Temporary Legal Protection: Labor Market and Migration Responses *

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Abstract

Temporary Protected Status (TPS) provides work authorization and deportation protection to individuals from countries experiencing armed conflict, environmental disasters, or other extraordinary conditions. In March 2025, 1.3 million people in the United States held TPS. By December 2025, TPS designations for 9 countries had been terminated, affecting approximately 700,000 individuals. Using data on all TPS events between 2000 and 2024, we examine two questions. First, what are the labor market impacts of TPS? By comparing country-by-arrival-year cohorts, we find that TPS increases employment and reduces return migration. Studying attempted terminations, we find that threatening to remove TPS produces employment effects three times larger than introducing it. Second, does TPS incentivize unauthorized migration? While illegal entry and visa overstays respond to initial TPS designations, these responses may be confounded by migration following the underlying disaster. Examining redesignation events—where the eligible arrival date is extended—we find no evidence of increased migration, with effects primarily negative.

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1 Introduction

In March 2025, 1.3 million people in the United States held Temporary Protected Status (TPS), a humanitarian immigration status that protects individuals from deportation and grants them legal work authorization. By December 2025, the Trump administration had terminated TPS designations for 9 countries, affecting approximately 700,000 individuals. These policy changes have reignited longstanding debates about TPS. Critics argue that "temporary" protections have become effectively permanent—Nicaraguans, for example, held TPS for 25 years—and that TPS designations may incentivize unauthorized migration from affected countries. Yet despite these concerns and the scale of recent terminations, we lack systematic empirical evidence on two crucial questions: What are the economic effects of removing work authorization from TPS holders? And does TPS designation actually increase migration flows from designated countries?

To answer these questions, we examine the effects of both granting and removing (or attempting to remove) TPS protections, using data on all TPS events between 2000 and 2024. In total, we observe 12 designation events, 4 redesignation events, 4 termination events, and 6 additional attempted terminations. To be eligible for TPS, an individual must already be present in the US. To examine the effect of receiving TPS, we compare immigrants who arrived in the US in the same year, where immigrants from one country later became eligible for TPS and immigrants from another country did not. To examine the effect of losing (or the prospective loss of) TPS, we compare two immigrants, both with TPS, where one is from a country that experiences a removal (or attempted removal) of TPS status. Finally, to examine whether TPS increases immigration, we compare both legal and illegal migration flows immediately before and after a country receives a TPS designation or status update. Our results suggest that TPS has substantial economic effects. Granting individuals work authorization increases employment and earnings and reduces return migration. Removing (or threatening to remove) TPS has the opposite effect, decreasing both employment and earnings, with effects larger in magnitude that the initial designation. Finally, while illegal entry and visa overstays respond to initial TPS designations, these responses may be confounded by migration following the underlying disaster. Examining redesignation events—where the eligible arrival date is extended—we find no evidence of increased migration, with effects

primarily negative.

Our identification strategy is a cohort-based difference-in-difference strategy. Individuals are only eligible for TPS if they are already present in the United States. As a result, migration decisions are made before TPS outcomes are known. We leverage the arrival cohort of immigrants to identify individuals who become eligible for TPS once TPS is designated for a country, individuals who become newly-eligible once a country is redesignated, and those who lose TPS when a country loses (or has an attempted termination) of its TPS status. We compare individuals from an arrival year x country cohort, before and after the arrival year x country cohort becomes eligible for TPS. For example, Venezuelan migrants who were present in the US on March 8, 2021 became eligible for TPS in March 2021. We compare the change in outcomes between 2022 and 2021 for a Venezuelan migrant who arrived in the US in 2020 to understand the effect of receiving legal status. We compare this migrant to a control group of the change in outcomes between 2022 and 2021 for other migrants who also arrived in the US in 2020 but did not receive TPS status.

We leverage comprehensive administrative data on all TPS designations, extensions, and terminations (both proposed and implemented) to generate a database of TPS events. We then use data from the American Community Survey to estimate the causal effects of legal protection on beneficiaries' economic outcomes and labor market trajectories. We also investigate whether a TPS designation induces subsequent unauthorized migration flows from designated countries—a key concern for policymakers weighing the costs and benefits of humanitarian protection programs—by examining immigrant inflows, border apprehensions, and visa overstays.

