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Economic and Magnet Effects of Work Authorization for Asylum Seekers: Descriptive Evidence from the United States

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Abstract

Legalizing work by asylum seekers is politically contested. Many states view work permits as a necessary expedient for self-reliance. Others limit or ban asylum seekers' labor, citing concerns about labor-market competition for natives, fiscal drain, and 'magnet' effects on subsequent asylum inflows. Empirical tests of these effects are numerous in Europe, rare in the United States. We use full-universe anonymized court records that precisely locate asylum applicants' residences to test the effects of the post-2021 surge in asylum-seeker arrivals on native labor-market outcomes, and on fiscal and economic-growth indicators at the local level. Using a nationality-based shift-share instrument across commuting zones, we find that an asylum-seeker inflow equal to 1% of local population raises incumbent employment by 2.8 percentage points, wages by 6.3%, and local real GDP by approximately 5.5%, while reducing unemployment and means-tested public-benefit reliance. The result is consistent with asylum seekers' labor acting as a strong complement to native labor and native capital in the production process due to task specialization. It is also in line with independent estimates of macroeconomic impacts from general-equilibrium models. We find no relationship between major changes in the number of asylum applications (past collapses and recent surges alike) and changes in US employment-authorization policy for asylum seekers. The evidence supports a substantial stimulus to local economies from asylum seekers' labor, and shows no important role for formal work authorization in asylum seekers' migration decisions.

JEL Codes: J61, J15, R23, H53, K37
Keywords: asylum, employment, authorization, refugees, labor markets, magnet, work permit, labor, integration

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1 Introduction

Work authorization for asylum seekers can affect destination-country economies in two key ways. It can add to the supply of labor in countries where migrants have already arrived, and in principle it could create an incentive for more people to migrate and seek asylum. Recent literature, largely European, has converged on modest or positive average effects of refugee and asylum inflows on native outcomes and on small or null effects of work-authorization policy on further asylum flows (Brell et al. 2020; Fasani et al. 2021). But key questions remain open: estimates of native labor-market effects vary widely across settings and methods, and these effects have been tested almost exclusively outside the United States.

These questions now carry direct policy weight on both sides of the Atlantic. Across Europe, governments are investing in language training, in initial placement in high-employment labor markets, and in some cases in outright reforms to shorten or remove employment bans, all aimed at accelerating asylum seekers' labor-market integration (Foged et al. 2024; Fasani et al. 2022). In the United States, the direction of recent policy has been the opposite. In February 2026, the Department of Homeland Security proposed a rule to substantially restrict employment authorization for asylum seekers, citing displacement, productivity, and magnet-effect concerns.[1] The US is the single largest destination for asylum seekers in the world, but credible evidence on these claims, for the US specifically, has been sparse.

In this paper, we consider the economic effects of asylum seekers' labor on local economies in the United States, and the hypothesis that employment authorization acts as a 'magnet' for migrants to seek asylum in the United States. We use anonymized data on the universe of asylum seekers in US immigration court proceedings, including each individual's nationality, date of entry, and residential ZIP code at case filing. The micro-geography of that universe allows us to estimate local-area treatment effects that the aggregate-flow literature cannot. For the economic-effects question, we estimate long-difference, instrumental-variable regressions across US commuting zones during the 2021–2023 window of rising net inflows, exploiting a shift-share instrument that interacts pre-surge (2019) nationality-by-location settlement shares with leave-one-out national asylum flows; we decompose the implied GDP effect into labor and capital components and benchmark the total against projections from the Congressional Budget Office and the Penn Wharton Budget Model. For the magnet question, we turn to historical filing data from the 1994–1995 US reform (the US government's principal cited precedent), nationality-level decomposition of the 2021–2024 surge, and an administrative claim that high rates of work-permit receipt among denied asylum applicants demonstrate a magnet effect. In each case we test the patterns that a common Employment Authorization

[1] Dept. of Homeland Security, "Employment Authorization Reform for Asylum Applicants", 91 Fed. Reg. 8616, Feb. 23, 2026.

Document (EAD) incentive would produce against the patterns observed in the data.

The findings are consistent across margins. An asylum inflow equal to 1% of commuting-zone population raises incumbent employment by 2.8 percentage points, wages by 6.3%, and local real GDP by approximately 5.5%. Each asylum seeker is associated with roughly 1.1 additional jobs for incumbent workers (citizens and pre-surge non-citizens taken together), and the residual share of GDP (capital and other non-labor income) rises in proportion to labor income. The unemployment rate falls, and reliance on means-tested public benefits declines. On the magnet side, the post-1994 decline in affirmative filings that the proposed rule cites as precedent reflects the end of Central American civil wars and legal deadlines, not EAD policy: once Central American nationalities are set aside, US filings tracked contemporaneous European filings closely over the same period, reflecting worldwide trends associated with the winding down of the Cold War. The 2021–2024 surge is concentrated in a small set of crisis-origin nationalities with historically *above*-average grant rates, and its nationality-specific timing aligns with origin-country events and regional route changes: a pattern hard to attribute to US employment-authorization rules, which have not meaningfully changed. An administrative statistic cited as evidence for a magnet effect, that the great majority of denied asylum applicants had previously received work permits, turns out to be a mechanical byproduct of multi-year adjudication backlogs. A queue that holds cases for years will hand work permits to most applicants, eligible and ineligible alike, before any decision is reached.

The paper contributes to three bodies of evidence. First, to the literature on the labor-market effects of asylum-seeker and refugee inflows on host economies. The existing evidence, spanning quasi-experimental studies of refugee dispersal in Europe, Syrian refugees in Turkey and Jordan, and Venezuelan refugees in Colombia, has generally found small or positive effects on natives' wages and employment, sometimes with displacement of informal workers offset by formal-sector gains.[2] Our estimates extend this body to the United States during the largest single asylum episode in its modern history. They also reach a second literature on the productivity and aggregate effects of asylum and other forced-migration inflows (d'Albis et al. 2018; Maffei-Faccioli and Vella 2021; Peters 2022; Becker et al. 2020; Moser et al. 2014). Our GDP and residual-share results sit in the positive range of that literature.

[2] The refugee-specific literature that underlies this summary falls into three groups. 1) Cross-country surveys and meta-analyses of the labor-market effects of forced displacement (Brell et al. 2020; Becker and Ferrara 2019; Verme and Schuettler 2021). 2) Studies of the labor-market effects of refugee and asylum-seeker arrivals on host-country natives, holding work-authorization policy essentially fixed (Foged and Peri 2016; Peri and Yasenov 2019; Clemens and Hunt 2019; Del Carpio and Wagner 2015; Tumen 2016; Ceritoğlu et al. 2017; Akgündüz et al. 2018, 2023; Altındağ et al. 2020; Aksu et al. 2022; Fallah et al. 2019; Calderón-Mejía and Ibáñez 2016; Kreibaum 2016; Loschmann et al. 2019). 3) Studies of the effects of granting or restricting legal employment authorization to refugees and asylum seekers (Fasani et al. 2021, 2022; Foged et al. 2024; Marbach et al. 2018; Hainmueller et al. 2016; Slotwinski et al. 2019; Ahrens et al. 2023; Bahar et al. 2021, 2025; Ibáñez et al. 2025; Winton 2026; Shamsuddin et al. 2021; Clemens et al. 2018; Evans and Fitzgerald 2017).

Second, the paper contributes to the literature on the "magnet" effects of destination-country policies for asylum flows. A parallel strand of research has tested whether welfare-benefit generosity attracts migrants more broadly; the strongest recent evidence identifies a non-trivial welfare elasticity on unselected inflows into Denmark (Agersnap et al. 2020), though that study bundles asylum seekers with other non-EU migrants. The more directly relevant literature tests whether work-authorization policy itself shifts asylum flows. That literature finds only small or null effects: the cleanest panel test finds a null effect of employment bans on asylum applications (Fasani et al. 2021), and destination-choice and cross-country-panel studies reach the same conclusion when EAD-type provisions are isolated from other policy margins (Hatton 2009, 2016, 2020; Neumayer 2004, 2005; Thielemann 2004; Bertoli et al. 2022; Di Iasio and Wahba 2024; Valenta and Thorshaug 2013; Gammeltoft-Hansen and Tan 2017). Our analysis provides the first nationality-level US test of that hypothesis on both the 1994–1995 reform and the 2021–2024 surge, and adds evidence consistent with the European literature's modest-to-null picture of the EAD margin specifically.

The paper proceeds as follows. Section 2 describes the court-records, survey, and administrative data that underpin the analysis. Section 3 documents the national and local geography of the 2021–2024 surge. Section 4 presents the long-difference IV design. Section 5 reports the economic-effects estimates, decomposes the GDP effect into labor and capital components, and benchmarks the total against projections from the Congressional Budget Office and the Penn Wharton Budget Model; Section 6 reports sensitivity and weak-IV checks. Section 7 examines the EAD-magnet hypothesis using the historical 1994 reform, an administrative claim about work-permit receipt among denied asylum applicants, and the contemporary nationality composition of the surge. Section 8 concludes.

2 Data

The empirical strategy combines five data sources: individual-level immigration-court records that identify asylum seekers by nationality, entry date, and residence; the American Community Survey for labor-market, earnings, and program-participation outcomes; the BEA's county-level GDP accounts; the QCEW for sector-level employment; and LAUS for unemployment. The court records are the essential ingredient, and we describe them first.

2.1 Asylum Seeker Flows

Our treatment variable is constructed from immigration court records obtained from the Transactional Records Access Clearinghouse (TRAC), a nonpartisan data research center at Syracuse University that acquires case-level administrative data from the Executive Office for Immigration Review (EOIR) through Freedom of Information Act requests. The TRAC database contains the universe of immigration court proceedings, including each respondent's hearing schedule, immigration judge decision, nationality, date of birth, date of US entry, and residential ZIP code at the time of case filing.

We restrict to individuals whose case includes a filing for asylum, asylum withholding, or withholding under the Convention Against Torture, and further restrict to working-age adults (ages 18–64 at entry). For individuals with multiple proceedings (e.g., an initial hearing followed by a BIA appeal), we use the last proceeding's decision to determine whether the individual retains employment authorization. Removals are defined as decisions that cause loss of Employment Authorization Document (EAD) status, following a legal classification of each decision type.

Each individual's residential ZIP code is mapped to a county using HUD USPS ZIP Code Crosswalk Files, and counties are aggregated to commuting zones using the Autor et al. (2013) FIPS-to-CZ crosswalk. For each CZ × year, we construct the cumulative net adult asylum flow (entries minus EAD-losing removals) and the corresponding Bartik-predicted flow. The resulting panel is, to our knowledge, the first use of the full universe of asylum-seeker court records with sub-county geographic detail to estimate local economic effects, and it is what allows the shift-share design that follows.

2.2 American Community Survey

Local-area outcomes come from the American Community Survey, administered by the US Census Bureau, which samples approximately 3.5 million addresses annually. We use 1-year PUMS files for 2021–2024 (and 2015–2019 for robustness), accessed through IPUMS USA (Ruggles et al. 2025). PUMAs are mapped to commuting zones using the Missouri Census Data Center's Geocorr 2022[3] population-weighted allocation factors.

