts and is superior to the CES coefficient, given the standard errors and the absence of most of the surge

[45] This calculation ignores that CES reports average wages that are weighted by hours worked.

migrants from the ACS, we feel the evidence that overall wages fell as a result of surge migration is weak.

For native workers, the coefficients in Table 6 column 1 show that a one percentage point increase in surge migrants over 2021-2024 has a statistically significant positive effect on (adjusted) native wages of 1.6% in OLS and 1.2% with the shift-share instrument, in both cases controlling for wage changes in 2016-2019. The reduction in the coefficient suggests upward bias in OLS as surge migrants choose locations with faster wage growth. Without the past wage change covariate (third row), the corresponding IV coefficient is unchanged, and it is also robust to two different ways of trimming wages in the micro data (coefficients of 1.2 and 1.1 in the fourth and fifth rows). The estimate for untrimmed weekly wages, which should have less measurement error but reflect the responsiveness of both hourly wages and weekly hours, is slightly higher at 1.5 (sixth row).[46] These IV coefficients are similar to the corresponding 1.3% coefficient of Clemens et al. (2026) (their Appendix Table 1) [47] but different from Wilson and Zhou (2026), who find a negative effect (-0.7%) for 2021-2023. To account for the EOIR case undercount, our coefficients should be multiplied by 0.8.[48]

The positive wage effect of migration on natives 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 legally. Clemens et al. (2026) instead suggest complementarity between surge migrants and the average U.S. native as the cause of the positive wage effect. Pre-pandemic immigrants from the surge countries (other than Colombia, Venezuela, and Ukraine) typically had little education, making them particularly substitutable with native high school dropouts and potentially complementary with more educated natives. A positive overall wage effect would result if complementarity outweighed substitution.

The economy should adjust to an initial boom in demand either by local responses or the dispersal of the local effect across metros. There could be adjustment to increased complementarity, as native workers enter the most complementary occupations, for example.

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[46] As implied by the stable coefficient when the 2016-2019 wage trend is dropped, a regression of this wage trend on the instrument, a test for confounding pre-trends, yields a null coefficient. 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.

[47] However, they report a 3.1% coefficient for the 2021-2023 period, whereas our unreported coefficient for this period is only 1.8%.

[48] The coefficients on the two other papers using EOIR data also need to be adjusted. The factor for Clemens et al. (2026) could be quite different, as their origin countries differ due to a focus on asylum seekers.

Such adjustment is consistent with our finding that the estimated shift-share IV wage impacts are more positive for the beginning of the surge in 2021-2022 (Table 6, column 2) than over 2021-2024 and except in one row considerably so, even if the differences are unlikely to be statistically significant. The IV point estimates vary considerably, however, ranging from a statistically insignificant 1.6 (row 5) to a large 4.3 for weekly wages (row 6). [49]

The year-to-year effects generally decline with time (Table 6, columns 2-4) to become zero for 2023-2024. The timing is slightly different in the fifth row for the wage measure with outliers trimmed based on a pooled national sample ("National trim"), as the positive effect peaks in 2022-2023 rather than 2021-2022, and the 2023-2024 impact is negative and statistically significant. The reduced-form effects all decline with time and are never statistically significantly negative (first three rows of online appendix table A5). Overall, the year-to-year coefficient pattern is consistent with long-run adjustment of the economy to shocks that were increasingly anticipated and planned for, though also with a deterioration in the macro-economy or a change in the origin country mix over time leading to changes in the Rotemberg weights.

For comparison to the 2023-2024 buslift instrument results in the 30-metro sample, column 5 shows whether restricting the column 4 analysis to the 30 metros changes the results. As in column 4, the coefficients are variable and, except the coefficient of -0.9 in the fifth row, imprecisely estimated.

The bus IV point estimates are consistently negative in column 6, varying from -1.4 to -2.0, all of them except -1.4 more negative than estimates found in the literature even after applying the EOIR undercount adjustment factor of 0.8.[50] However, the only statistically significant coefficients are those based on untrimmed hourly wages, which are statistically significant at only the 10% level. Given the standard errors, it is difficult to be sure whether in the 30-metro sample, the buslift IV coefficients and the shift-share IV coefficients differ from each other. If they do not, both may reflect that the impact was more negative late in the surge. The difference in point estimates is nevertheless suggestive of different LATE effects, either because bus passengers and surge migrants choosing destinations based on ethnic networks have

[49] Despite the stronger first stage for 2021-2022 than for 2021-2024, the standard errors in the second stage are larger for 2021-2022 when the variation in the share of surge migrants was not yet as high.
[50] For example, the comparable coefficient of -1.2 in Altonji and Card (1991) is among the most negative in the literature (see National Academies 2017 chapter 5).

different characteristics, or because the impact of migrants with limited choice of destination is more negative than the impact of migrants availing themselves of networks.

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

	2021-2024	2021-2022	2022-2023	2023-2024	2023-2024	2023-2024
	(1)	(2)	(3)	(4)	(5)	(6)
OLS		4.9***	1.1	-1.1	-0.6	
Trend control	1.6***	(1.3)	(0.9)	(0.7)	(0.7)	
No trim	(0.4)					
IV	1.2***	3.2***	1.0	-0.6	0.1	-2.0***
Trend control	(0.4)	(1.0)	(0.7)	(0.7)	(0.5)	(0.5)
No trim						[p=0.06]
IV	1.2***	3.2***	1.0	-0.6	-0.1	-1.8***
No trend	(0.4)	(1.0)	(0.7)	(0.7)	(0.4)	(0.6)
No trim						[p=0.09]
IV	1.2***	2.8**	1.3	-0.7	0.8	-1.7***
Trend control	(0.4)	(1.1)	(1.0)	(0.8)	(0.5)	(0.5)
Metro trim						[p=0.12]
IV	1.0***	1.6	3.2***	-1.4**	-0.9**	-1.4***
Trend control	(0.4)	(1.0)	(0.8)	(0.6)	(0.4)	(0.5)
National trim						[p=0.27]
IV	1.5***	4.3***	1.7	-1.3*	-0.3	-1.8***
Trend control	(0.4)	(1.0)	(1.2)	(0.8)	(0.4)	(0.6)
Weekly wage						[p=0.26]
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, except for the weekly-wage row, which uses the corresponding differenced native log real weekly wage. The trend control is the 2016-2019 change in the (unadjusted) native log wage. Trimming involves dropping the bottom 2%, top 0.5% and part-time workers in the next 0.5%, at the worker level. "National trim" indicates that the trimming is applied to pooled metros for each year, while "Metro trim" indicates that it is applied separately by metro and year. Parentheses contain robust standard errors (and asterisks refer to these); brackets contain Webb wild bootstrap p-values based on 9,999 repetitions (column 6). *** p<0.01, ** p<0.05, * p<0.10

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 214 to the surely unimagined height of 19,047: 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.

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 do not see clear evidence for this hypothesis, as the 2021-2024 shift-share IV point estimates are positive for all four education groups and the relative size of the positive coefficients is sensitive to changes in the wage measure (appendix table A6). Standard errors are large, with a statistically significantly positive effect for college graduates being the only robust result; our test is thus somewhat inconclusive.

IV.C. Native Employment Rates

We investigate the impact of surge migrants on the (adjusted) native employment rate in Table 7. 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 (column 1) allow us to rule out moderate effects over the whole period. The shift-share IV coefficient of 0.2% for 2021-2024 (rows two and three) equals the corresponding statistically insignificant coefficient of 0.2% in Clemens et al. (2026) (their Appendix Table 1). Clemens et al. (2026) estimate a statistically significant coefficient of 1.7% for 2021-2023, however, which is much larger than both our unreported coefficient of 0.2% and the Wilson and Zhou (2026) coefficient of 0.2% for 2021-2023.

Analysis by native education suggests that in 2021-2022, college graduates experienced employment gains while high school dropouts experienced similar-sized losses (Table 8, column 2), but the fact that this pattern reverses in 2022-2023 (column 3) leaves the interpretation unclear. More clear is that high school dropout employment responded negatively to buslift-influenced surge migration, with a one percentage increase in surge migration reducing the employment rate by a very large 4.8 percentage points (column 6).

Table 7: 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 Trend control	0.0 (0.2)	0.6 (0.6)	-0.3 (0.5)	-0.4 (0.4)	-0.3 (0.3)	
IV Trend control	0.2 (0.1)	0.8 (0.5)	-0.2 (0.4)	-0.4 (0.5)	-0.0 (0.3)	-0.9** (0.4) [p=0.14]
IV No trend control	0.2 (0.1)	0.9 (0.6)	-0.3 (0.4)	-0.4 (0.5)	-0.0 (0.3)	-0.9** (0.4) [p=0.13]
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 native employment rate in the ACS. The trend control is the three-year 2016-2019 change (unadjusted) in the native employment rate. Parentheses contain robust standard errors (and asterisks refer to these); brackets contain Webb wild bootstrap p-values based on 9,999 repetitions (column 6). *** p<0.01, ** p<0.05, * p<0.10

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

	2021-2024 (1)	2021-2022 (2)	2022-2023 (3)	2023-2024 (4)	2023-2024 (5)	2023-2024 (6)
High school dropouts	-0.8 (0.8)	-2.4* (1.5)	2.3** (1.1)	-1.4 (1.3)	0.4 (0.7)	-4.8*** (0.9) [p=0.03]
High school graduates	0.4 (0.3)	0.4 (0.7)	-0.5 (1.0)	0.6 (0.7)	0.6 (0.5)	-1.3 (1.0) [p=0.14]
Some college	0.3 (0.3)	0.7 (1.2)	-0.1 (0.8)	-0.3 (0.8)	0.5 (0.9)	-0.2 (0.5) [p=0.65]
College graduates	0.4* (0.2)	2.6** (1.1)	-1.2* (0.6)	-0.6 (0.5)	-0.9*** (0.2)	-0.4 (0.4) [p=0.28]
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 %). The outcome is the differenced native employment rate in the ACS. All regressions include the three-year 2016-2019 change (unadjusted) in the native employment rate. Parentheses contain robust standard errors (and asterisks refer to these); brackets contain Webb wild bootstrap p-values based on 9,999 repetitions (column 6). *** p<0.01, ** p<0.05, * p<0.10

IV.D. Rents

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 all rentals (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 over 2021-2024, based on OLS and the shift-share instrument (Table 9, column 1). In the IV specification controlling for past rent changes, a one percentage point increase in surge migrants increased rents by 1.4% for the sample of 164 metros in 2021-2024.