We restrict to prime-age (25–54), civilian non-institutional respondents and construct four population

[3]Geocorr 2022: Geographic Correspondence Engine, Missouri Census Data Center, https://mcdc.missouri.edu/applications/geocorr2022.html

panels: **Native-born**: birthplace in US states or DC; **Citizens**: native-born plus naturalized; **Incumbent workers**: all citizens plus non-citizens who entered the US in 2020 or earlier; **Recent arrivals** (residual): non-citizens who entered after 2020. For each group × CZ × year, we compute: employment-to-population ratio, mean log wage income (employed workers), Medicaid enrollment rate, and welfare receipt rate. Cells with fewer than 50 unweighted observations are dropped.

2.3 Other Data Sources

The Bureau of Economic Analysis (BEA) CAGDP1 table provides county-level real GDP in chained 2017 dollars, aggregated to CZs by summation. The Quarterly Census of Employment and Wages (QCEW), covering approximately 95% of nonfarm payrolls, provides county-level supersector employment data for sector decomposition. The Local Area Unemployment Statistics (LAUS) program provides model-based county-level unemployment estimates, aggregated to CZs. For the magnet analysis (Section 7), we additionally use DHS Yearbook filing counts, EOIR court records obtained via FOIA, Eurostat European asylum data, and UNHCR global asylum statistics.

3 The Asylum Surge

The causal question requires identifying variation both in time and across locations. This section documents both: the national time-series of asylum inflows that rose and then fell between 2021 and 2024, and the highly uneven geographic distribution of those inflows across US commuting zones. Two features of the data are important for the analysis that follows. First, aggregate net flows are modest relative to the national labor force but sharply concentrated geographically, giving meaningful local variation from which to identify a treatment effect. Second, the timing of the 2021–2023 rise defines a well-bounded surge window that is the basis of our primary specification.

Figure 1 plots national asylum seeker flows for working-age adults by month of US entry. "Entries" counts individuals who eventually filed an asylum application, plotted by the month they entered the United States; "Removals" counts EAD-losing decisions, plotted by the month of the decision. Entries rise sharply beginning in early 2021, peak in mid-2023, and decline in late 2023 and 2024. Approximately 760,000 working-age asylum seekers entered the United States during 2021–2023. Net flows, the treatment variable in our regressions, turn negative by late 2024 as removals exceed new entries.

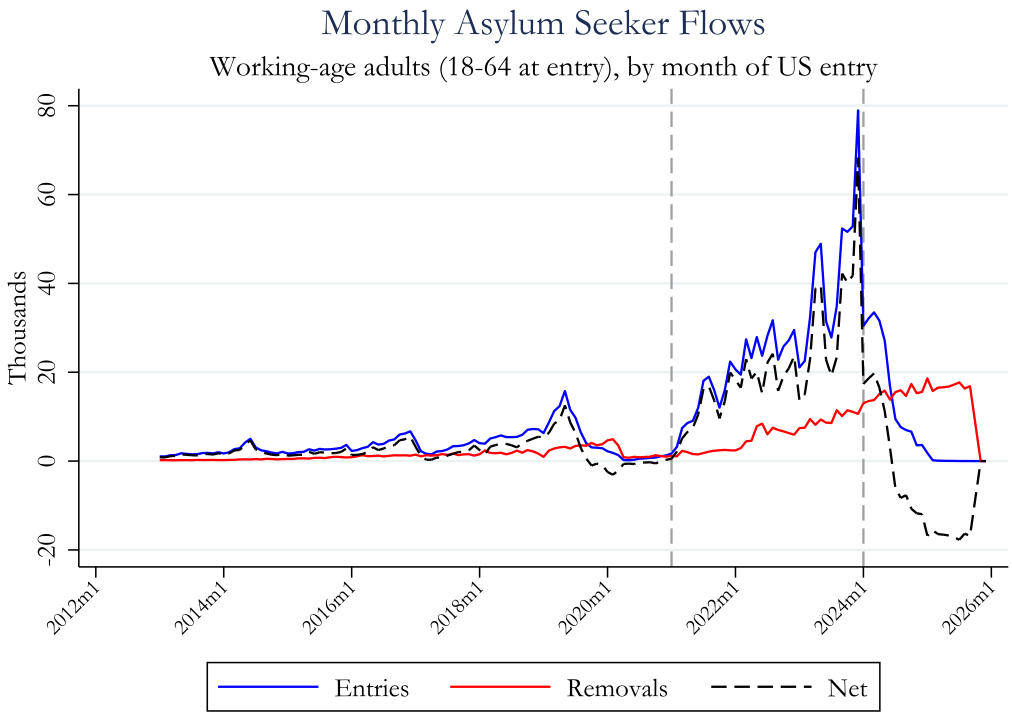

Figure 1: Monthly Asylum Seeker Flows: Working-Age Adults, by Month of US Entry
Notes: "Entries" = working-age (18–64) asylum seekers plotted by month of US entry. "Removals" = EAD-losing decisions plotted by month of decision. "Net" = entries minus removals. This figure plots flows by *entry date*, not by date of court filing; filing typically lags entry by months to years. Source: TRAC immigration court records.

Figure 2 shows the distribution of cumulative net adult asylum entries (2021–2023) as a share of 2019 CZ population. The upper tail extends beyond 1%, giving the local treatment variation that the shift-share design exploits.

We define two surge windows. Our primary specification, Surge A, covers 2021–2023, the period during which cumulative net migration (entries minus EAD-losing removals) grew most rapidly. By 2024, new entries had declined sharply while removals continued, compressing net flows (Figure 1). We use the 2021–2024 window (Surge B) as a robustness check. Section 7 examines asylum *filing* counts rather than net migration; filing counts, which reflect the date of court filing rather than the date of US entry, continued to rise through FY2024. The distinction between gross filings (by filing date) and net migration (by entry date) is important for interpreting the two parts of the paper.

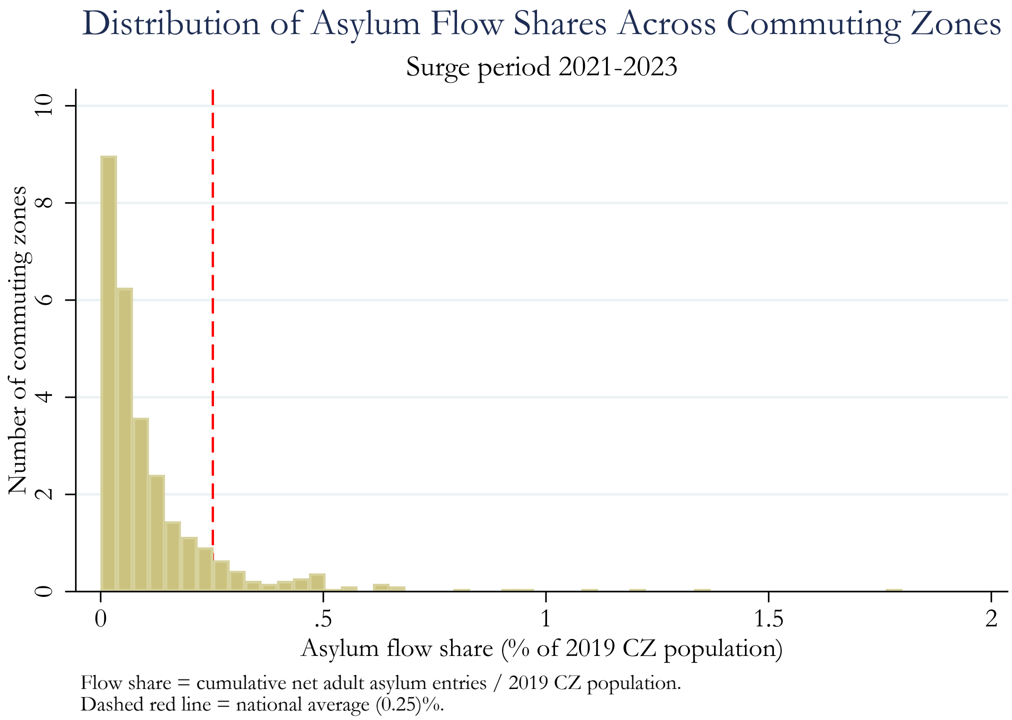

Figure 2: Distribution of Asylum Flow Shares Across Commuting Zones (2021–2023)
Notes: Histogram of cumulative net working-age asylum flow (2021–2023) divided by 2019 CZ population.

4 Empirical Strategy

The identification problem is that asylum seekers, like other migrants, sort across destinations in ways correlated with local economic conditions. Naively regressing changes in local outcomes on changes in the asylum share would conflate the causal effect with that sorting. We address the problem with a long-difference, shift-share instrumental-variable design of the type used in the broader literature on local labor-market effects of immigration and trade shocks. The treatment variable is the cumulative local net asylum flow normalized by pre-surge population; the instrument exploits the interaction between pre-surge nationality-by-location settlement patterns and subsequent leave-one-out national flows.

For each surge window, we estimate by two-stage least squares:

$$\Delta Y_j = \beta \cdot \text{FlowPC}_j + \varepsilon_j, \tag{1}$$

where j indexes commuting zones and ΔY_j is the change in the outcome between $t_0 = 2021$ and t_1. The treatment variable is $\text{FlowPC}_j = \sum_t F_{j,t}/\text{Pop}_{j,2019}$, where $F_{j,t}$ is the net working-age asylum flow. Each coefficient gives the effect of an inflow equal to 1% of CZ population.

We instrument with a Bartik shift-share predictor:

$$Z_j = \frac{\sum_t \widehat{F}_{j,t}}{\text{Pop}_{j,2019}}, \quad \text{where} \quad \widehat{F}_{j,t} = \sum_c s_{j,c} \cdot G_{c,t}^{(-j)}, \tag{2}$$

and $s_{j,c}$ is the 2019 share of nationality c's asylum seekers residing in CZ j, and $G_{c,t}^{(-j)}$ is the leave-one-out national flow. All regressions are weighted by 2019 CZ population with heteroskedasticity-robust standard errors. We winsorize the top 1% of the instrument distribution. First-stage F-statistics range from 15 to 20 across Surge A (2021–2023) specifications, above conventional thresholds for strong identification. First stages are weaker in Surge B (2021–2024) specifications, so we complement them with Anderson–Rubin tests that are valid regardless of instrument strength (Appendix Table A4).

The outcomes in equation (1) are:

- *Real GDP*: $\Delta Y_j = \ln(\text{GDP}_{j,t_1}) - \ln(\text{GDP}_{j,2021})$, from BEA county-level real GDP in chained 2017 dollars aggregated to CZs. Because the outcome is a log change, $\hat{\beta} \times 0.01$ approximates the percent change in GDP per inflow of 1% of CZ population.
- *Employment rate*: $\Delta Y_j^g = (\text{Emp}_{j,t_1}^g / \text{Pop}_{j,t_1}^g) - (\text{Emp}_{j,2021}^g / \text{Pop}_{j,2021}^g)$, from ACS microdata for prime-age (25–54) civilians using person weights, for native-born, citizen, and incumbent groups. $\hat{\beta}$ reads as the percentage-point change in the employment rate per inflow of 1% of CZ population.
- *Mean log wage* (same three groups): $\Delta Y_j^g = \overline{\ln(\text{wage})}_{j,t_1}^g - \overline{\ln(\text{wage})}_{j,2021}^g$, the CZ-level average of log wage income among employed workers. Interpretation parallels GDP: $\hat{\beta} \times 0.01$ is approximately the percent change in average wages.
- *Unemployment rate*: $\Delta Y_j = (\text{Unemp}_{j,t_1} / \text{LF}_{j,t_1}) - (\text{Unemp}_{j,2021} / \text{LF}_{j,2021})$, from BLS LAUS county-level unemployment aggregated to CZs and rescaled from 0–100 to 0–1 to match the ACS employment rates. $\hat{\beta}$ reads as the percentage-point change in the unemployment rate.
- *Public-benefits participation*: same construction as the employment rate, using ACS indicators for Medicaid coverage and positive welfare income.

5 Results: Economic Effects of the Asylum Surge

We organize the results around the two channels through which the policy debate frames the direct effects of asylum-seeker inflows: labor income for native and near-native workers (employment, wages, and unemployment), and capital income and aggregate output (GDP and the residual share). We then aggregate the estimates to national totals and benchmark the implied GDP effect against projections from the Congressional Budget Office and the Penn Wharton Budget Model, examine whether asylum inflows raise reliance on means-tested public programs, and decompose the employment response by sector to locate where the gains accrue. The results across these margins point in the same direction and are consistent with the cross-country literature on refugee and asylum inflows.

5.1 Labor Income: Employment and Wages

Table 1 reports IV estimates for Surge A (2021–2023). An asylum inflow equal to 1% of CZ population:

- raises native-born employment by 1.7 pp, citizen employment by 1.8 pp, and incumbent employment by 2.8 pp (all $p < 0.01$);
- raises native wages by 3.1%, citizen wages by 4.1%, and incumbent wages by 6.3%;
- reduces the unemployment rate by 1.7 pp ($p < 0.01$).

The pattern across nested groups (native $\subset$ citizen $\subset$ incumbent) reveals that effects strengthen as the population broadens to include naturalized citizens and pre-surge non-citizens, consistent with complementarity rather than displacement.

The employment effect implies roughly 1.1 additional incumbent jobs per asylum seeker. The next subsection translates the employment and GDP estimates into labor and capital compensation per asylum seeker; the one after that computes implied national totals and benchmarks them against CBO and Penn Wharton projections.

5.2 Capital Income: GDP and the Residual Share

The employment and wage effects in Table 1 measure labor income for incumbent workers. GDP (column 1) captures total output: labor income plus returns to capital and other residual income. The gap between the

Table 1: GDP, Employment, Wages, and Unemployment: IV Estimates (2021–2023)

		Native-born		Citizens		Incumbent		
	(1) Real GDP	(2) Emp	(3) Wage	(4) Emp	(5) Wage	(6) Emp	(7) Wage	(8) Unemp
Asylum flow per capita	5.530*	1.690***	3.129*	1.849***	4.145**	2.765***	6.274***	-1.682***
	(3.079)	(0.645)	(1.737)	(0.681)	(1.661)	(0.654)	(1.305)	(0.466)
Observations	605	451	451	451	451	451	451	605
First-stage F	15.4	20.3	20.3	20.3	20.3	20.3	20.3	15.3

Notes: Each column reports a separate IV long-difference regression at the commuting zone level. Real GDP: BEA CAGDP1 (chained 2017 dollars, log change). Employment rate = share employed; mean log wage = average of log wage income among the employed. ACS outcomes for prime-age (25–54). Native-born: US-born respondents. Citizens: native-born plus naturalized. Incumbent workers: all citizens plus non-citizens who entered the US in 2020 or earlier. Unemployment rate: BLS LAUS annual average (rescaled to 0–1). The endogenous variable is cumulative net working-age asylum flow divided by 2019 CZ population. The instrument is cumulative Bartik-predicted flow (2019 nationality settlement shares × leave-one-out national flows) divided by 2019 CZ population. Weighted by 2019 CZ population. Heteroskedasticity-robust standard errors in parentheses. Top 1% of instrument distribution excluded. ACS columns restrict to CZs with ≥ 50 ACS observations. * p<0.10, ** p<0.05, *** p<0.01.

two provides information about the non-labor-income channel. We decompose the GDP effect into labor and non-labor components using data from the ACS and standard compensation adjustments.

Labor income per asylum seeker. Each asylum seeker is associated with approximately 1.1 additional incumbent jobs (from the incumbent employment coefficient applied CZ by CZ; see Section 5.3). To value each job, we use the population-weighted arithmetic mean annual wage among full-time employed incumbent workers (prime-age, 25–54) in the 2021 ACS: \$73,900.[4] We apply a benefits multiplier of 1.45× to account for employer-paid health insurance, retirement contributions, and payroll taxes, following BLS Employer Costs for Employee Compensation.[5] This yields incumbent labor compensation of $1.11 \times \$73{,}900 \times 1.45 \approx \$119{,}000$ per asylum seeker.

For the asylum seeker's own labor contribution, we observe in the 2021 ACS that prime-age recent non-citizen arrivals (those entering after 2020) have an employment rate of 57% and a population-weighted arithmetic mean annual wage of \$47,000 among those employed. Applying the same benefits multiplier: $0.57 \times \$47{,}000 \times 1.45 \approx \$39{,}000$.

[4]Full-time defined as ≥35 usual hours per week and ≥48 weeks worked per year. The all-worker mean (including part-time) is \$64,800. We use the full-time mean as our central estimate because the employment regressions measure transitions into employment, which are more appropriately valued at full-job earnings.

[5]Bureau of Labor Statistics, "Employer Costs for Employee Compensation," September 2024. Wages and salaries account for approximately 69% of total compensation; the remainder comprises legally required benefits, insurance, and retirement. We use this all-worker multiplier as a conservative upper bound. Workers in the lower-wage service sectors where asylum-seeker employment is concentrated likely receive benefits at lower rates than the full-workforce average; a sector-specific multiplier would therefore produce a lower labor-compensation estimate, leaving a larger residual attributable to capital and other income. We retain the 1.45× multiplier to maintain comparability with the Regulatory Impact Analysis of U.S. Department of Homeland Security (2026), which uses the same figure.

Table 2: Decomposition of GDP per Asylum Seeker into Labor Compensation and Capital Income

	Lower-bound labor ($128,000)	Central labor ($158,000)
Conservative GDP per asylum seeker	$263,000	$263,000
Labor compensation	$128,000	$158,000
Capital and other residual income	$135,000	$105,000
Residual share of GDP	51%	40%

Our central estimate of total labor compensation is therefore approximately $119,000 + $39,000 = $158,000 per asylum seeker. Replacing the full-time wage and multiplier with their lower-bound counterparts (the all-worker incumbent mean of $64,800 and a 1.30× multiplier) yields approximately $128,000.[6] The labor-compensation effect is thus in the range of $128,000–$158,000 per asylum seeker.

Capital and other residual income. The GDP point estimate implies approximately $430,000 per asylum seeker (Section 5.3), but this estimate is imprecise ($p = 0.07$; 95% CI: [−$39,000, $899,000]). We therefore also report results for a more conservative value of $263,000 per asylum seeker, corresponding to a GDP coefficient of approximately 3.4, well within the confidence interval. Under this conservative estimate, the residual after subtracting labor compensation is given in Table 2.

The residual, representing returns to capital, proprietors' income, and indirect effects, ranges from 40% to 51% of GDP depending on the labor-compensation assumption. The lower end of this range is closely in line with the national capital share of approximately 35–40% of GDP; the upper end is somewhat higher, which may reflect multiplier effects, informal economic activity not captured in ACS wages, or imprecision in the GDP coefficient. When additional workers enter a local economy, existing physical capital (commercial real estate, equipment, infrastructure) is utilized more intensively, generating additional profits and rental income for businesses and property owners.

The employment effects, which are precisely estimated at the 1% level and do not depend on the GDP coefficient, imply that each asylum seeker is associated with $128,000–$158,000 in labor compensation for incumbent workers and the asylum seeker themselves. The GDP effect adds capital and other residual income on top of that. This decomposition clarifies why the Regulatory Impact Analysis in U.S. Department of Homeland Security (2026), which focuses exclusively on labor replacement, understates the economic

[6] Lower bound, computed from unrounded sample means (incumbent all-worker mean wage $64,836; recent-arrival mean wage $47,003; recent-arrival employment rate 0.570): $1.11 \times \$64{,}836 \times 1.30 + 0.570 \times \$47{,}003 \times 1.30 \approx \$93{,}600 + \$34{,}800 = \$128{,}400$.

Table 3: Implied Aggregate National Effects (Surge A, 2021–2023)

	Implied national total	Per asylum seeker
GDP increase (point estimate)	\$330 billion	\$430,000
GDP increase (conservative)	\$200 billion	\$263,000
Additional employed natives (25–54)	362,000	0.49
Additional employed citizens (25–54)	474,000	0.64
Additional employed incumbents (25–54)	820,000	1.11
Fewer unemployed	647,000	0.85
Labor compensation (per asylum seeker)		\$158,000
of which: incumbent workers		\$119,000
of which: asylum seeker's own		\$39,000
Capital and residual income (conservative)		\$105,000

Notes: Implied national totals are computed CZ by CZ and summed (see text). Per-asylum-seeker figures divide each national total by the cumulative net working-age asylum flow within that outcome's estimation sample: 767,260 for GDP, 737,672 for the ACS employment outcomes, and 759,547 for unemployment. The samples differ because ACS outcomes are restricted to CZs with at least 50 observations per cell.

cost of the proposed rule. Restricting asylum-based work authorization would reduce not only labor income but also returns to capital and other residual income for businesses and property owners in affected communities.

5.3 Aggregate National Effects

To translate CZ-level coefficients into national totals, we compute implied effects CZ by CZ and sum. For employment:

$$\text{Additional employed}_j = \hat{\beta} \times \frac{\text{flow}_j}{\text{pop}_{j,2019}} \times \text{group\_pop}_{j,2021}$$

where $\text{group\_pop}_{j,2021}$ is the ACS-weighted population of the relevant group (e.g., native-born prime-age) in CZ j in the baseline year. This accounts for the geographic concentration of asylum seekers: CZs receiving larger inflows contribute more to the national total. The GDP calculation is analogous, using baseline GDP in place of group population.

During the 2021–2023 surge, approximately 760,000 working-age asylum seekers entered the United States. Applying the IV coefficients to CZ-level data yields the implied effects in Table 3.

The GDP point estimate ($\hat{\beta} = 5.53$, $p = 0.07$) is imprecisely estimated. The conservative estimate corre-

Table 4: Implied additional GDP per asylum seeker: Comparison with CBO and Penn Wharton

Study	Population (millions)	GDP per immigrant	Notes
This paper (conservative)	0.76	$263,000	IV estimate, 2021–2023 asylum surge, short-run
CBO (2024)	8.7	$149,000	Annual GDP increase in 2034 from 2021–26 surge
Penn Wharton (2025)			
4-year deportation	4.7	$70,000	GDP loss by 2034 from removing unauthorized
10-year deportation	11.8	$208,000	GDP loss by 2054 from removing all unauthorized

Sources: Congressional Budget Office (CBO 2024). Ruiz Mazin and Reichling, "Mass Deportation of Unauthorized Immigrants: Fiscal and Economic Effects," Penn Wharton Budget Model (2025). CBO figure: \$1.3T additional GDP in 2034 ÷ 8.7M immigrants. Penn Wharton: GDP percent changes applied to projected GDP levels, divided by immigrants removed. All three analyses include general-equilibrium effects on native workers and capital; CBO and Penn Wharton both find that removing immigrants reduces GDP per capita and average wages, not only total GDP.

sponds to $\hat{\beta} \approx 3.4$, well within the 95% confidence interval $[-0.50, 11.56]$, and produces a capital share of 40%, in line with the national average of 35–40%. The employment and unemployment figures are precisely estimated at the 1% level and do not depend on the GDP coefficient. Labor compensation is computed using full-time ACS wages with a 1.45× benefits multiplier (central estimate; Section 5.2 documents a \$128,000–\$158,000 range across plausible compensation assumptions).