Table 9: 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 Trend control	1.3* (0.7)	2.2 (1.5)	3.1 (1.9)	-1.0 (1.3)	-0.9 (1.2)	
IV Trend control	1.4** (0.7)	2.3* (1.2)	6.0** (2.4)	-2.9 (2.2)	0.2 (1.1)	-1.4 (1.3) [p=0.50]
IV No trend control	1.6** (0.7)	2.4** (1.2)	6.0** (2.3)	-2.5 (2.1)	-0.1 (1.2)	-1.0 (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 %), multiplied by 100. The outcome is differenced adjusted native log rent from the ACS. Rents include utilities and are deflated by the CPI excluding shelter (2024 $). "Trend control" refers to the three-year 2016-2019 change in the (unadjusted) native log rent. Parentheses contain robust standard errors (and asterisks refer to these); brackets contain Webb wild bootstrap p-values based on 9,999 repetitions (column 6). *** p<0.01, ** p<0.05, * p<0.10

The short-run shift-share results (Table 9, columns 2-4) show that, as with wages, the effects fade over time. The 2021-2022 coefficient of 2.3% and the 2022-2023 coefficient of 6.0% (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 peak adjustment in 2022-2023 is later than for most wage measures, 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, the shift-share IV coefficient is very small (0.2% in column 5). For these same metros using the bus instrument (column 6), the negative coefficient of -1.4% is not close to statistically significant.[51]

The aggregate results, i.e., effects for rental units independent of residents' nativity, confirm the results for natives: surge migration pushed up rents, especially early in the surge. The 2021-2024 shift-share IV coefficient of 1.8 (Table 10, column 1, row 2) is similar to the corresponding coefficient of 1.4 in Table 9. Wilson and Zhou (2026) report a similar marginal response. In our estimates, a 0.9% inflow of surge migrants implies an increase in rent over 2021-2024 of 1.6%. Wilson and Zhou (2026) find a much larger aggregate impact than this (4.3%), because they multiply their coefficient by 3.1%, which is the initial employment share of their measure of surge migration (we use population shares, in contrast). The short-run impacts diminish over time (columns 2-4), more quickly than in the natives' rent results reported in Table 9: 2021-2022 is the only period in which aggregate rents rise, with the statistically significant shift-share IV coefficient of 7.9% in 2021-2022 falling to -2.1% 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 the coefficient is imprecisely estimated (column 6).

[51] 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.

Table 10: Effect of surge migrants on average rent

	2021-2024	2021-2022	2022-2023	2023-2024	2023-2024	2023-2024
	(1)	(2)	(3)	(4)	(5)	(6)
OLS	0.6	6.3***	-1.0	-2.0**	-2.8**	
Trend control	(0.7)	(1.3)	(0.9)	(0.7)	(1.0)	
IV	1.8***	7.9***	-1.1*	-2.1***	-1.4	1.4
Trend control	(0.5)	(0.8)	(0.5)	(0.6)	(0.7)	(2.4)
						[p=0.6]
IV	2.0***	7.5***	-0.6	-1.7**	-0.9	1.8
No trend	(0.5)	(0.8)	(0.6)	(0.6)	(0.7)	(2.3)
						[p=0.5]
Observations	92	92	92	92	30	30
Instrument	Shift share	Shift share	Shift share	Shift share	Shift share	Buslift

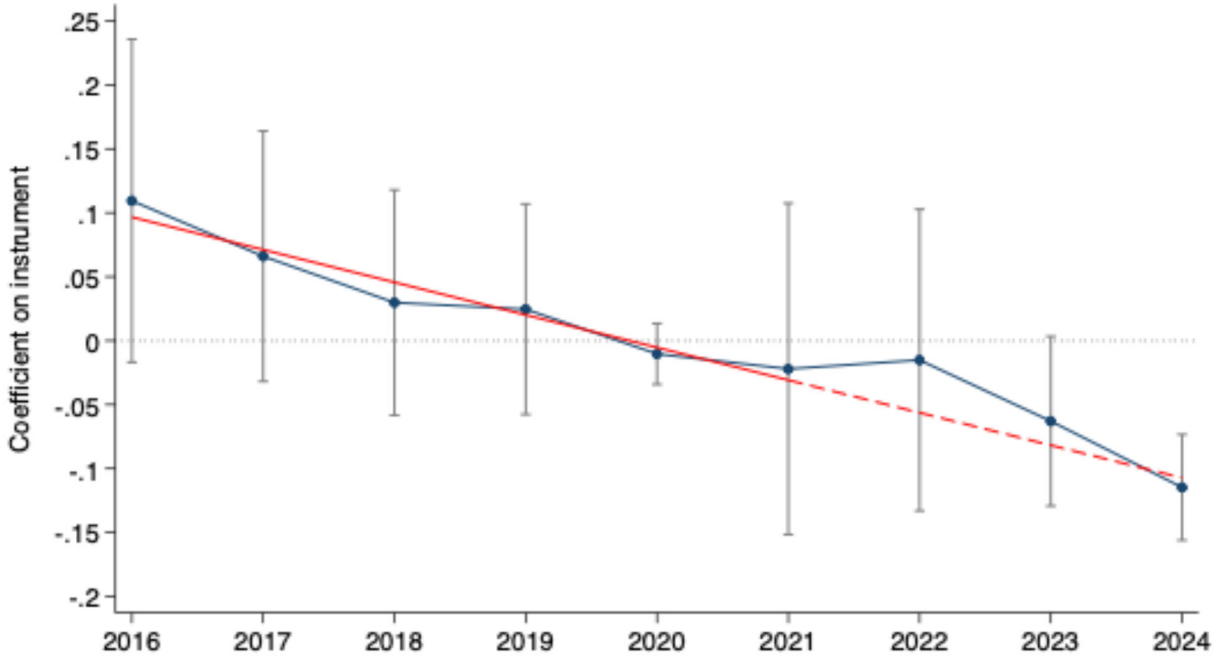
Source: Authors' calculations.

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 rent over the period indicated (×100). The trend control is the 2016-2019 difference in log rent. Parentheses contain robust standard errors (and asterisks refer to these); brackets contain Webb wild bootstrap p-values based on 9,999 repetitions (column 6). *** p<0.01, ** p<0.05, * p<0.10

IV.E. Domestic Migration

To assess whether domestic migration is an adjustment channel in our context, we analyze the impact of surge migration on net domestic in-migration (of all persons, unadjusted). The estimated shift-share coefficients are implausibly negative, generally less than -1, and as low as -1.6 (Table A7). By contrast, the preferred IV point estimate with the buslift instrument is 0.0. The explanation is that the shift-share IV predicts that surge migrants will choose metro areas that have been experiencing a long-run decline in net domestic in-migration. An unreported regression of the 2016-2021 change in net migration on the 2021-2024 instrument yields a coefficient of -0.131 (0.036). A different way of seeing this is shown in Figure 7, which reports coefficients from separate regressions of net migration in each year on the 2021-2024 shift-share instrument. Whereas the instrument predicts an extra percentage point of 2021-2024 surge migration to a metro is associated with a 0.1 percentage point higher net in-migration in 2016 (compared to an average of 0.32 over the whole period), the extra percentage point is associated with no higher net migration by 2020, and the negative association in 2023 and 2024 looks plausibly like a continuation of the same trend. Based on this and on the buslift IV coefficient, we conclude that there is little or no offsetting domestic migration.

Figure 7: Regressions of net domestic in-migration by year on 2021-2024 shift-share instrument



Source: Authors' calculations.
Notes: The blue dots are the coefficients on the 2021-2024 shift-share instrument from annual regressions of net domestic in-migration on the instrument; the gray vertical lines are the 95% confidence intervals based on robust standard errors. The red solid line is the regression lines through the first six coefficients (2016-2021), while the dotted red line is the out of sample prediction. A year is measured from 1 July of the previous year to 30 June of the current year.

IV.F. Robustness Checks

We have noted that the coefficient on the EOIR measure of surge migration does not represent the impact of one surge migrant, and we proposed multiplying the coefficient by 0.8 or 0.73. An alternative is to re-estimate the shift-share regressions omitting Ukraine, Cuba, and Haiti from the construction of the instrument, since for these three countries, the difference between total counts in the EOIR data versus the CBP counts is particularly large. Furthermore, Cuba has a large Rotemberg weight share of 48% in the 2021-2022 first stage (online appendix table A3), which falls as those of Haiti and Venezuela rise. There are no systematic differences between the new coefficients for wages and employment rates (online appendix table A8, panel B) and those in Tables 6 and 7. However, the new coefficients for rent are much closer to zero (notably, one third the size for 2021-2024) and are statistically insignificant, possibly representing a different LATE effect and suggesting that it is migrants whose destination is

influenced by Cuban and/or Haitian networks who are increasing native rents. The original shift-share instrument predicts that a large share of Cuban and Haitian migrants go to Miami, which experienced sizable rent increases during this period.

Wilson and Zhou (2026) and Clemens et al. (2026) focus on results from weighted regressions, and we repeat our regressions weighting with the 2019 metro population (online appendix table A9, panel B). For wages and rents, the coefficients suggest the immigration impacts on wages and rents were more concentrated in 2021-2022 and 2022-2023, respectively, than unweighted regressions suggested, while for employment the coefficients are similar to those in unweighted regressions. Wilson and Zhou (2026) and Clemens et al. (2026) also focus on native outcomes unadjusted for individual-level characteristics. Doing likewise does not flip any of the positive coefficients' signs, but it does reduce to zero the previously negative effect on wages using the buslift instrument (online appendix table A10). This appears to be due to a 2023-2024 increase in the share of Denver workers who are college graduates, which masks the negative immigration effect on wages purged of compositional effects.

We also repeat all of the Table 6 wage 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 A11). This is consistent with Wilson and Zhou (2026) and Clemens et al. (2026) for 2021-2024. Finally, we note that Miami has a much higher value of each shift-share instrument than any other metro. Although dropping Miami strengthens the first stage, as considerably fewer surge migrants went to Miami than predicted, dropping Miami raises the second-stage standard error on surge migration considerably, rendering some coefficients of interest statistically insignificant without greatly changing the point estimates (results not reported).

IV.G. 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 11, based on coefficients for our preferred IV specifications for each native outcome in Tables 6, 7 and 9. For wages, we present results for the two measures with the most different results.

Table 11: 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 (%), no trim	1.1***	0.6***	0.3	-0.3	-1.3*
Wages (%), national trim	0.9***	0.3	0.8***	-0.7**	-0.9
Employment rate (percentage points)	0.2	0.2	-0.1	-0.2	-0.6
Rent (%)	1.3**	0.4*	1.5**	-1.4	-1.0
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, Trend control") from table 6 (wages), table 7 (employment rate), and table 9 (rent), using underlying non-rounded numbers. The "Surge migrants / population" numbers are means presented (except for 2021-2024) in Table 1. In columns 1-4, asterisks indicate the statistical significance of the product based on robust standard errors in columns 1-4 of the relevant tables and the standard errors of the means, ignoring covariance between the product terms. The asterisks in column 5 reflect the coefficient Webb wild bootstrap p-value in column 5 of the relevant tables.

Based on the shift-share instrument, the surge raised native wages by 0.9-1.1% over the whole 2021-2024 period (first row, column 1), depending on the wage measure, 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 0.3-0.6% in 2021-2022 and 0.3-0.8% in 2022-2023 (columns 2 and 3), and fell in 2023-2024 (-0.3%- -0.7% in column 4), with the results overall pointing to the less negative end of the range (as argued in the discussion of Table 6). Based on the bus instrument, the surge reduced 2023-2024 native wages by a moderate 0.9-1.3% (column 5). The bus instrument calculations yield a moderate 0.6 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 1.3% 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 0.4% (column 2) rose to 1.5% in 2022-2023 (column 3). The effect on rents apparently became negative in 2023-2024, though this -1.4% is imprecisely estimated. If we assume all workers are renters and that 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%. Based on unreported wage and employment regressions for a sample of renters, we calculate that native renters' wages net of rent rose considerably more, by 2.3%.[52]

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 U.S. natives' 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. Although wages rose, we calculate that the average U.S. native renter's wage net of rent increased. We find that the migrants whose destination was influenced by the buslift had a more negative impact than other migrants, a result largely driven by Denver, which may be less adaptable than other metros.