Benchmarking against other estimates. Table 4 compares our GDP estimates with two recent independent analyses: the Congressional Budget Office's assessment of the 2021–2026 immigration surge, and the Penn Wharton Budget Model's analysis of mass deportation. Both are nonpartisan institutions that use general-equilibrium frameworks capturing effects on native workers, capital adjustment, and productivity.

All three analyses find that immigrants raise GDP, and that removing them would reduce it. Both CBO and Penn Wharton find effects beyond simple population scaling: CBO projects that the immigration surge raises GDP per capita and boosts long-run wages through innovation-related productivity growth; Penn Wharton finds that deportation reduces GDP per capita by 0.5–1.1% and lowers average wages by 0.5–1.7%, with high-skilled native workers harmed most by the loss of complementary low-skilled labor. Our estimate is somewhat higher than CBO's, as expected for a local IV estimate that captures the full marginal effect of an inflow at the intensive margin, rather than averaging across millions of immigrants at different stages of integration. The Penn Wharton long-run estimate (\$208,000) is closest to ours, reflecting the full GDP contribution including capital adjustment. Cross-country evidence from the IMF similarly finds that large immigration waves raise output and productivity in advanced economies within five years, with approximately two-thirds of the output gain attributable to higher labor productivity (Engler et al. 2020).

Table 5: Means-Tested Public Benefit Participation: IV Estimates (2021–2023)

	Native-born		Citizens		Incumbent	
	(1) Medicaid	(2) Welfare	(3) Medicaid	(4) Welfare	(5) Medicaid	(6) Welfare
Asylum flow per capita	-1.995**	-0.495*	-1.877**	-0.761**	-1.839**	-0.820***
	(0.778)	(0.297)	(0.785)	(0.301)	(0.761)	(0.299)
Observations	451	451	451	451	451	451
First-stage F	20.3	20.3	20.3	20.3	20.3	20.3

Notes: Each column reports a separate IV long-difference regression at the commuting zone level. Outcomes are from the ACS (prime-age 25–54). Medicaid enrollment = share reporting Medicaid coverage. Welfare receipt = share reporting positive welfare income. Native-born: US-born respondents. Citizens: native-born plus naturalized. Incumbent workers: all citizens plus non-citizens who entered the US in 2020 or earlier. The endogenous variable is cumulative net working-age asylum flow divided by 2019 CZ population. The instrument is cumulative Bartik-predicted flow (2019 nationality settlement shares × leave-one-out national flows) divided by 2019 CZ population. Weighted by 2019 CZ population. Heteroskedasticity-robust standard errors in parentheses. Top 1% of instrument distribution excluded. CZs with ≥ 50 ACS observations. * p<0.10, ** p<0.05, *** p<0.01.

Peri (2012) reports similar TFP effects at the US state level, driven by task specialization.

5.4 Public Benefits

Table 5 tests whether asylum inflows increase reliance on means-tested public benefits. Medicaid enrollment falls across all three groups (1.8–2.0 pp, $p < 0.05$). Welfare receipt also declines (0.5–0.8 pp). These results are inconsistent with asylum seekers straining public programs. The improved labor market conditions documented above appear to pull existing residents off public assistance rather than crowding them onto it.

5.5 Sector Decomposition

Figure 3 shows that employment gains are concentrated in lower-wage service sectors, especially leisure and hospitality: sectors in which labor-supply bottlenecks during the 2021–2023 recovery were widely documented and in which refugee labor-market integration has historically been fastest (Fasani et al. 2022).

6 Robustness

The main IV estimates are subjected to three classes of checks: alternative specifications of the treatment and instrument, weak-identification-robust inference, and pre-trend controls that absorb differential

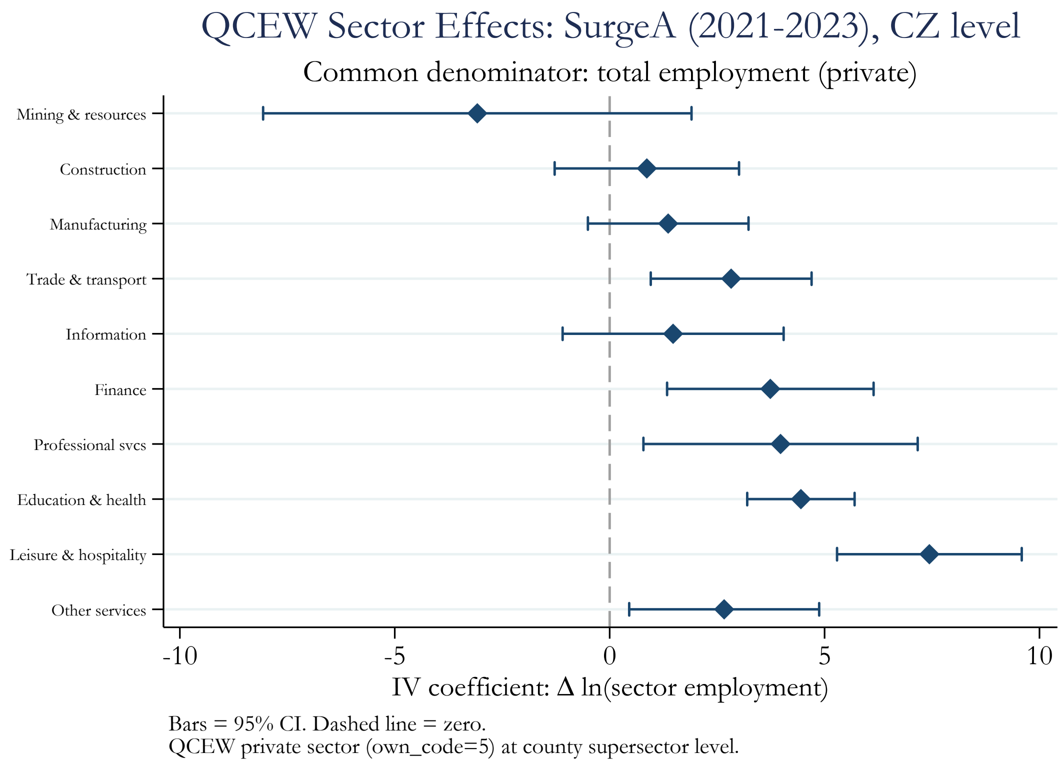

Figure 3: QCEW Sector Employment Effects: Surge A (2021–2023)
Notes: Each point is a separate IV regression. Bars show 95% confidence intervals.

commuting-zone trajectories predating the surge. Appendix Table A1 reports OLS estimates and Surge B (2021–2024) results. OLS coefficients are uniformly smaller than IV, consistent with negative selection, measurement error, or treatment-effect heterogeneity. In Surge B, some employment coefficients attenuate but remain positive; the unemployment effect remains significant throughout. Appendix Table A4 reports Anderson–Rubin tests for Surge B, valid regardless of instrument strength. Appendix Tables A5–A7 report estimates with stepwise controls and pre-trend controls: the employment, unemployment, and public-program effects are stable across these specifications, while the GDP coefficient—imprecise at baseline—attenuates and loses significance once pre-trend or state fixed-effect controls are added.

Taken together, the evidence in Sections 5–6 implies that the direct effect of an additional asylum seeker on native economic outcomes is positive across every margin we measure. This is the first empirical pillar of the analysis. The estimates lie in the range of prior refugee-specific evidence from Europe and Latin America, though at the upper end for magnitude—consistent with the tight 2021–2023 US labor market. We turn next to the second pillar: whether work-authorization policy substantially changes the *number* of asylum seekers who arrive.

7 Does Employment Authorization Act as a Magnet?

The magnet question is the second empirical pillar of the analysis. The relevant research literature has tested, across a range of European settings, whether employment-authorization policy materially changes the number of asylum applications received. The evidence from that literature points one way: the cleanest cross-country panel test finds a statistically null effect of employment bans on application flows (Fasani et al. 2021), and destination-choice and cross-country-panel studies reach the same conclusion when EAD-type provisions are isolated from other policy margins (Hatton 2009, 2016, 2020; Neumayer 2004, 2005; Thielemann 2004; Bertoli et al. 2022; Di Iasio and Wahba 2024; Valenta and Thorshaug 2013; Gammeltoft-Hansen and Tan 2017). A parallel literature on welfare-benefit magnets has identified a non-trivial elasticity on unselected migration to high-benefit destinations (Agersnap et al. 2020), but that evidence concerns choices *among* destinations for migrants who have already decided to leave origin countries rather than choices about whether to migrate at all. For the EAD margin specifically, the European literature has repeatedly failed to locate an effect of policy-relevant magnitude.

This literature has not directly investigated the United States. The policy debate relies heavily on a domestic historical precedent—the 1994 Asylum Reform, which introduced the 150-day EAD waiting period[7] and was followed by a sharp decline in affirmative filings—and on an implicit claim that the 2021–2024 US surge reflects a similar EAD incentive. We evaluate both. For the 1994 reform, we re-examine the filing decline using combined affirmative-plus-defensive filings (the full population of cases) and the nationality composition of the decline, rather than the aggregate affirmative series on which the domestic argument rests. For the 2021–2024 surge, we test the nationality-level predictions that a common EAD incentive would generate against the patterns that actually appear in the data.

A single feature of US EAD policy structures the analysis that follows. US work-authorization rules for asylum seekers do not vary by nationality: every applicant faces the same waiting period, validity terms, and renewal rules. A uniform policy incentive is therefore a poor candidate for explaining a filing surge concentrated in particular nationalities, a point we develop below in analyzing the 2021–2024 episode.

[7] Under the 1994 rule (effective January 1995), an asylum applicant must wait 150 days after filing before applying for an EAD, and no EAD may be granted until the asylum application has been pending 180 days (the 150-day waiting period plus a 30-day adjudication window). IIRIRA later codified the 180-day clock. Elsewhere in the paper we refer to the operative bar as the 180-day waiting period.

7.1 The 1994 Precedent

The 1994 US reform is the principal domestic historical precedent invoked in contemporary debates about whether restricting EADs substantially reduces asylum filings. The pre-reform baseline was approximately 127,000 affirmative filings in FY1993 (rising to roughly 146,000 in FY1994 as settlement-related filings surged); the affirmative series fell nearly 80% between FY1995 and FY1999, and the proposed 2026 rule projects "similar results" under the new waiting-period regime (U.S. Department of Homeland Security 2026, p. 8651). Two features of the 1994 episode complicate this inference. First, the published 80% figure rests exclusively on *affirmative* filings; the Illegal Immigration Reform and Immigrant Responsibility Act (IIRIRA) of 1996 structurally shifted cases from affirmative to defensive proceedings, so the affirmative series mechanically overstates the decline. Combined affirmative-plus-defensive filings, the complete population of asylum cases, show a much more modest fall. Second, that smaller combined decline was concentrated in a small set of Central American nationalities whose trajectories are better explained by origin-country and legal events than by EAD policy.

Figure 4 decomposes combined filings by nationality from FY1993 to FY2003. The aggregate decline was driven almost entirely by El Salvador and Guatemala. Filings from all other countries (red) barely declined: they *rose* for two years after the EAD rule took effect in January 1995, dipped modestly in 1998–1999, and then climbed past 1995 levels. Each Central American country's trajectory tracks its own crisis, not EAD policy. Salvadoran filings surged as the roughly 187,000 Salvadorans registered for TPS became eligible to file under the *ABC* settlement before its January 31, 1996 deadline, then collapsed as the peace process concluded. Guatemalan filings declined after the comprehensive peace accords were signed in December 1996. Mexican filings *rose* after the reform: the opposite of what a deterrence theory would predict.

The modest post-1994 dip in non-Central American filings was not unique to the United States. Figure 5 indexes US filings (excluding El Salvador, Guatemala, and Mexico) and total European filings to 1993. The two series move closely together from 1995 onward, both declining through 1995–1996 and both recovering through 2000–2002, despite no comparable EAD reform in Europe. US EAD policy cannot explain the European series. The resolution of post-Cold War displacement crises can explain both.

A further point uses the rule's own reasoning. U.S. Department of Homeland Security (2026) cites the post-1994 rise in grant rates, from 15% in FY1993 to 38% in FY1999, as evidence that the reform successfully deterred frivolous filers, concluding that "with overall asylum filings decreasing and the approval rate increasing, the clear implication was that ineligible aliens … stopped filing" (U.S. Department of Homeland

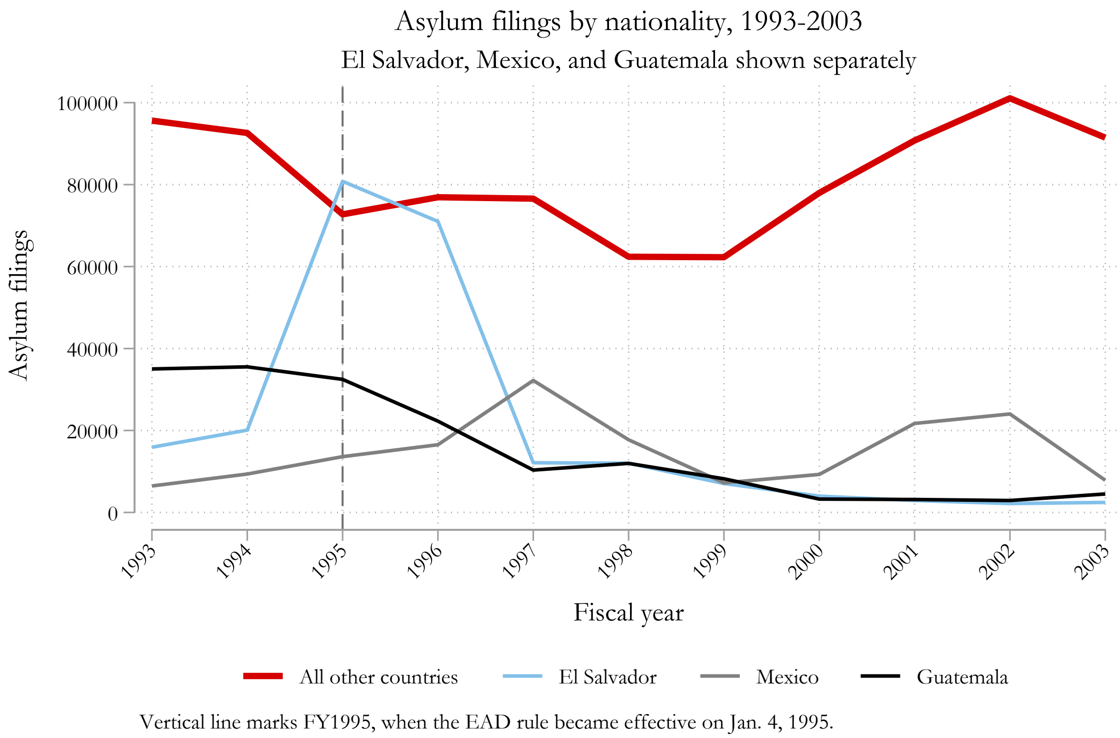

Figure 4: Asylum Filings by Nationality, FY1993–2003

Notes: Combined affirmative and defensive filings. Vertical dashed line marks FY1995 (EAD rule). Red series: all countries except El Salvador, Guatemala, and Mexico.

Security 2026, p. 8632). Extending that logic forward is informative. As combined filings *recovered* from about 109,000 in FY1999 to 156,000 in FY2002, grant rates continued to rise, reaching approximately 40%. By DHS's own reasoning, the recovery of filings was driven by applicants with increasingly meritorious claims, not by frivolous filers returning to exploit EAD access.

The long-run record is also difficult to reconcile with a durable EAD deterrent. After a temporary dip, defensive filings climbed steadily to 959,000 by FY2024, which is 13.4 times the FY1999 level, with the 180-day EAD waiting period in effect throughout (Figure 6). Three decades of data show a growing, not shrinking, defensive caseload under the post-1994 EAD regime. The 1994 precedent thus does not bear the weight the policy debate has placed on it. Accounting for the structural shift from affirmative to defensive proceedings shrinks the decline considerably; setting aside El Salvador and Guatemala removes most of what remains. A deterrence account also leaves unexplained both the rising grant rates during the recovery and the growth of the defensive caseload in the decades since. In short, the case that EAD restrictions substantially and durably reduce filings is difficult to defend empirically.

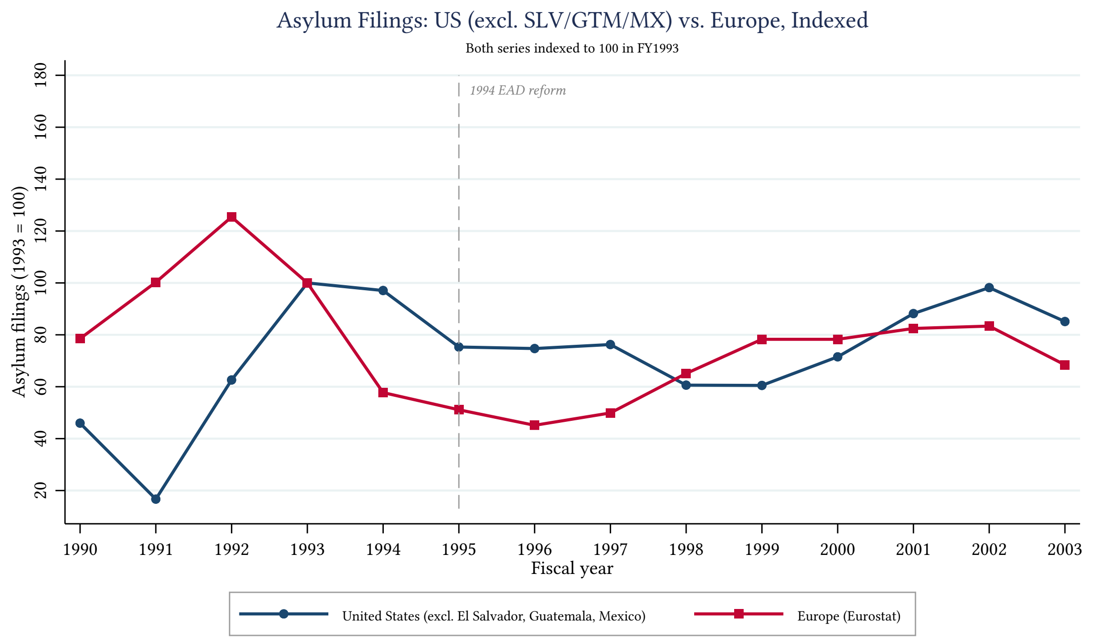

Figure 5: US and European Asylum Filings, Indexed to FY1993
Notes: US filings exclude El Salvador, Guatemala, and Mexico. Europe had no comparable EAD reform. Sources: INS/DHS Yearbooks and EOIR (US); Eurostat (Europe).

7.2 High EAD Receipt Among Denied Applicants as a Processing Artifact

A second piece of domestic evidence invoked in support of the magnet hypothesis is an administrative statistic rather than a historical analogy. U.S. Department of Homeland Security (2026, Table 3, p. 8640) reports that in FY2024, USCIS issued 5,709 denials or referrals to asylum applicants, of which 5,087 (89 percent) had previously received a (c)(8) EAD, and frames the figure as evidence that "current asylum processing is not functioning properly" (U.S. Department of Homeland Security 2026, p. 8640). The implicit claim is that a large share of ultimately-denied applicants filed primarily to obtain work authorization.

The figure does not support that claim. DHS itself concedes the point explicitly: "USCIS notes that it is not necessarily assigning, and does not need to assign, any fraudulent or bad intent to this population. These are simply cases where the alien was ultimately found ineligible for asylum, but, due to current agency regulations, policies, and processes, was able to derive employment authorization despite asylum ineligibility" (U.S. Department of Homeland Security 2026, p. 8640). U.S. Department of Homeland Security (2026) further acknowledges that "the alien's asylum application will likely remain pending for years given the asylum backlog" (U.S. Department of Homeland Security 2026, p. 8643), with reported average processing times of 22 to 35 months.

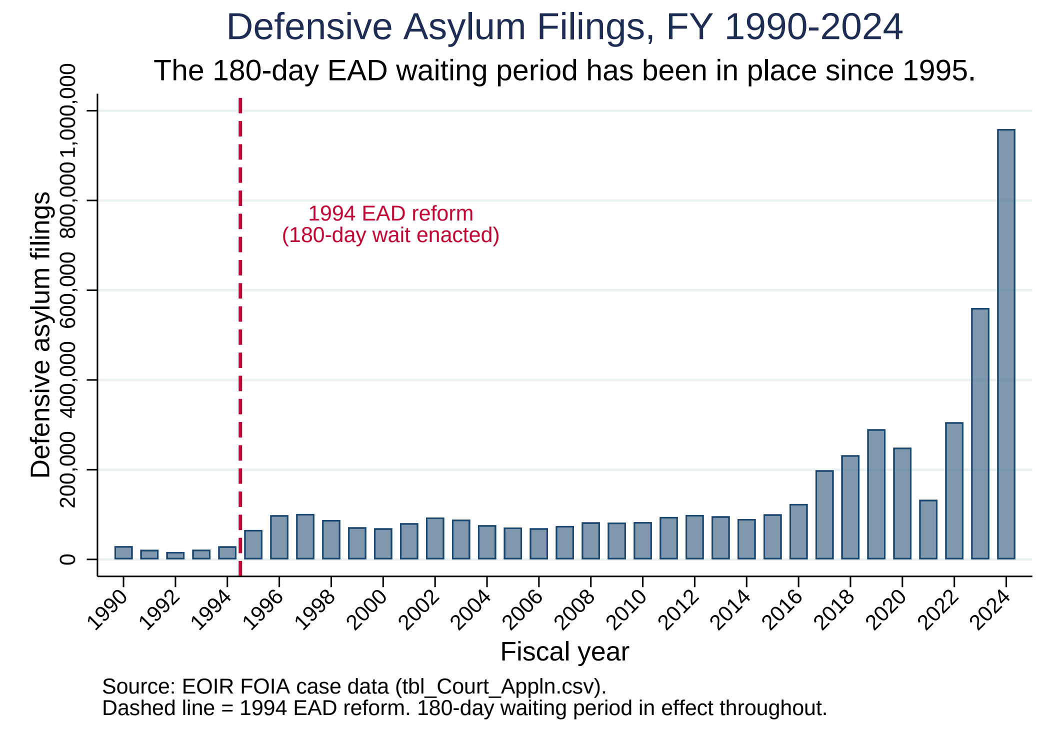

Figure 6: Defensive Asylum Filings, FY 1990–2024

Figure 7 shows the processing-time distribution for asylum cases decided between 2020 and 2024. The mean processing time is 2.3 years (the median is 1.5 years), and more than three-quarters of cases take longer than 180 days, the statutory threshold for EAD eligibility. The arithmetic is then mechanical: any system that combines a multi-year adjudication backlog with a 180-day EAD threshold will produce a high share of EAD receipt among eventually-denied cases, regardless of applicant intent. The 89 percent figure would appear even if every applicant filed in good faith. It is an artifact of the queue, not a signal about why people filed.