We find interesting dynamics when looking at year-to-year changes during the three-year period. The short-run responses of natives' wages and rents to surge migration declined over time and even reversed in the case of surge migrants who were bused. This could reflect economic adjustment or the waning of the post-pandemic recovery. Changes in domestic migration played little or no role in dampening these responses over time. Nevertheless, the impacts of the surge migration on affected metros could have been diffused across larger geographies over time by capital or trade flows so as to be no longer detectable in cross-metro comparisons. Although the magnitude of surge migration increased over 2021-2024, the total impact of the surge as measured across metros on GDP, native wages and rents nonetheless fell over that period, 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. Of course, it is likely that the unique circumstances of the pandemic recovery, marked by the

[52] Unreported results show that a one percentage point surge increase raised native renters' wages by 2.2% over 2021-2024 and had no impact on native renters' employment rate. This translates into a 0.9*2.2=2.0% increase in wages and hence a (2.0-0.3*1.3)/0.7=2.3% increase in post-rent wage.

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 economic effects of the migration surge.

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Online appendix

Data appendix

Surge migrant data

Our key explanatory variable, the number of surge migrants, is measured as the number of cases in the U.S. Department of Justice, Executive Office for Immigration Review (EOIR) database, available at https://www.justice.gov/eoir/foia-library-0. We use the January 2026 version of the EOIR case data and include only non-detained cases. 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; this occurs for about 38% of cases during 2021-2024. (If both date of entry and date of NTA are included, they are the same calendar year in 84% of cases with a 2021-2024 entry.) The EOIR data include migrants' zipcode, which we map into metro areas using a crosswalk from the U.S. Department of Housing and Urban Development. A case may represent an individual or a family, and some individuals or families may appear more than once in the data.

The national surge entries by origin country, used in the construction of the shift-share instrument, are counts of the number of migrants encountered by Customs and Border Protection (CBP) personnel and allowed to enter the United States. We use the U.S. Department of Homeland Security microdata on U.S. Border Patrol (USBP) encounters with migrants between ports of entry, available at https://www.cbp.gov/document/foia-record/customs-and-border-protection-border-patrol-statistics, and classify migrants with a disposition code of NTA, P (Parole), Rel, or I as released (allowed to enter). As a robustness check, we add migrants with a disposition code of WA/NTA, multiplied by the share of Immigrant and Customs Enforcement (ICE) book-outs at the Southwest border that were not for removal, by year, using data from the DHS Office of Homeland Security Statistics, and migrants with a disposition code of ADMIT. We also use microdata on encounters with migrants at ports of entry from the Office of Field Operations (OFO), available at https://www.cbp.gov/document/foia-record/cbp-office-field-operations-statistics, and classify migrants with a disposition code of NTA, NTA/WA, NTA-Person Released, or any form of Paroled as allowed to enter.

American Community Survey (ACS) data

We use ACS data from IPUMS (Ruggles 2025) to measure outcomes of the native-born and to construct the shift-share instrument. We define natives as those born in the United States or born abroad of U.S.-citizen parents. The native employment rate is based on natives aged 16-64. The native wage is calculated either using a sample of native-born workers aged 16-64 who are not self-employed and whose wage is not zero, or using this sample with outliers trimmed in two different ways based on wage. Native rents are based on a sample of all natives who do not live in group quarters, and are "gross," meaning they include utilities. Gross rent is considered more comparable across households than simple rent since utilities are included in some rents.

We calculate hourly wages as annual wage or salary income divided by the product of usual weekly hours and annual weeks worked, with the latter measured as the midpoint of the categorical variable (except for the 50-52 weeks category, which is assigned a value of 52).

There is a large share of implausibly small hourly wage values: about 2% of workers in the pooled underlying microsamples earn less than half the Federal minimum wage (with wages measured in $2024), typically as a result of reporting low annual income. Meanwhile, about half of the 0.4% of workers calculated to earn more than $300 per hour report working 20 hours per week or less, suggesting possibly considerable errors in usual hours that merit the observations being dropped rather than winsorized. We create two samples based on dropping the lowest 2% and highest 0.5% of hourly wages and dropping observations in next 0.5% if usual weekly hours are 20 or lower. The first sample is created by trimming each year separately, the second by trimming each metro-year separately.

Our analysis is at the metro-year level, using the 164 metropolitan areas (CBSAs) that can be consistently identified from 2021-2024. We aggregate the micro-level outcomes using person weights; for native wages, employment and rents, we first adjust for potentially changing gender, education and age as described in the main text.

To calculate the shares in the shift-share instrument, we pool the 2013-2017 surveys and calculate the national number of immigrants from each of 25 origin countries or regions (see appendix table A1); we then calculate the fraction of each origin's national population that lives in each metro, weighting by person weights and imposing no age restriction. We identify as individual countries those whose number of entries to the US based on our CBP data surged following the COVID-19 pandemic or whose share of the national immigrant population was at least 2%; we group other countries by continent. We then use these distributions to allocate our estimate of national surge migrants from CBP data to metro-years to predict the total number of surge migrants to each metro in each year.

Other data

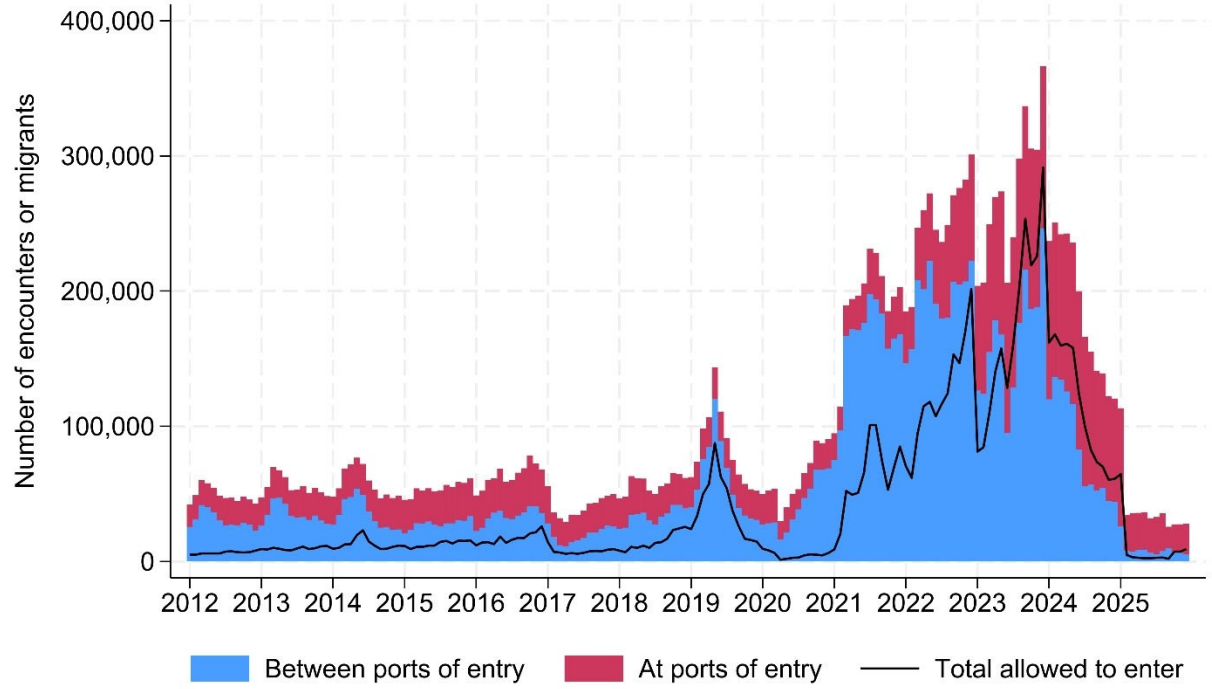
The counts of buslift migrants to various destinations were obtained directly from the Arizona Department of Emergency and Military Affairs and the Texas Division of Emergency Management. Public record requests may be submitted to https://dema.az.gov/resources/public-records-request and https://tdem.texas.gov/public-information-requests.

Population and net domestic in-migration data are vintage 2025 data from the U.S. Bureau of the Census. These data are based on the 2020 Census, administrative data, and ACS data that are more detailed than the publicly available data, allowing the annual flows to be defined from 1st July to 30th June each year. https://www.census.gov/data/tables/time-series/demo/popest/2020s-national-total.html

Data for several variables were acquired via Haver: aggregate employment and average hourly data from the Bureau of Labor Statistics, Current Employment Statistics program; multifamily rent data from Zillow; and GDP data from the Bureau of Economic Analysis. Average hourly wages are deflated using the CPI-U-RS (2024=100); multifamily rents are deflated using the national CPI excluding shelter (2024=100); and GDP is deflated using the national GDP deflator (2024=100).

Figures Appendix

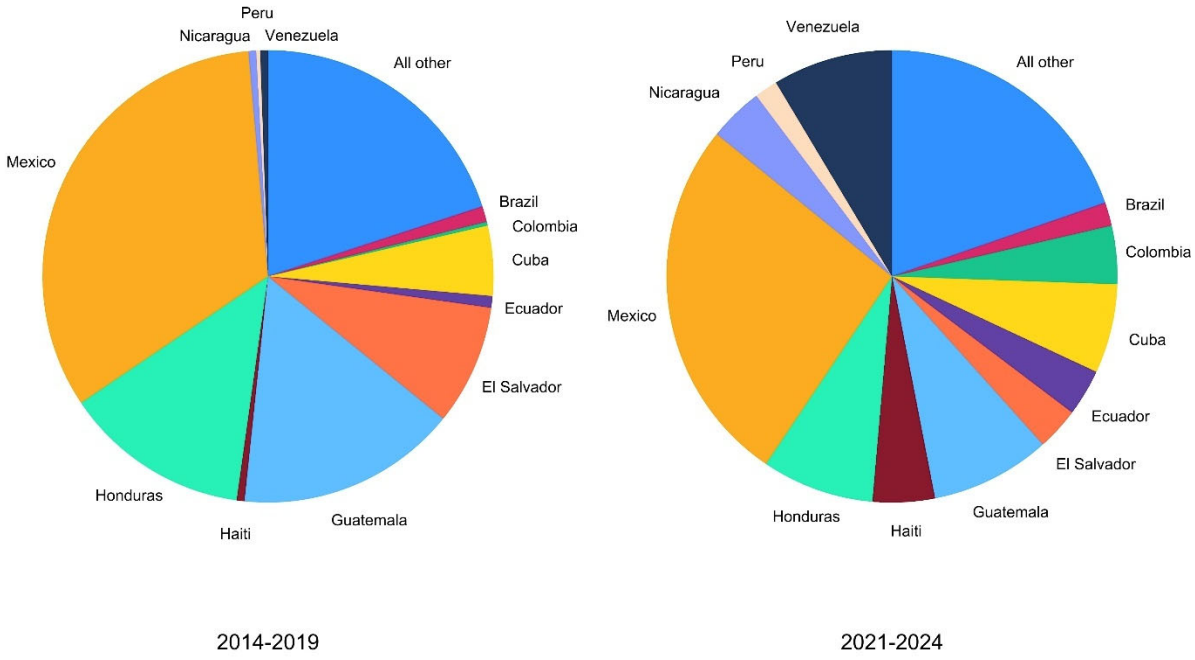
Figure A1: Customs and Border Protection encounters with migrants, between and at ports of entry, and alternative estimate of migrants allowed to enter, 2012-2025



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.