7.3 The 2021–2024 Surge

If neither the 1994 precedent nor the administrative EAD-receipt statistic supports the magnet hypothesis, does the 2021–2024 surge provide better evidence? The magnet hypothesis generates two testable implications for the contemporary surge. First, because EAD policy does not vary by nationality, a common EAD incentive predicts that the filing increase should be broad-based across nationalities, not concentrated in a few. Second, if the surge is driven by applicants seeking work authorization rather than protection, the filing increase should be skewed toward nationalities with historically low grant rates, groups whose claims

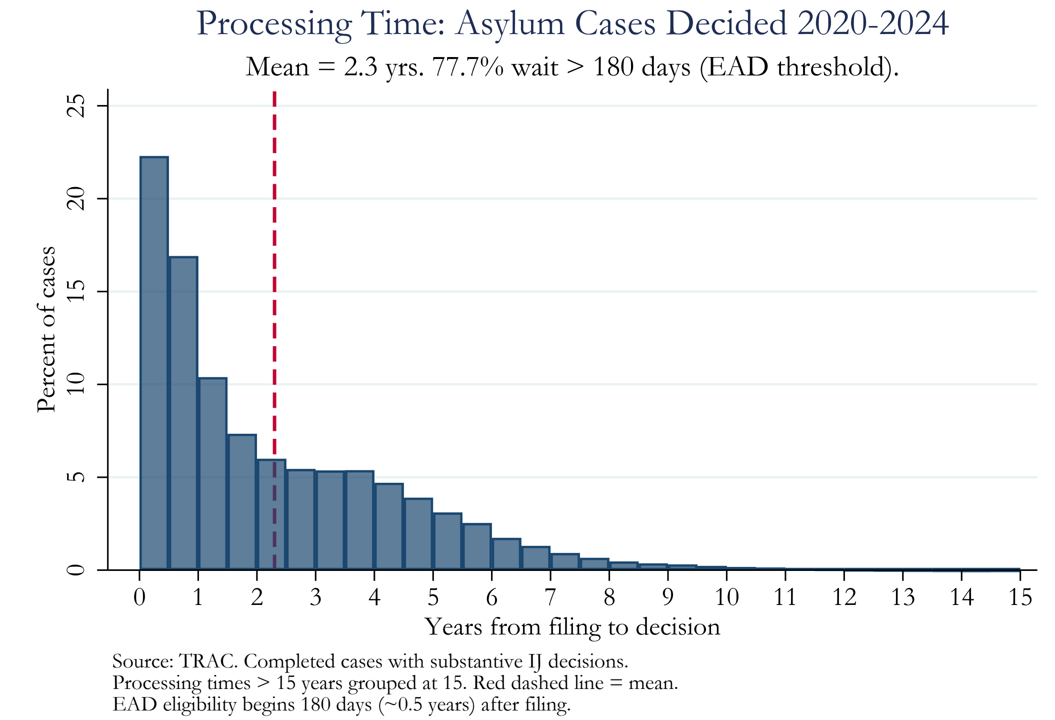

Figure 7: Processing Time: Asylum Cases Decided 2020–2024
Notes: Distribution of processing times (filing to decision) for asylum cases decided FY2020–2024. Mean processing time is 2.3 years (median 1.5 years); 77.7% of cases exceed the 180-day EAD eligibility threshold. Source: TRAC.

are routinely denied. Neither prediction is borne out.

Figure 8 shows the nationality composition using asylum filings by date of court filing (not date of US entry, as in Figure 1). Venezuela alone accounts for roughly 86,000 average annual filings during 2021–2024, up from approximately 4,400 pre-surge, a twentyfold increase. Colombia, Cuba, Haiti, Ecuador, and Nicaragua follow. Filings from other nationalities barely changed. This extreme concentration is inconsistent with the first prediction.

The second prediction also fails. The largest filing increases are not concentrated among nationalities with historically low grant rates (Figure 9 and Table 6). Grant rates among the top surge nationalities vary widely (Venezuela's is 51.9%, China's 72.1%, and Russia's 67.8%, while Colombia's is 32.8% and Haiti's 11.7%), but they are not systematically low: the filing-weighted average historical grant rate across the top 15 surge nationalities is 40%, close to the 37% average across all asylum cases filed over the same period. The cross-sectional correlation between filing increases and historical grant rates is essentially zero ($r = -0.10$). A frivolous-filing account would predict a strongly negative relationship; the data show none.

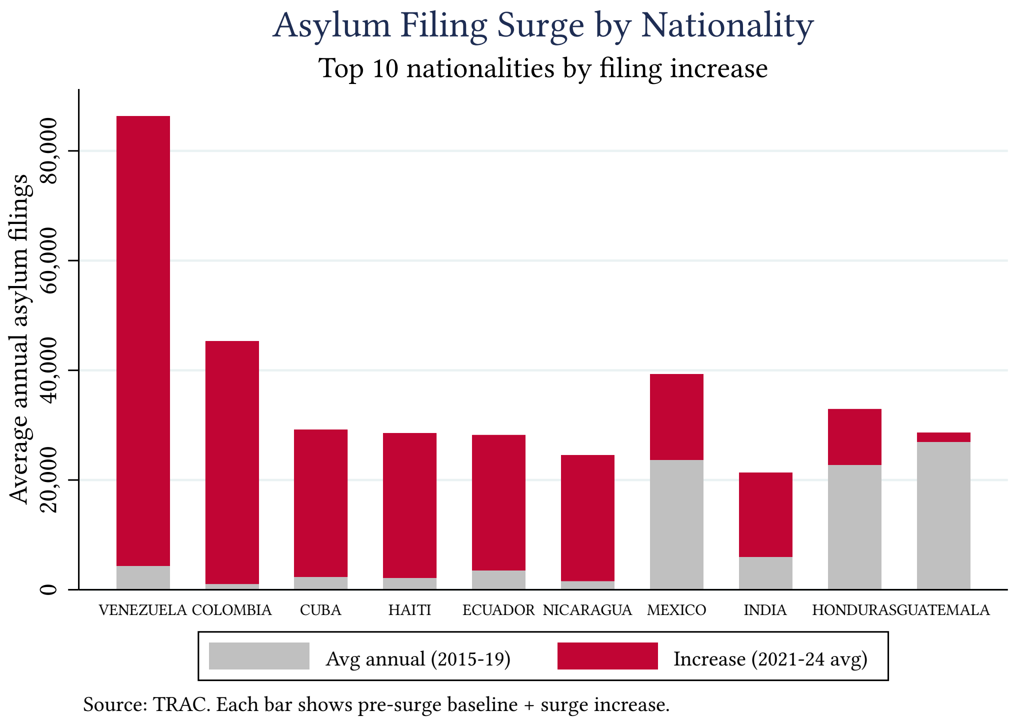

Figure 8: Asylum Filing Surge by Nationality (by Date of Court Filing)
Notes: Average annual asylum filings by nationality, measured by date of court filing. This differs from Figure 1, which plots flows by date of US entry. Filing typically lags entry by months to years. Source: TRAC.

If neither the breadth nor the composition of the surge is consistent with an EAD incentive, what does account for the nationality-specific timing? For each of the three largest surge nationalities, the timing tracks a specific origin-country crisis or route change, and none of these coincides with any change in employment-authorization policy. The **Venezuelan** case is the clearest and unfolds in three phases, each driven by events outside the United States. First, between 2014 and 2020, the collapse of the oil sector (production fell from roughly 2.8 million barrels per day in 2014 to a monthly low of approximately 350,000 in mid-2020), hyperinflation that peaked above 65,000% in 2018, and political repression drove over 7.7 million Venezuelans, more than 20% of the population, out of the country. The initial displacement went overwhelmingly to regional neighbors: by the end of 2019, Colombia hosted approximately 1.8 million Venezuelans, Peru over 860,000, Chile 455,000, and Ecuador 385,000. Second, the COVID-19 pandemic deteriorated conditions for Venezuelans in those regional host countries, prompting secondary movement northward beginning in 2020–2022; this movement originated in Colombia, Peru, Chile, and Ecuador rather than in Venezuela itself. Third, the Darién Gap opened as a viable overland corridor: approximately 3,000 Venezuelans crossed the Darién in all of 2010–2021 combined, rising to more than 150,000 in 2022 alone. The US surge in 2022–2023 is the visible downstream effect of this three-stage process. EAD policy did

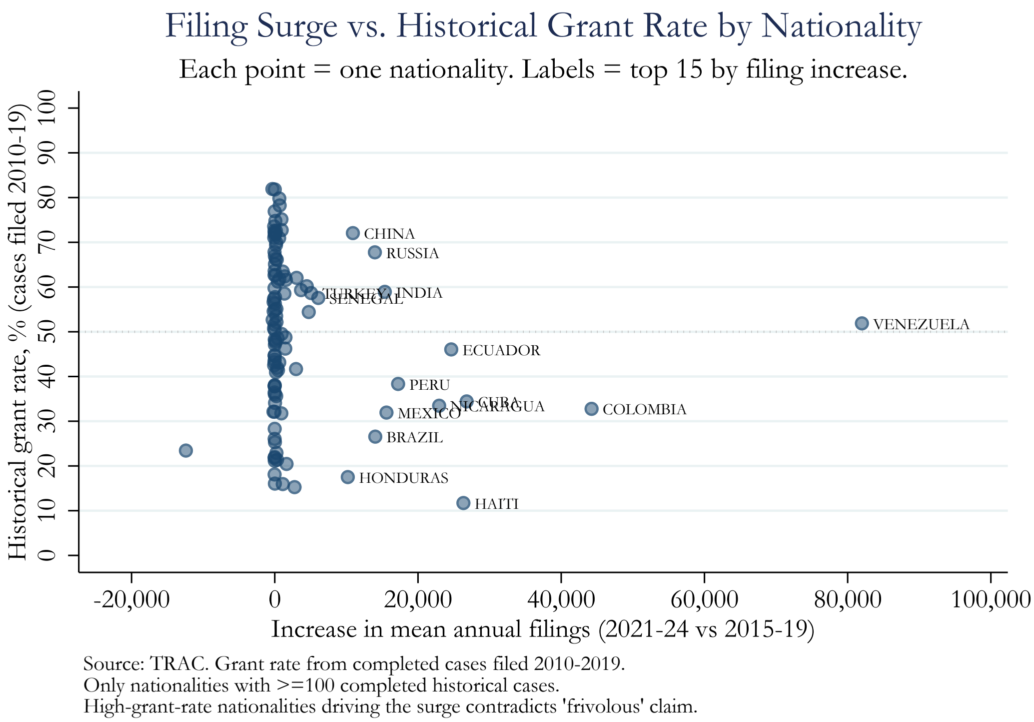

Figure 9: Filing Surge vs. Historical Grant Rate by Nationality

Table 6: Top 15 Surge Nationalities by Filing Increase

Rank	Nationality	Avg/yr 2015–19	Avg/yr 2021–24	Increase (per yr)	Grant rate (%)
1	VENEZUELA	4,362	86,356	81,994	51.9
2	COLOMBIA	1,036	45,278	44,242	32.8
3	CUBA	2,369	29,170	26,801	34.4
4	HAITI	2,159	28,505	26,346	11.7
5	ECUADOR	3,576	28,224	24,647	46.1
6	NICARAGUA	1,555	24,510	22,955	33.5
7	PERU	633	17,860	17,227	38.3
8	MEXICO	23,689	39,290	15,601	31.9
9	INDIA	5,984	21,355	15,371	58.9
10	BRAZIL	1,713	15,748	14,034	26.6
11	RUSSIA	814	14,796	13,982	67.8
12	CHINA	6,209	17,114	10,905	72.1
13	HONDURAS	22,758	32,956	10,198	17.5
14	SENEGAL	108	6,182	6,074	57.6
15	TURKEY	225	5,300	5,075	58.7