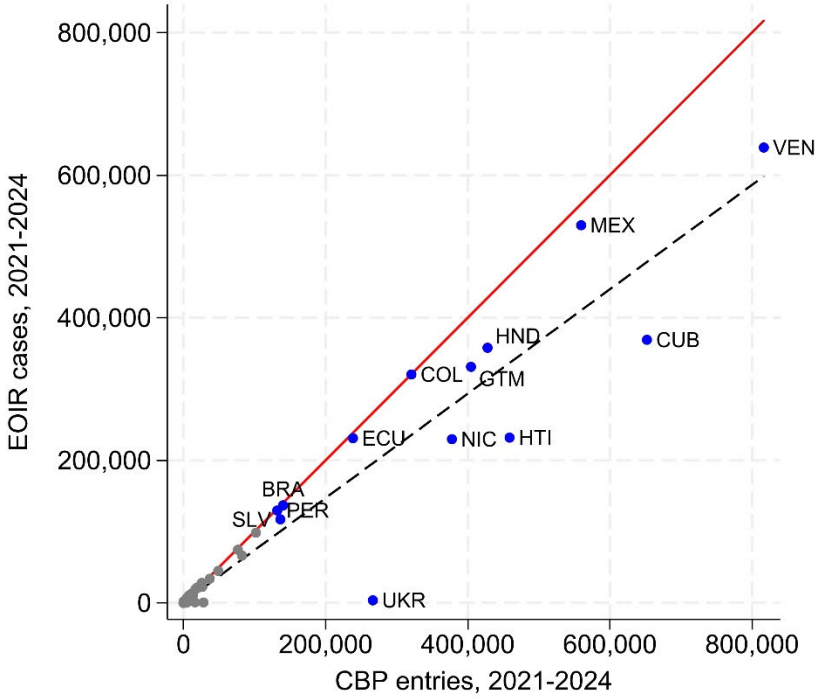
Appendix Figure A1 includes an additional group as allowed to enter: migrants encountered by USBP who received a disposition code of WA/NTA (Warrant of Arrest/Notice to Appear), multiplied by the share of ICE book-outs at the border that were not for removal, by year. The share of ICE book-outs at the border that were not for removal is only available for 2018-2024, so we apply the CY2018 share to earlier years. About three-quarters of ICE book-outs at the border who were released during 2021-2024. Including those migrants as allowed to enter raises the total number of surge migrants who were allowed to enter between January 2021 and December 2024 from 5 million to 5.6 million (and from July 2021 to June 2024, the period on which our estimates focus, from 4.38 million to 4.95 million).

Figure A2: 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.

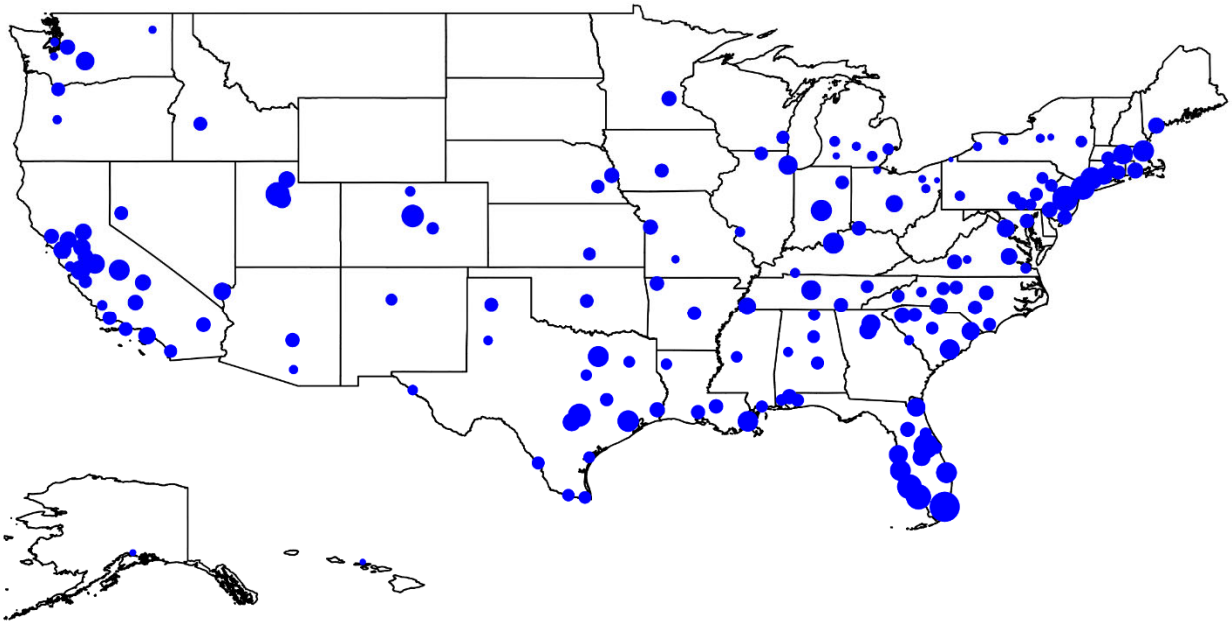
Figure A3: CBP entries and EOIR cases, 2021-2024, by country



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. The red line is the 45° line. The dashed black line is the best linear fit. Its slope is 0.73 (0.01). The 13 labeled countries have the most CBP entries during 2021-2024.

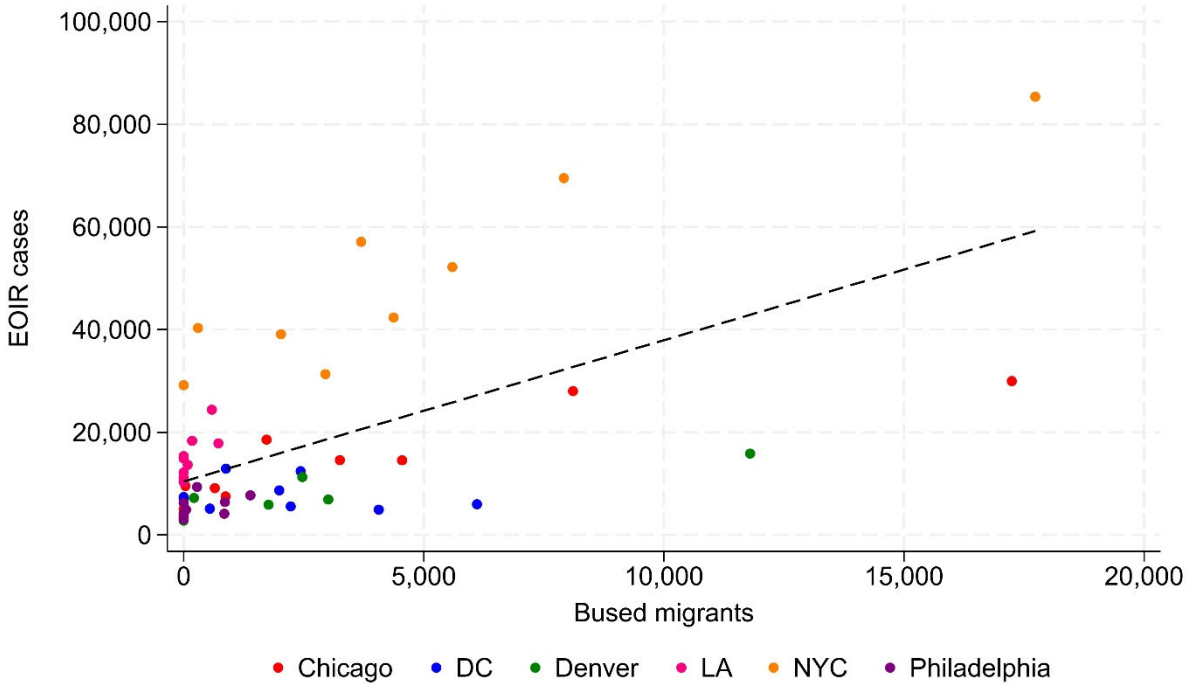
Figure A3 reproduces Figure 4 with a broader measure of CBP entries (which includes USBP encounters with a disposition code of WA/NTA, multiplied by the share of ICE book-outs at the border that were not for removal, by year, and with a disposition code of ADMIT).

Figure A4 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.

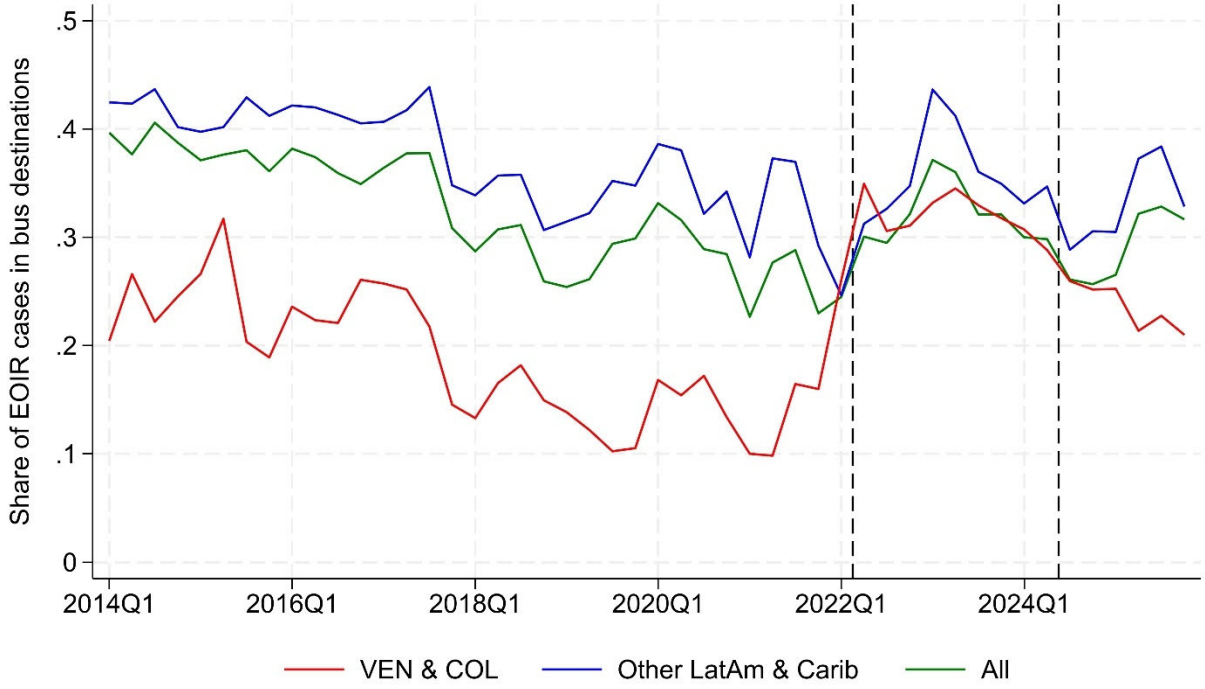
Figure A5: Bused migrants and EOIR cases, 2022Q2-2024Q2



Source: Authors' calculations using data from the U.S. Department of Justice, Executive Office for Immigration Review (EOIR), Arizona Department of Emergency and Military Affairs, and Texas Division of Emergency Management. 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 2.8 (0.9) and an R-squared of 0.38.

Figure A5 shows the relationship between the number of buslift migrants and EOIR cases, by metro destination and quarter.

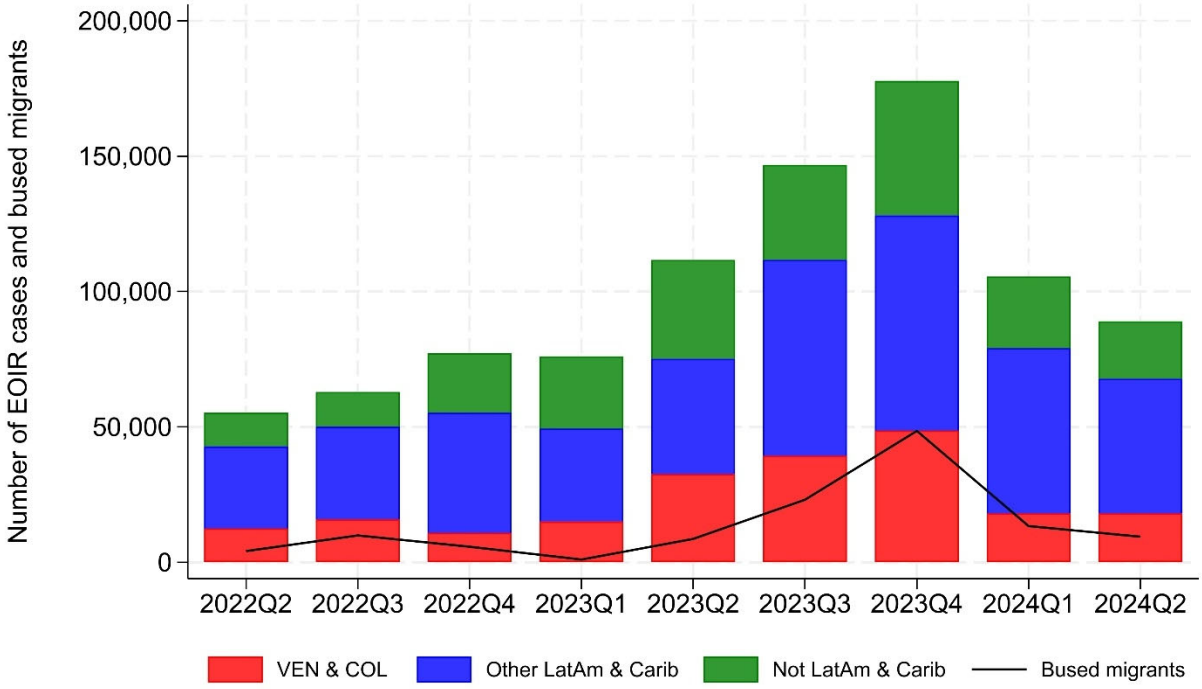
Figure A6: Share of surge migrants in buslift destination metro areas, 2014-2025



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).

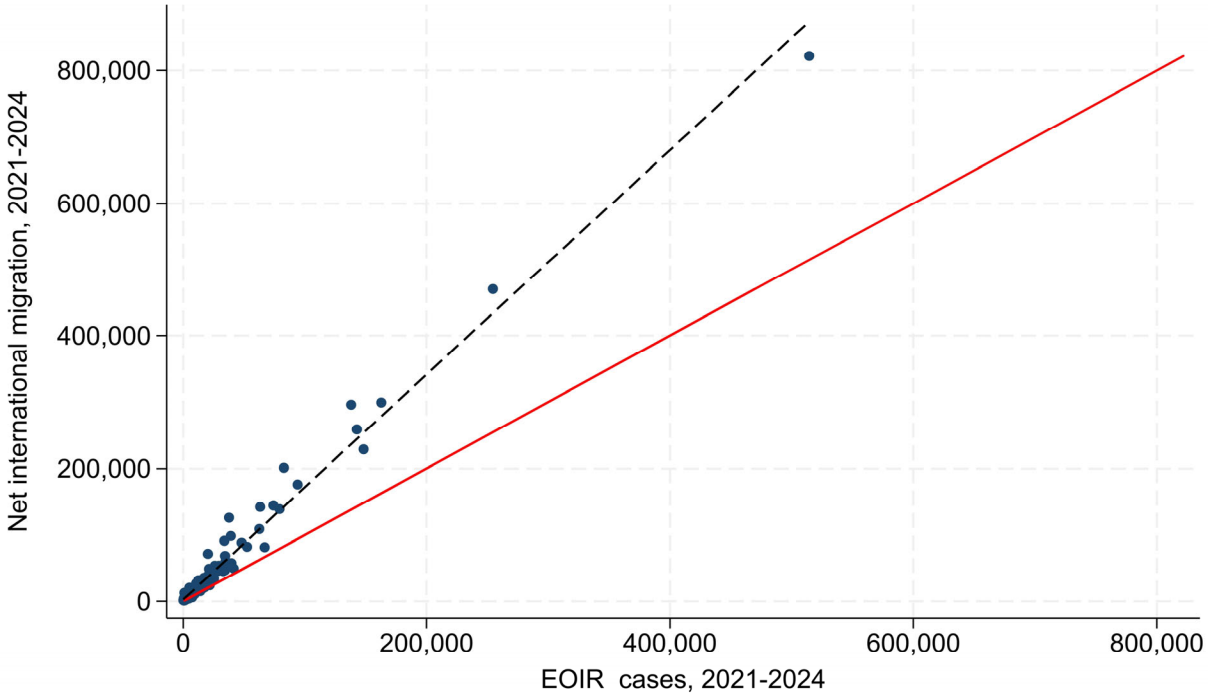
The share of EOIR cases among migrants from Venezuela and Colombia in the six buslift destinations (Chicago, Denver, Los Angeles, New York, Philadelphia, and Washington, DC) rose considerably when the buslift began. The share of EOIR cases in those six destinations for migrants from other Latin American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

Figure A7: Number of EOIR cases and bused migrants in bus destination metro areas, 2022Q2-2024Q2



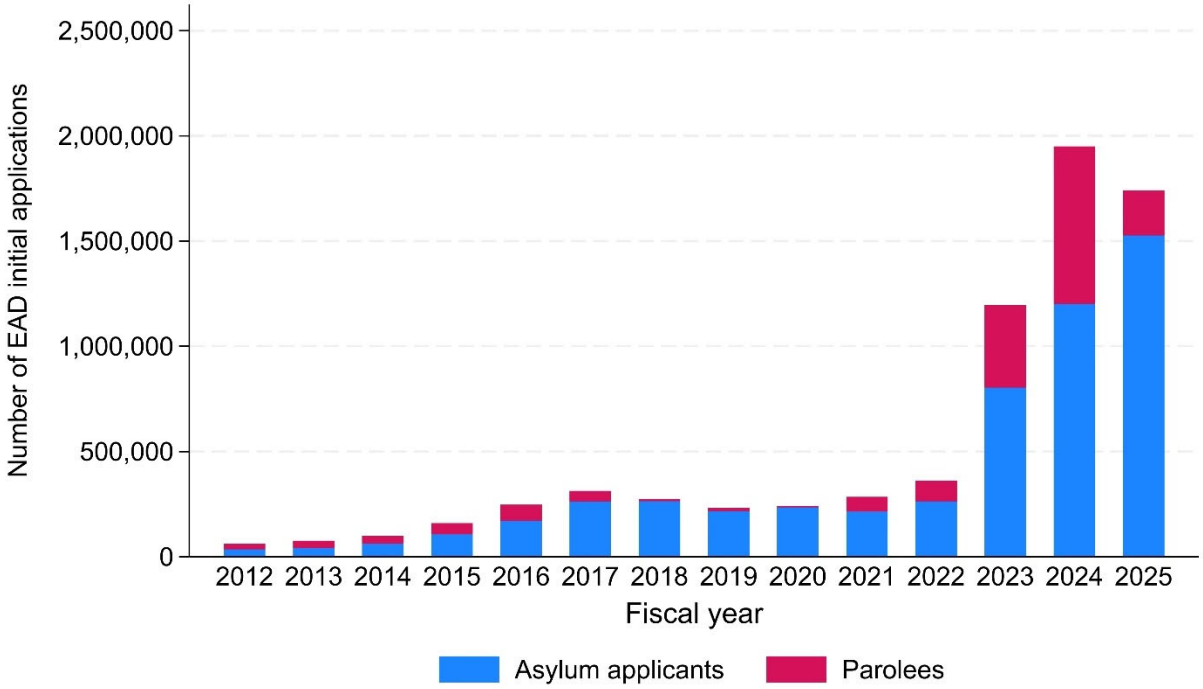
Source: Authors' calculations using data from the U.S. Department of Justice, Executive Office for Immigration Review, and the Texas and Arizona governors' offices. Only non-detained cases in the six bus destination metro areas (Chicago, Denver, Los Angeles, New York, Philadelphia, and Washington, DC)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

Figure A8: 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 are the New York City and Miami areas.

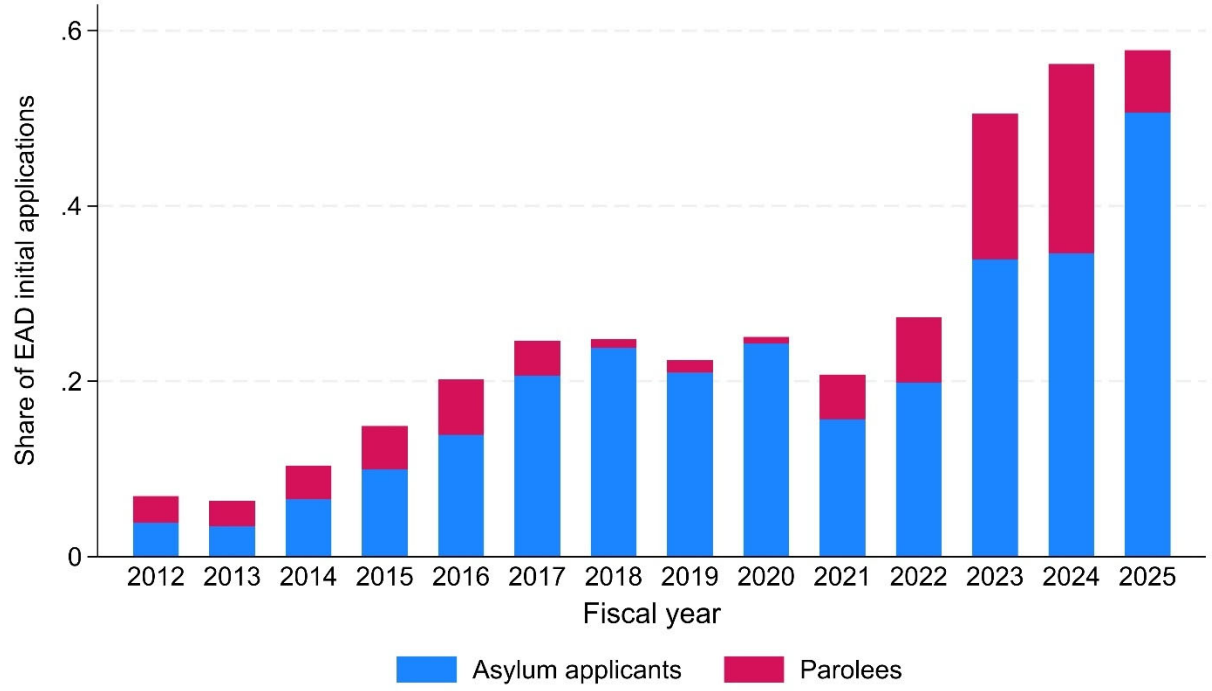
Figure A9: EAD applications by asylum applicants and parolees, FY 2012-2025