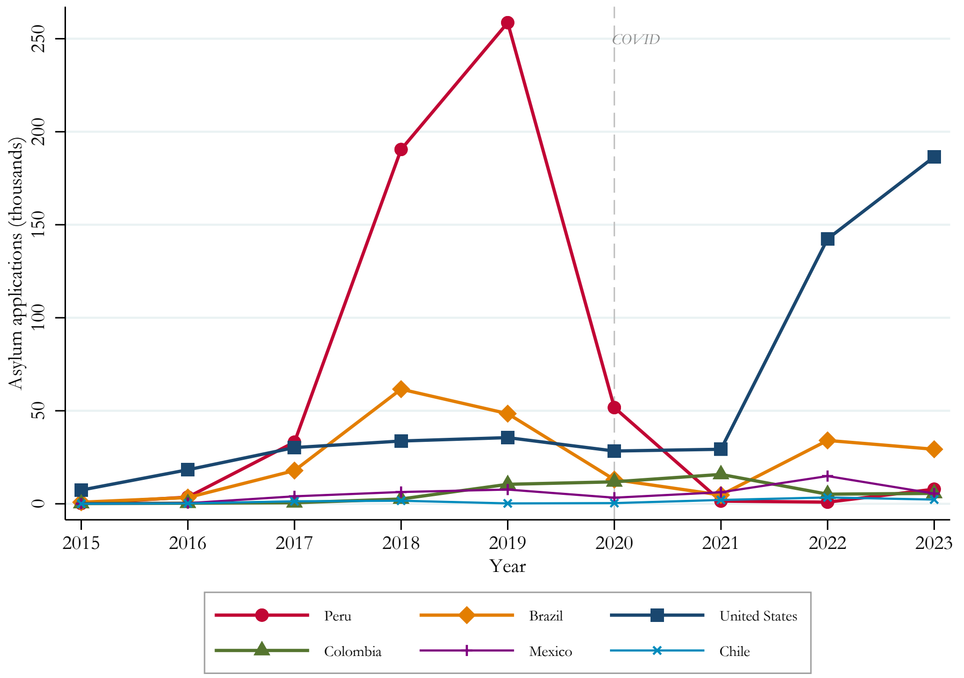

Figure 10: Venezuelan Asylum Applications by Destination Country, 2015–2023

not change across any of the three phases. And Venezuelan asylum applications surged across multiple destination countries in the Americas, not just the United States (Figure 10); none of the other destinations offers US-style employment authorization.

Cuban filings surged immediately after Nicaragua dropped its visa requirement for Cuban citizens in November 2021, creating a new overland route to the US border (Figure 11). The Cuban surge began within months of that route change, amid a deepening economic and political crisis on the island; EAD policy did not change. Nicaragua reimposed the visa requirement in February 2026, providing a second quasi-experimental test of the route-change story: a fall in Cuban US filings after the reimposition, with EAD rules held fixed, would further corroborate the route explanation over the magnet explanation.

The **Haitian** case parallels the Cuban one: filings increased from near zero to tens of thousands in the years following the assassination of President Jovenel Moïse on July 7, 2021, which precipitated a severe security crisis (Figure 12). By 2024, approximately 80% of Port-au-Prince was under gang control. Haiti's 11.7% historical grant rate, as plotted in Figure 9, understates the strength of current Haitian claims because it is computed over a baseline period (2010–2019) in which conditions were less dire than those facing applicants filing in 2021–2024.

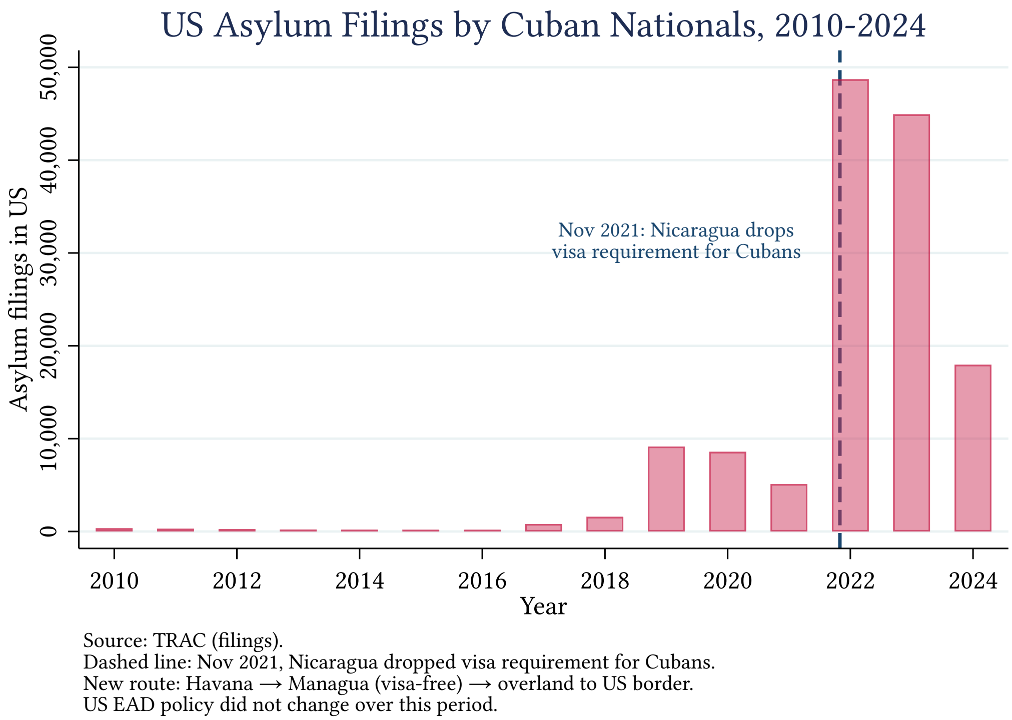

Figure 11: US Asylum Filings by Cuban Nationals and the November 2021 Nicaragua Visa Change

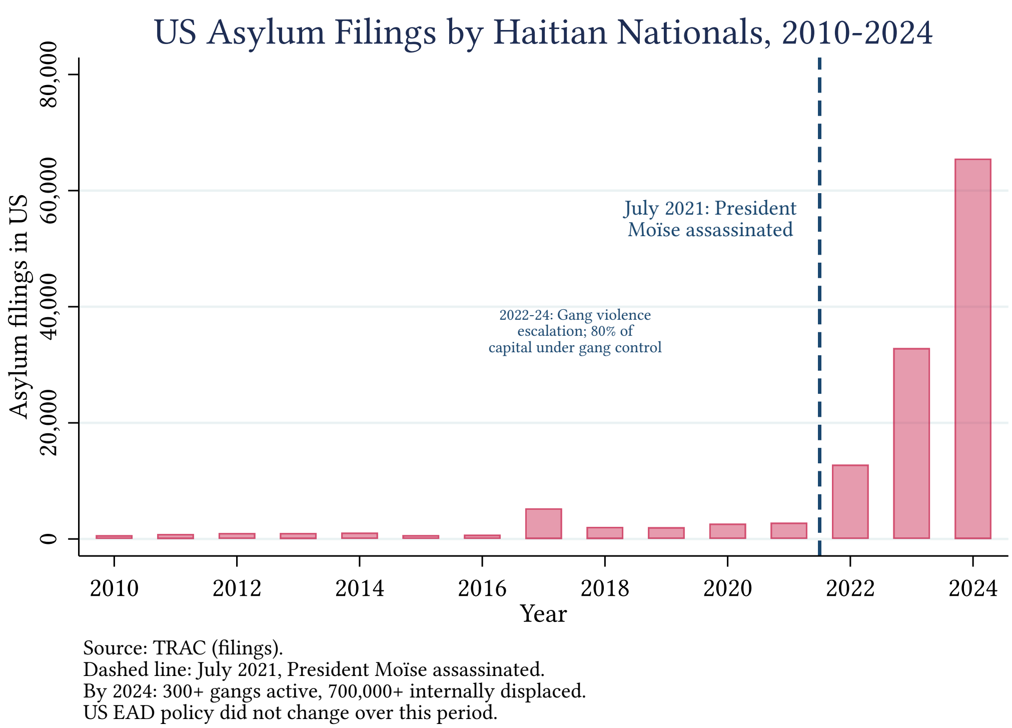

Figure 12: US Asylum Filings by Haitian Nationals and the Post-2021 Security Crisis

In each case, the same EAD rules applied before, during, and after the surge. A long-standing policy is a poor explanation for a sudden, nationality-specific change. The 180-day EAD waiting period has been in effect since 1995. The subsequent liberalizations, extended validity periods in September 2023 and longer automatic renewals in April 2024, *postdate* the onset of the surge and therefore cannot have caused it. In sum, the surge reflects origin-country crises and route changes rather than US employment-authorization policy, and the US nationality-level evidence matches the null-to-small effect that the European cross-country literature has found on the EAD margin.

8 Conclusion

This paper has asked two questions about employment authorization for asylum seekers in the United States, questions that a European-centered literature has left unresolved for the largest destination country in the world. Does an additional asylum seeker raise or lower economic outcomes for existing residents? And does the availability of work authorization itself act as a magnet that substantially changes the number of asylum seekers who arrive? Our answers are consistent with the broader refugee-and-asylum literature but add rigorous US evidence that had been missing.

On the first question, the answer is positive on every margin we measure. Each asylum seeker is associated with roughly 1.1 additional jobs for incumbent workers, and incumbent wages rise. The unemployment rate falls. Means-tested public-benefit reliance declines. The residual share of GDP is consistent with the national capital share, indicating that returns to capital rise alongside labor income. Across nested population groups (native-born, all citizens, all incumbent workers), effects strengthen as the reference group broadens, a pattern that points to complementarity between asylum seekers and the existing labor force rather than displacement. These findings sit at the upper end of the positive-effect range reported in prior refugee-specific studies of European and Latin American settings, and the implied GDP effect per asylum seeker is of the same order of magnitude as projections from the Congressional Budget Office and the Penn Wharton Budget Model.

On the second question, the US nationality-level evidence aligns with the cross-country European literature: work-authorization policy is not a meaningful driver of asylum flows. The 1994 US reform, invoked as the principal domestic precedent for a large deterrent effect, does not support such an effect on closer inspection, once the affirmative-to-defensive shift is accounted for and Central American episodes are separated from the rest. The 2021–2024 surge is concentrated in crisis-origin nationalities with historically above-average grant rates, and its nationality-specific timing aligns with origin-country events and regional route changes rather than with unchanged US EAD rules. An administrative claim that high rates of work-permit receipt among denied asylum applicants reveal a magnet effect does not survive scrutiny either: with adjudication times averaging more than two years, the pattern follows from queue length alone and carries no information about applicant intent. Taken together, these episodes do not support an extrapolation from the European literature in any new direction. On the EAD margin, the magnet story is weak.