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

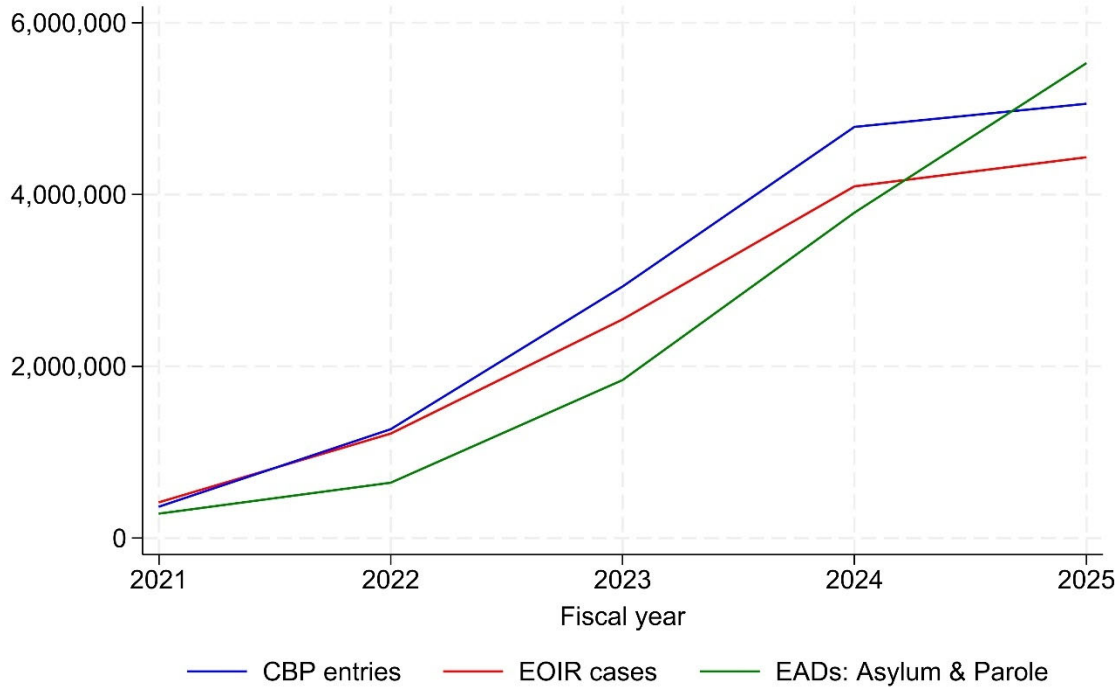
The number of initial applications for an Employment Authorization Document (EAD, or Form I-765) filed by applicants for asylum and migrants admitted to the United States with parole soared as a result of the surge. The share of EAD applications in those two categories rose as well (next figure). For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

Figure A10: 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 A11: Cumulative number of surge migrants in CBP and EOIR data and EAD applications by asylum applicants and parolees, FY 2021-2025



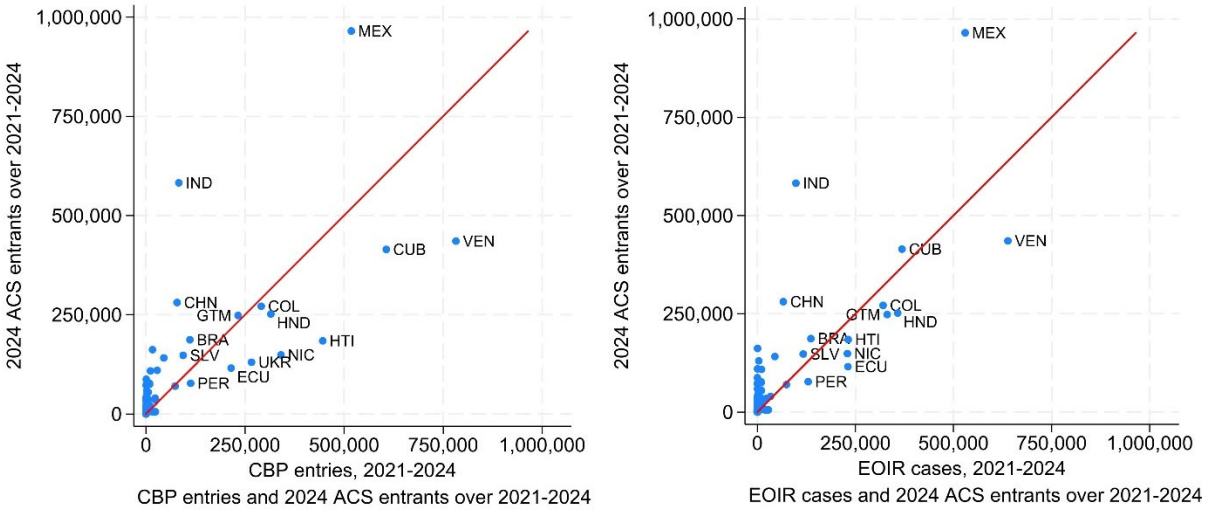
Source: Authors' calculations using data from the U.S. Department of Homeland Security, Customs and Border Protection and U.S. Citizenship and Immigration Services, and U.S. Department of Justice, Executive Office for Immigration Review

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 A11 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.

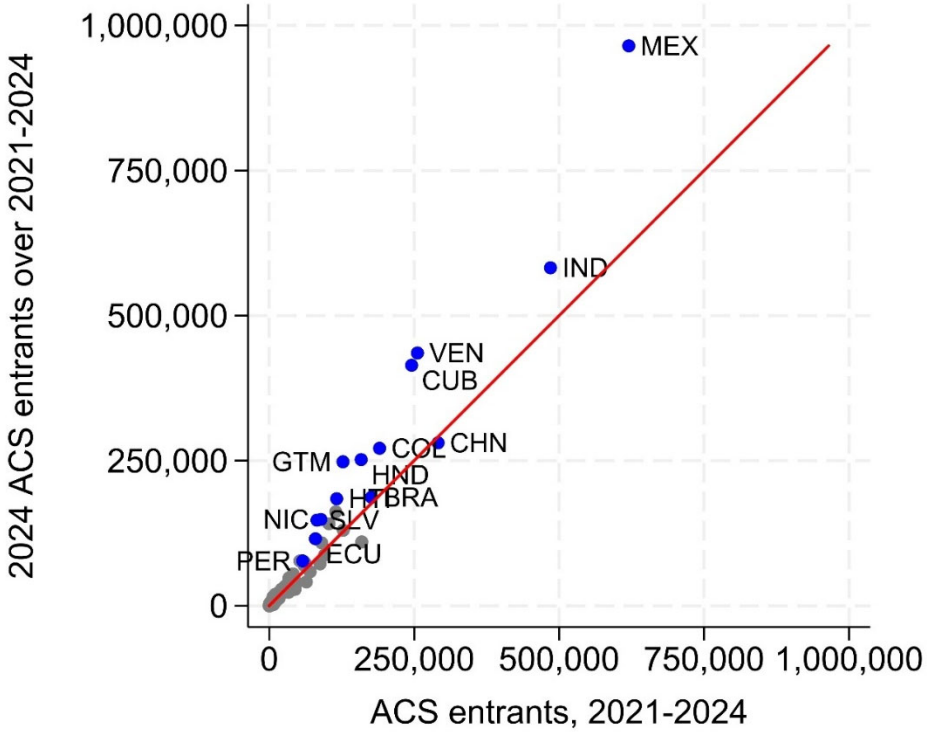
Figure A12: Comparison of number of surge migrants in CBP and EOIR data with number of 2021-2024 entrants in 2024 ACS, by country



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 A12 is the equivalent of Figure 6 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 6). 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 absent visa overstays causing the increase in the cohort size (see Van Hook et al. 2014).

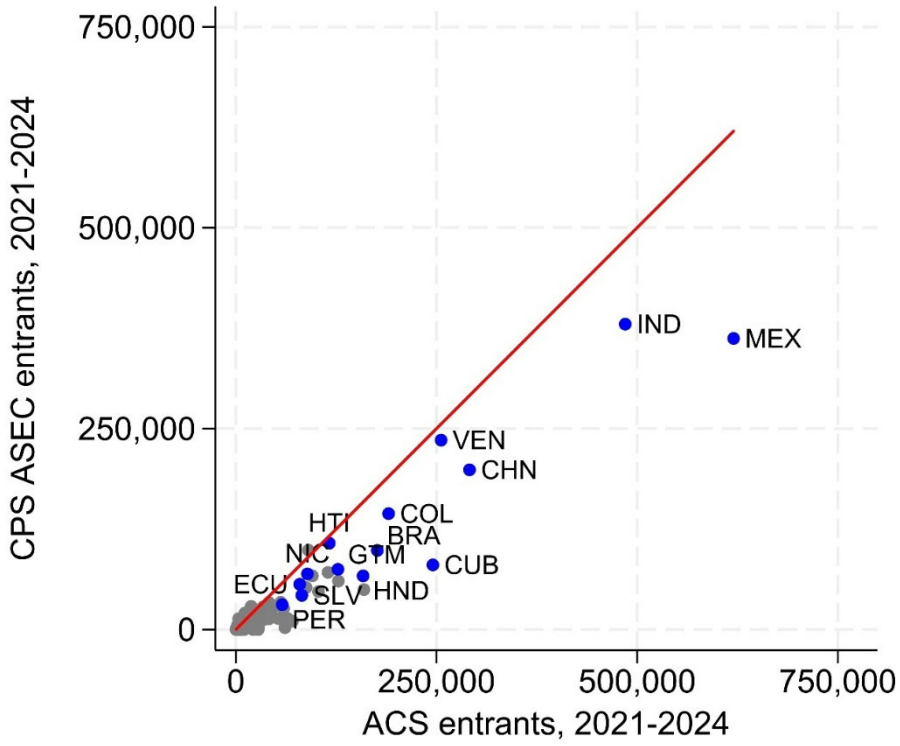
Figure A13: Comparison of number of new entrants in 2021-2024 ACS and number of 2021-2024 entrants in 2024 ACS, by country



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 A13 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 (although we use the 2013-2017 ACS to distribute CBP entries across metro areas).

Figure A14: Comparison of number of new entrants in 2021-2024 ACS and the 2021-2024 CPS ASEC, by country

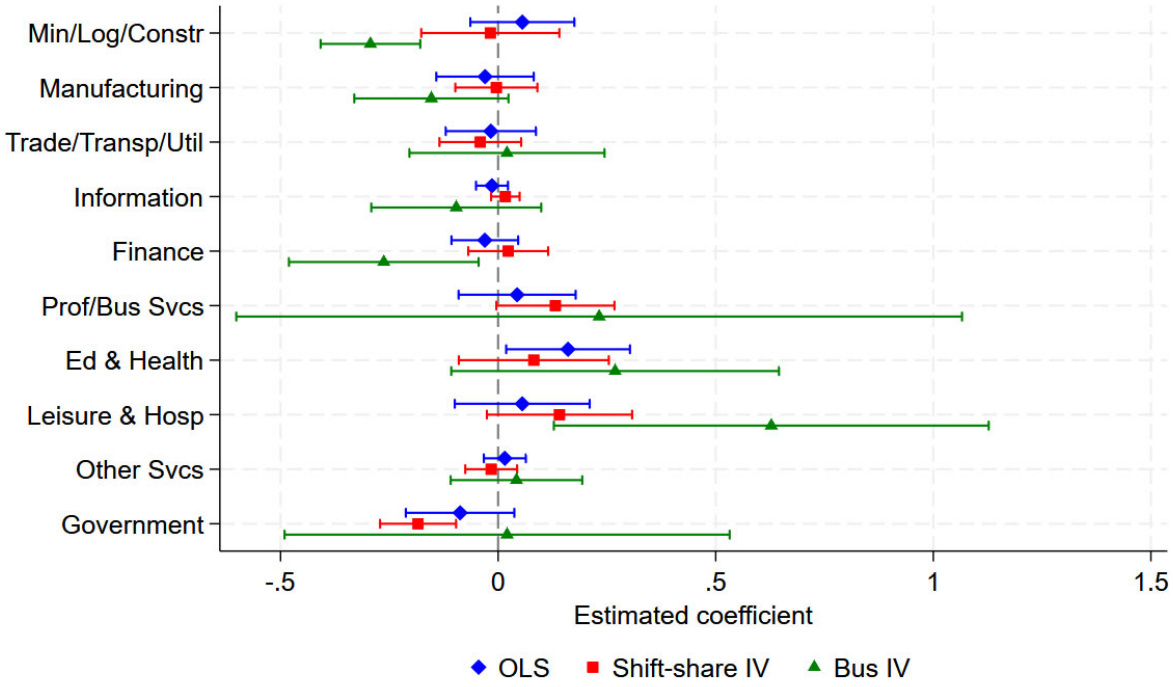


Source: Authors' calculations using data from the 2021-2024 American Community Survey (ACS) on non-U.S. citizens whose residence one year ago was abroad and from the same using data from the 2021-2024 Current Population Survey Annual Social and Economic Supplements (CPS ASEC). The ACS and CPS ASEC person 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 A14 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 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).

Figure A15: 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 as a share of the population. 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. Robust 95% confidence intervals shown.

Figure A15 is for comparison to the estimates in Figure 7(a) in Wilson and Zhou (2026). They similarly find that the largest relative effect is in leisure and hospitality. While they do not report results for the government sector, we find a negative relative effect in government when using the IV shift-share, a surprising result given that the surge migrants increased demand for many local government services.

Tables Appendix

Table A1: 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.

Notes: 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 A2: 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	1.1	2.4	0.3
China	0.2	0.0	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	0.1	0.3	0.4
Other Asia	0.1	0.0	0.1	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	6.3	1.0	3.4
Nicaragua	6.2	14.7	10.8	1.6
Cuba	20.8	41.4	25.3	11.8
Dominican Republic	0.3	0.0	0.7	0.2
Haiti	15.1	2.6	11.4	20.9
Brazil	0.5	2.4	0.2	0.2
Colombia	6.4	5.1	7.5	4.4
Ecuador	5.9	1.4	3.4	8.1
Peru	1.0	0.4	2.5	0.4
Venezuela	27.8	19.9	23.6	28.3
Ukraine	4.0	2.3	8.3	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 changes in unadjusted, untrimmed, log native wages.

Table A3: 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	3.5	-0.1	0.3	9.9
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	0.2	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	0.2	0.2	0.0	0.2
Guatemala	2.1	0.9	0.1	4.7
Honduras	2.7	5.4	0.7	2.8
Nicaragua	6.0	13.5	11.7	1.6
Cuba	29.4	47.7	35.8	17.2
Dominican Republic	0.1	0.0	0.2	0.0
Haiti	15.6	2.8	11.8	22.4
Brazil	0.5	2.7	0.2	0.2
Colombia	4.2	3.8	5.1	3.0
Ecuador	2.0	0.5	1.1	3.0
Peru	0.5	0.2	1.4	0.2
Venezuela	32.6	21.6	28.8	34.1
Ukraine	0.1	0.2	1.4	0.0
Total	100.0	100.0	100.0	100.0

Source: Authors' calculations.

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 changes in unadjusted, untrimmed, log native wages.

Table A4: IV effect of surge migration on log wage for different wage measures, 2021-2024

Micro worker sample	Wage measure	Coefficient
All	Mean worker log hourly wages	0.8***
		(0.3)
All, national trim	Mean worker log hourly wages	0.5*
		(0.3)
All, metro trim	Mean worker log hourly wages	0.7**
		(0.3)
All	Mean worker log weekly wages	1.3***
		(0.3)
Non-farm, private	Log(weekly wage bill/total weekly hours)	0.4
		(0.7)

Source: Authors' calculations.

Notes: IV coefficients on surge migrants/population using shift-share instrument. The outcome is the 2021-2024 change in wages not adjusted at micro level. All regressions include a control for the log native wage change 2016-2019. All micro samples are for workers age 16-64. Coefficients are multiplied by 100. 164 observations. Trimming involves dropping the bottom 2%, top 0.5% and part-time workers in the next 0.5%, at the worker level. "National trim" indicates that the trimming is applied to pooled metros for each year, while "Metro trim" indicates that it is applied separately by metro and year. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table A5: IV effect of predicted surge migration on outcomes (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	0.1*	0.1	0.1***	0.07*	0.04	-0.2
	(0.04)	(0.10)	(0.04)	(0.03)	(0.04)	(0.3)
						[p=0.63]
Total employment: buslift metro indicator	--	--	--	--	--	-1.2
						(0.7)
						[p=0.16]
GDP	0.6*	1.8**	0.6*	0.05	0.03	-2.2***
	(0.2)	(0.6)	(0.3)	(0.1)	(0.07)	(0.4)
						[p=0.12]
Average wage	-0.6	-1.0	-0.8	-0.3	-0.4*	2.4
	(0.4)	(1.0)	(0.5)	(0.2)	(0.1)	(3.0)
						[p=0.88]
Rent	0.5***	3.9***	-0.2	-0.5*	-0.2	1.5
	(0.1)	(0.7)	(0.1)	(0.2)	(0.1)	(2.6)
						[p=0.63]
B. ACS-constructed and domestic migration variables:						
Native wage No trim	0.4***	1.7***	0.2	-0.2	0.0	-2.5***
	(0.1)	(0.6)	(0.2)	(0.2)	(0.1)	(0.5)
						[p=0.07]
Native wage National trim	0.3***	0.9	0.8***	-0.4	-0.1**	-1.8**
	(0.1)	(0.5)	(0.3)	(0.2)	(0.1)	(0.7)
						[p=0.37]
Native wage Weekly, no trim	0.5***	2.3***	0.4	-0.4	-0.0	-2.4***
	(0.2)	(0.8)	(0.3)	(0.3)	(0.1)	(0.6)
						[p=0.18]
Native employment rate	0.1	0.4	-0.1	-0.1	-0.0	-1.1**
	(0.1)	(0.3)	(0.1)	(0.1)	(0.1)	(0.4)
						[p=0.15]
Native rent	0.4**	1.2	1.4**	-0.8	0.0	-1.9
	(0.2)	(0.8)	(0.7)	(0.7)	(0.2)	(1.7)
						[p=0.49]
Net domestic in-migration	-0.4***	-0.5***	-0.3***	-0.4***	-0.2***	0.0
	(0.1)	(0.1)	(0.1)	(0.1)	(0.0)	(0.3)
						[p=0.99]
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 surge migrant flows as predicted by the shift-share variable (columns 1-5) or the buslift instrument(s) (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. Where the outcomes are in logs, the coefficients and standard errors are multiplied by 100, while in row 2 these numbers are

multiplied by 10,000. In column 6, the pair of results in rows 1-2 are from the same regression. In row 1, the measure of surge migration is in levels; in all other rows, the measures of surge migration are relative to the initial population (in %). The regressions in rows 1-2 also control for the 2019 population, the 2016-2019 change in employment, and the 2016-2019 share of employment in each supersector (excluding government). The regressions in all other rows except net domestic migration control for the change in the (log of the) variable indicated over 2016-2019; in the net domestic migration regressions the control is the three-year sum of domestic in-migration divided by population. The number of observations is 164 in columns 1-5, except for GDP (160) and average rent (92 in panel A); and 30 in column 6. Wages, employment and rent in panel B are adjusted for gender, age and education. Trimming involves dropping the bottom 2%, top 0.5% and part-time workers in the next 0.5%, at the worker level. "National trim" indicates that the trimming is applied to pooled metros for each year. Parentheses contain robust standard errors (and asterisks refer to these); brackets contain Webb wild bootstrap p-values based on 9,999 repetitions (column 6). *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A6: IV effect of surge migrants on native log wages adjusted for gender and age, by education of natives

	2021-2024		2023-2024	
	No trim	National trim	No trim	National trim
	(1)	(2)	(3)	(4)
High school dropouts	1.9	2.6*	1.5	1.0
	(1.1)	(1.5)	(4.8)	(2.5)
			[p=0.79]	[p=0.70]
High school graduates	0.7	0.4	-3.3	-3.9***
	(1.3)	(1.1)	(2.4)	(1.1)
			[p=0.25]	[p=0.20]
Some college	1.1**	0.7	-4.9***	-2.3*
	(0.5)	(0.5)	(1.8)	(1.2)
			[p=0.43]	[p=0.53]
College graduates	1.1**	1.0**	-0.8	-0.8
	(0.5)	(0.4)	(1.1)	(0.5)
			[p=0.56]	[p=0.32]
Observations	164		30	
Instrument	Shift share		Buslift	

Source: Authors' calculations.

Notes: Unweighted IV coefficients on surge migrant flows/population (in %), multiplied by 100. The outcomes are differenced average native adjusted log real hourly wages in the ACS. Trimming involves dropping the bottom 2%, top 0.5% and part-time workers in the next 0.5%, at the worker level. "National trim" indicates that the trimming is applied to pooled metros for each year (columns 2 and 4). All regressions include the three-year 2016-2019 changes in the (unadjusted) native log wage. Parentheses contain robust standard errors (and asterisks refer to these); brackets contain Webb wild bootstrap p-values based on 9,999 repetitions (columns 3-4). *** p<0.01, ** p<0.05, * p<0.10.