Several caveats apply. The GDP effect is imprecisely estimated ($p = 0.07$); the employment and unemployment effects are precisely estimated and do not depend on the GDP result. The shift-share design identifies local effects; general-equilibrium effects propagating across commuting zones are not captured. The surge window is short by the standards of long-run refugee integration studies. The magnet analysis is descriptive rather than causal: it documents patterns inconsistent with the magnet hypothesis, but cannot rule out that EAD availability plays a role at the margin smaller than our identification can detect.

Notwithstanding these caveats, the evidence on both margins points in the same direction, and in the same direction as the existing refugee-and-asylum literature. Restricting employment authorization for asylum seekers would not, on the evidence we have, improve economic outcomes for natives, and it would not materially reduce asylum filings. It would, however, impoverish the people who file them, in keeping with the European evidence that employment bans generate persistent scarring effects on refugees' own outcomes that outlast the bans themselves (Fasani et al. 2021; Marbach et al. 2018; Hainmueller et al. 2016; Ahrens et al. 2023). ■

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Appendix

A1 Additional Tables and Figures

Appendix Table A1: GDP, Employment, Wages, and Unemployment: OLS and Surge B

	Real GDP	Native-born		Citizens		Incumbent		Unemp
		Emp	Wage	Emp	Wage	Emp	Wage	
Panel A: OLS (2021–2023)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Asylum flow per capita	0.881	1.216***	2.373*	1.488***	3.609***	2.199***	5.188***	-1.712***
	(2.092)	(0.443)	(1.328)	(0.548)	(1.133)	(0.618)	(0.888)	(0.628)
Observations	605	451	451	451	451	451	451	605
Panel B: IV (2021–2024)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Asylum flow per capita	4.977	0.178	1.331	0.407	2.171	1.286***	4.347***	-1.668***
	(3.343)	(0.543)	(1.464)	(0.521)	(1.349)	(0.461)	(1.075)	(0.403)
Observations	608	451	451	451	451	451	451	609
First-stage F	11.5	15.7	15.7	15.7	15.7	15.7	15.7	11.5
Panel C: OLS (2021–2024)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Asylum flow per capita	0.006	0.252	1.340	0.305	2.001**	0.967**	3.516***	-1.478***
	(1.587)	(0.484)	(0.965)	(0.508)	(0.851)	(0.464)	(0.778)	(0.547)
Observations	608	451	451	451	451	451	451	609

Notes: See notes to main table. Each panel reports a different estimator or time period. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Appendix Table A2: Means-Tested Public Benefit Participation: OLS and Surge B

	Native-born		Citizens		Incumbent	
	Medicaid	Welfare	Medicaid	Welfare	Medicaid	Welfare
Panel A: OLS (2021–2023)						
	(1)	(2)	(3)	(4)	(5)	(6)
Asylum flow per capita	-1.257*	-0.142	-1.160**	-0.362*	-1.156**	-0.424**
	(0.665)	(0.236)	(0.582)	(0.201)	(0.553)	(0.172)
Observations	451	451	451	451	451	451
Panel B: IV (2021–2024)						
	(1)	(2)	(3)	(4)	(5)	(6)
Asylum flow per capita	-0.134	-0.104	-0.203	-0.368	0.021	-0.386
	(0.690)	(0.309)	(0.696)	(0.312)	(0.665)	(0.310)
Observations	451	451	451	451	451	451
First-stage F	15.7	15.7	15.7	15.7	15.7	15.7
Panel C: OLS (2021–2024)						
	(1)	(2)	(3)	(4)	(5)	(6)
Asylum flow per capita	0.192	0.167	0.241	-0.020	0.278	-0.019
	(0.590)	(0.233)	(0.571)	(0.196)	(0.474)	(0.169)
Observations	451	451	451	451	451	451

Notes: See notes to main table. Each panel reports a different estimator or time period. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table A3: Recent Non-Citizen Arrivals: Employment and Wages

	Surge A (2021–2023)		Surge B (2021–2024)	
	(1) Emp	(2) Wage	(3) Emp	(4) Wage
Asylum flow per capita	-4.931	-15.146	4.238	-23.611
	(9.230)	(27.630)	(7.982)	(24.450)
Observations	217	185	224	191
First-stage F	12.0	11.5	8.9	8.4

Notes: Recent non-citizen arrivals: foreign-born non-citizens (citizen = 3) who entered the US after 2020. Same IV specification as main table. Cells with fewer than 50 ACS observations excluded. * p<0.10, ** p<0.05, *** p<0.01.

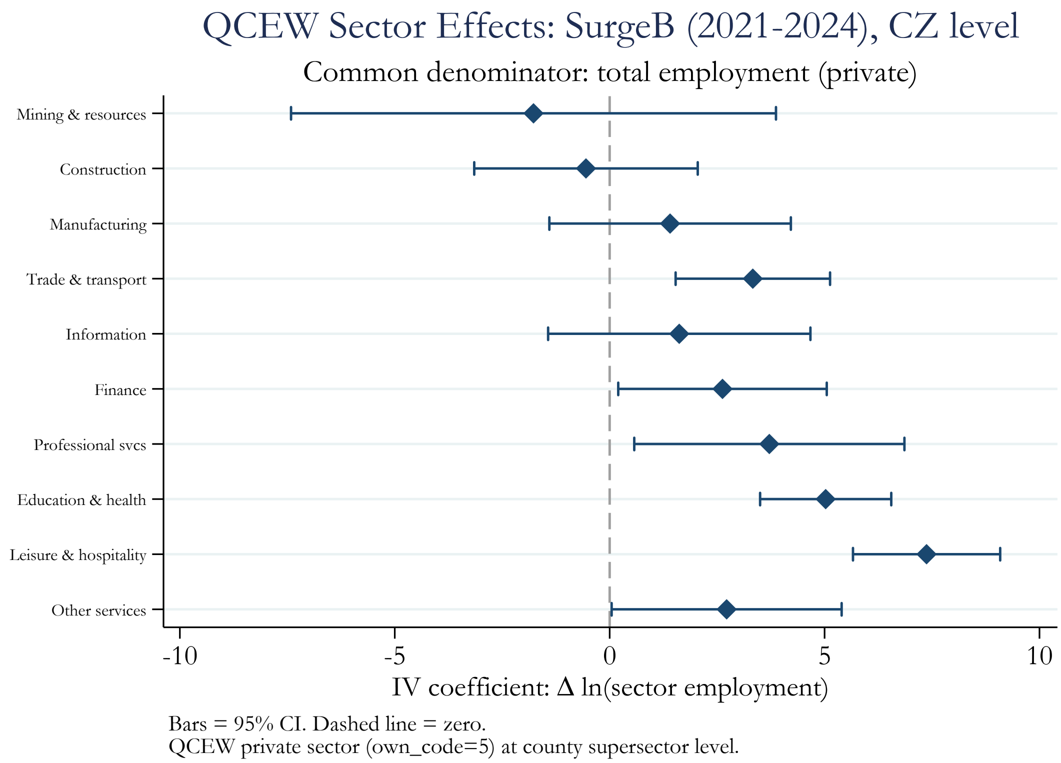

Figure A1: QCEW Sector Employment Effects: Surge B (2021–2024)

A2 Robustness Tables

Appendix Table A4: Weak-IV Robust Inference: Anderson–Rubin Tests (Surge B)

Specification	$\hat{\beta}$ (Wald)	SE	Wald p	AR χ^2	AR p	KP F	N
LAUS_unemp_SurgeB	-1.6683	0.4027	0.0000	3.907	0.0481**	11.5	609
GDP_SurgeB	4.9766	3.3433	0.1371	3.351	0.0672*	11.5	608
Inc_emp_SurgeB	1.2863	0.4615	0.0055	2.898	0.0887*	15.7	451

AR p < 0.05: result robust to weak instruments (AR confidence set excludes zero).

Appendix Table A5: Robustness to Stepwise Controls: IV Estimates (2021–2023)

	(1)	(2)	(3)	(4)	(5)
Panel A: Real GDP (Δ ln GDP)					
	(1)	(2)	(3)	(4)	(5)
Asylum flow per capita	5.530*	1.728	5.180**	4.378*	0.450
	(3.079)	(2.605)	(2.486)	(2.412)	(1.433)
Observations	605	605	604	604	604
First-stage F	15.4	12.6	18.0	18.4	20.8
Panel B: Incumbent Employment Rate (Δ emp rate)					
	(1)	(2)	(3)	(4)	(5)
Asylum flow per capita	2.765***	3.346***	2.940***	2.671***	2.773***
	(0.654)	(0.695)	(0.903)	(0.958)	(0.904)
Observations	451	450	450	450	450
First-stage F	20.3	16.0	19.5	19.1	29.3
Panel C: Unemployment Rate (Δ unemp rate)					
	(1)	(2)	(3)	(4)	(5)
Asylum flow per capita	-1.682***	-2.049***	-1.616***	-1.394***	-1.224***
	(0.466)	(0.476)	(0.446)	(0.458)	(0.447)
Observations	605	604	604	604	604
First-stage F	15.3	12.4	18.0	18.4	20.8

Notes: Each cell reports the IV coefficient on asylum flow per capita from a separate regression. Column 1: baseline specification (no controls). Column 2: adds BEA compensation growth 2017–2019. Column 3: adds 2019 unemployment rate and log compensation per capita. Column 4: adds accommodation and food services share of total compensation. Column 5: adds state fixed effects (assigned to each CZ via modal county). All regressions use a Bartik shift-share instrument, are weighted by 2019 CZ population, and use heteroskedasticity-robust standard errors. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table A6: Labor Market Effects: IV with Pre-Trend Control (2021–2023)

	(1) Real GDP	(2) Inc emp	(3) Inc wage	(4) Unemp
Asylum flow per capita	1.728	3.346***	5.459***	-2.049***
	(2.605)	(0.695)	(1.418)	(0.476)
Comp. growth 17–19	0.553***	-0.087**	0.137	0.052**
	(0.132)	(0.037)	(0.087)	(0.022)
Observations	605	450	450	604
First-stage F	12.6	16.0	16.0	12.4

Notes: IV long-difference regressions (2021–2023) with BEA compensation growth 2017–2019 as control. Same specification as main table. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table A7: Public Programs: IV with Pre-Trend Control (2021–2023)

	Native-born		Citizens		Incumbent	
	(1) Medicaid	(2) Welfare	(3) Medicaid	(4) Welfare	(5) Medicaid	(6) Welfare
Asylum flow per capita	-1.574**	-0.328	-1.345*	-0.605**	-1.373*	-0.726**
	(0.764)	(0.289)	(0.754)	(0.300)	(0.744)	(0.315)
Comp. growth 17–19	-0.069**	-0.027*	-0.086***	-0.025*	-0.074**	-0.015
	(0.033)	(0.015)	(0.032)	(0.015)	(0.032)	(0.014)
Observations	450	450	450	450	450	450
First-stage F	16.0	16.0	16.0	16.0	16.0	16.0

Notes: IV long-difference regressions (2021–2023) with BEA compensation growth 2017–2019 as control. Same specification as main table. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.