Table A7: 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 Trend control	-0.9*** (0.2)	-0.9*** (0.3)	-1.1*** (0.2)	-0.7*** (0.2)	-0.6 (0.3)	
IV Trend control	-1.2*** (0.2)	-0.9*** (0.2)	-1.5*** (0.2)	-1.4*** (0.2)	-1.6*** (0.3)	0.0 (0.2) [p=0.99]
IV No trend control	-0.6 (0.4)	-0.1 (0.6)	-0.7 (0.5)	-1.1*** (0.3)	-1.5*** (0.4)	-0.4 (0.4) [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 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. "Trend control" refers to the sums of domestic in-migration (divided by initial population) for 2016-2019. Parentheses contain robust standard errors (and asterisks refer to these); brackets contain Webb wild bootstrap p-values based on 9,999 repetitions (column 6). *** p<0.01, ** p<0.05, * p<0.10

Table A8: Shift-share IV effects of surge migration, instrument computed without Ukraine, Cuba, or Haiti

	2021-2024	2021-2022	2022-2023	2023-2024
	(1)	(2)	(3)	(4)
Panel A: aggregate variables				
Total employment	0.5***	0.3	0.7***	0.5***
	(0.1)	(0.2)	(0.2)	(0.1)
GDP	1.6**	2.7**	2.0*	0.3
	(0.5)	(0.9)	(0.9)	(0.6)
Average wage	-1.8*	-2.2	-2.8*	-0.7
	(0.7)	(1.5)	(1.1)	(0.9)
Rent	1.4*	7.2***	-1.5*	-2.2***
	(0.7)	(1.1)	(0.7)	(0.6)
Panel B: ACS-constructed variables				
Native wage	1.1***	3.5***	1.0	-0.9
No trim	(0.4)	(0.8)	(0.8)	(0.8)
Native wage	0.9**	1.9**	2.7***	-1.7**
National trim	(0.4)	(0.9)	(0.8)	(0.7)
Native employment rate	0.3	0.8	0.0	-0.6
	(0.2)	(0.6)	(0.4)	(0.5)
Native rent	0.5	1.6	3.5*	-2.7
	(1.0)	(1.2)	(1.9)	(1.8)

Source: Authors' calculations.

Notes: Coefficients are based on the row 2 "IV Trend control" specifications of tables 3 (total employment), 4 (GDP), 5 (average wage), 6 (native wage), 7 (native employment rate), 9 (native rent), and 10 (rent), with the shift-share instrument adapted to exclude Ukraine, Cuba, and Haiti. Native wages, employment rate, and rent are adjusted at the worker level for gender, age, and education (though the trend control is not). Coefficients ×100 except total employment and native employment rate. The number of observations is 164, except for GDP (160) and average rent (92 in panel A). Trimming involves dropping the bottom 2%, top 0.5% and part-time workers in the next 0.5%, at the worker level. "National trim" indicates that the trimming is applied to pooled metros for each year. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table A9: IV effects of surge migrants on native log wages adjusted for gender, age, and education, regressions weighted using 2019 population

	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***	0.3	1.0***	0.5***	0.3	0.1
	(0.1)	(0.2)	(0.3)	(0.1)	(0.2)	(0.1)
						[p=0.68]
GDP	1.3*	2.4***	1.3	-0.1	0.1	-1.5
	(0.6)	(0.6)	(1.2)	(0.2)	(0.3)	(0.9)
						[p=0.25]
Average wage	-1.7**	-0.8	-2.9***	-1.6***	-1.9***	-0.1
	(0.6)	(1.0)	(0.5)	(0.3)	(0.6)	(2.3)
						[p=0.97]
Rent	2.8***	8.9***	-0.6	-1.9***	-1.7*	3.6
	(0.3)	(0.5)	(0.5)	(0.5)	(0.9)	(3.8)
						[p=0.41]
B. ACS-constructed variables:						
Native wage No trim	1.1**	2.8***	0.6	-0.2	0.1	-1.8**
	(0.5)	(0.5)	(0.5)	(0.3)	(0.4)	(0.8)
						[p=0.08]
Native wage National trim	0.8*	1.3*	2.3**	-0.8**	-0.8*	-0.9
	(0.5)	(0.7)	(0.9)	(0.3)	(0.4)	(0.7)
						[p=0.37]
Native employment rate	0.1	0.3	-0.1	-0.1	0.1	-1.1**
	(0.1)	(0.3)	(0.1)	(0.2)	(0.3)	(0.5)
						[p=0.07]
Native rent	1.4	0.4	4.3**	0.2	0.7	-0.2
	(1.0)	(1.1)	(1.9)	(0.8)	(0.7)	(1.2)
						[p=0.87]
Instrument	Shift share	Shift share	Shift share	Shift share	Shift share	Buslift

Source: Authors' calculations.

Notes: Coefficients on surge migrant flows (row 1) or surge migrant flows/population (in %) from IV regressions controlling for the 2016-2019 change in the outcome, weighted by 2019 population. Native wages, employment rate, and rents are adjusted at the worker level for gender, age, and education (though the trend control is not). Coefficients are multiplied by 100 except for total employment and native employment rate. The number of observations is 164 in columns 1-5, except for GDP (160) and average rent (92 in panel A, and 30 in column 6). Trimming involves dropping the bottom 2%, top 0.5% and part-time workers in the next 0.5%, at the worker level. "National trim" indicates that the trimming is applied to pooled metros for each year. Parentheses contain robust standard errors (and asterisks refer to these); brackets contain Webb wild bootstrap p-values with 9999 repetitions (column 6). *** p<0.01, ** p<0.05, * p<0.10.

Table A10: IV effects of surge migrants on unadjusted native outcomes

	2021–2024	2021–2022	2022–2023	2023–2024	2023–2024	2023–2024
	(1)	(2)	(3)	(4)	(5)	(6)
Native wage No trim	1.2***	3.9***	1.0	-1.1	0.3	-0.2
	(0.4)	(0.8)	(0.8)	(0.7)	(0.5)	(0.5)
						[p=0.66]
Native wage National trim	0.9***	2.1***	3.3***	-1.9***	-0.9	0.3
	(0.4)	(0.7)	(0.9)	(0.6)	(0.6)	(0.4)
						[p=0.56]
Native employment rate	0.2	0.8	-0.3	-0.4	0.1	-0.5
	(0.1)	(0.5)	(0.4)	(0.5)	(0.4)	(0.3)
						[p=0.14]
Native rent	1.5**	1.8	5.4***	-1.8	1.1	-2.7
	(0.6)	(1.4)	(1.6)	(1.7)	(1.0)	(1.7)
						[p=0.58]
Observations	164	164	164	164	30	30
Instrument	Shift share	Shift share	Shift share	Shift share	Shift share	Buslift

Source: Authors' calculations.

Notes: Coefficients on surge migrant flows/population (in %) from IV regressions controlling for the 2016-2019 change in the outcome, with no prior micro-level adjustment of the outcome. Wage and rent coefficients are multiplied by 100. Trimming involves dropping the bottom 2%, top 0.5% and part-time workers in the next 0.5%, at the worker level. "National trim" indicates that the trimming is applied to pooled metros for each year. Parentheses contain robust standard errors (and asterisks refer to these); brackets contain Webb wild bootstrap p-values with 9,999 repetitions (column 6). *** p<0.01, ** p<0.05, * p<0.10.

Table A11: Effect of surge migrants on immigrant 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	0.4	-5.0	9.2**	-2.8	-0.7	
Trend control	(0.9)	(3.1)	(3.9)	(2.2)	(2.1)	
IV	0.1	-2.5	4.4	-0.7	-1.6	-1.9
Trend control	(0.7)	(2.7)	(3.1)	(2.7)	(1.6)	(1.5)
						[p=0.60]
IV	0.1	-2.5	4.4	-0.7	-1.8	-1.7
No trend	(0.7)	(2.7)	(3.1)	(2.7)	(1.5)	(1.4)
						[p=0.57]
IV	-0.2	-3.8	3.8	-0.1	-2.4*	-0.9
Trend control	(0.8)	(2.6)	(2.7)	(3.0)	(1.3)	(1.3)
National trim						[p=0.64]
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 immigrant adjusted log real hourly wage in the ACS. The trend control is the 2016-2019 change in the (unadjusted) native log wage. Trimming involves dropping the bottom 2%, top 0.5% and part-time workers in the next 0.5%, at the worker level. "National trim" indicates that the trimming is applied to pooled metros for each year. Parentheses contain robust standard errors (and asterisks refer to these); brackets contain Webb wild bootstrap p-values based on 9999 repetitions (column 6). *** p<0.01, ** p<0.05, * p<0.10.

Literature Review Appendix

The working papers of Wilson and Zhou (2026) and Clemens et al. (2026), which appeared before our paper, both examine the impact of the surge migration on the economy, examining many of the outcomes we do. Both papers examine the impact on native wages and employment rates, while Wilson and Zhou examine aggregate wages, employment, and rents, and Clemens et al. examine GDP. Neither studies the impact on domestic migration nor on native rents. Both examine several outcomes we do not, such as personal income, government transfer receipts and house prices (Wilson and Zhou) or capital income (Clemens et al.). Wilson and Zhou extend the data to June 2025 to examine the slowdown period separately.

The data sources of the three papers are the same for most of the common outcomes, though the construction of some variables differs. All three use EOIR data to measure surge migration (Wilson and Zhou winsorize the top 1% of values), but while we use gross new cases as our key covariate, the other papers calculate net migration by assuming that cases resolved with an order for the individual or family to leave the United States led the individuals concerned to do so and combining this with other estimates of entries and exits. Further, while we include all new EOIR cases for non-detained persons, Clemens et al. focus on asylum seekers, while Wilson and Zhou principally use cases that include someone they consider a likely worker, and scale by employment rather than population. Clemens et al. use EOIR data alone to construct the shift-share instrument, while Wilson and Zhou use the 2015-2019 ACS for shares as we do (but we use 2013-2017); we use CBP data for the shifts, whereas Wilson and Zhou and Clemens et al. use their measures of net EOIR flows for the shifts. Clemens et al. winsorize the top 1% of instrument values. For aggregate wages (weekly) and employment, we use CES data while Wilson and Zhou use the QCEW; we both use Zillow to measure aggregate rents. For native employment and wages, all three papers use the ACS: Wilson and Zhou examine the impact on hourly wages, with the top 1% winsorized, while Clemens at al. study annual wage and salary income of the non-self employed; neither paper adjusts the outcomes for individual characteristics. We use hourly wages, treating outliers in three different ways, compare with weekly wages, and adjust the outcomes. While we use 164 metro areas for cross-section variation, Clemens et al. (2026) use 451 larger commuting zones, while Wilson and Zhou (2016) use 712-721 commuting zones in most of their analysis, but also provide results for aggregate outcomes based on 348 metro areas.

All three papers use a shift-share instrument for identification, with Wilson and Zhou being the first to circulate a paper doing so; we follow their lead, but note that earlier work by Cornaggia et al. (2025) also uses a shift-share instrument and EOIR data. However, due to what we think is a better choice of data to construct the instrument (the CBP data on entries), our instrument is in another respect less sophisticated, since we do not leave out one metro in its construction; neither do we leave out one metro and origin, as Wilson and Zhou do for the first time in the literature. Unlike the other papers, we additionally use bus passengers as an instrument. Wilson and Zhou and Clemens et al. employ weighted regressions, while we focus on unweighted regressions but provide weighted results in an appendix table. Wilson and Zhou study 2021-2023 for the surge period, while Clemens at al. study both 2021-2023 and 2021-2024 but emphasize the results from the former.

Appendix-Only References

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