Our first result concerns the economic effects of receiving TPS authorization. Using updated econometric methods that account for the correct control group to generate robust findings on the effects of legal status for eligible individuals, we find that across-the-board, receiving legal status increases employment outcomes. This result is in line with the literature that has studied specific TPS designation events in isolation. For example, Orrenius and Zavodny (2015) study the effect for the 2001 designation of El Salvadorans, and Amuedo-Dorantes et al. (2023) study the 2010 and 1999 designations for Haitians and Hondurans respectively. Secondly, we ask whether the threat of losing TPS status affects economic outcomes. We focus on six high profile termination attempts in 2018 which were ultimately

halted or reversed in court. Using our cohort-based design with the ACS data, we compare TPS recipients from countries facing an attempted termination with recipients from countries whose TPS designation was not challenged. We find that termination attempts reduce employment among likely undocumented TPS holders by about 13 percentage points and lower real wages by 30 percent, with no clear effect on cohort size. These results are larger in magnitude that the estimates of gaining TPS, suggesting that policy uncertainty alone can undo the gains from legalization of status even when legal protection ultimately remains in place.

Next, we examine whether TPS generates additional migration. This question is relevant because individuals must be physically present in the US to be eligible for TPS. However, authorization is often extended, and additional arrival cohorts become eligible when a country is redesignated for TPS. For example, Venezuela was originally designated for TPS in March 2021, making any Venezuelans already present in the US on March 8, 2021 eligible. Venezuela was subsequently redesignated in September 2023, extending eligibility to any Venezuelans present in the country by July 31, 2023. Consequently, some Venezuelans who entered the US between March 8, 2021 and July 31, 2023 initially lacked legal authorization but later gained temporary authorization through redesignation. Overall, in 80% of cases, the eligibility period is extended, allowing immigrants who arrive after the initial designation date to become eligible for protection ex post. This creates an arrival window during which migrants may reasonably expect to benefit from future redesignation, even if they arrive without legal status. Examining legal non-immigrant arrivals (I-94s) and visa overstay estimates, we find that overall arrivals and overstay rates increase following TPS designation. We observe similar patterns for apprehensions, with a spike occurring around initial TPS designations.

However, one natural challenge to the interpretation of the results is that migration may be driven by the push factors that led to TPS being declared, rather than the effect of TPS itself. TPS is often, although not always, declared in response to natural disasters. For example, Haiti received TPS status on January 15, 2010, right after the January 12 earthquake, giving protection to individuals who were physically in the US by the day of the earthquake. A large literature has established that migration is often a response to natural disasters (see, e.g., Mahajan and Yang (2020)). To isolate whether it is TPS itself—and the

increase in salience of legal authorization—we ask if migration responds to redesignation and termination events. Redesignation events, if they happen, usually occur a period after the initial designation. By studying migration responses around the redesignation window, we hold the overall effects of the natural disaster constant, and can then isolate the effects of the TPS announcement itself. We find no evidence of increased migration in response to redesignations—indeed, effects are primarily negative. We find no evidence that extending TPS increases unauthorized immigration.

A large literature has focused on the impacts of legal status on outcomes. Other studies of TPS study a specific set of TPS events and tend to find positive employment and earnings gains (Orrenius and Zavodny, 2015; Amuedo-Dorantes et al., 2023; Harris and Jerch, 2025). Kaushal (2006) shows that a 1997 amnesty program (NACARA) targeted at Central American and Caribbean populations increased earnings, particularly among people with a high school degree, and had no significant effects on employment. Studies of the effects of giving legal status to childhood arrivals through Deferred Action for Childhood Arrivals (DACA) finds positive effects on mobility and occupational investment (Villanueva Kiser and Wilson, 2024). Kroft et al. (2025) find labor market gains for legal immigrants who gain permanent resident status in Canada. Work outside the US has found that Venezuelan migrants afforded work authorization and legal protection in Colombia led to improvements in migrant well-being with minimal negative effects on local workers (Bahar et al., 2021; Ibáñez et al., 2025), and a legalization program in Spain led to an increase in formal employment, and a decrease in informal employment, among those directly affected (Elias et al., 2025). There is less evidence on the removal of authorizations; an exception is Szurop (2025), who studies threats of removing DACA on labor market outcomes, finding negative effects on employment, labor force participation, and earnings.

Despite the policy concern, research on whether legalizing immigration leads to more migration—the "magnet" hypothesis—generally finds little evidence of increased migration following legal status. Orrenius and Zavodny (2003) study the immigration response after the 1986 legalization of 2.7 million immigrants, finding no long-run change in immigration patterns. Larramona and Sanso-Navarro (2016) and Elias et al. (2025) find little evidence of a magnet effect in Spain following a 2005 legalization.[1] Relative to this literature, we are

[1] Helms and Leblang (2022) ask whether TPS leads to an increase in remittances, hence reducing further

able to examine country-specific policy changes and therefore control for overall migration patterns.

The rest of the paper proceeds as follows. Section 2 discusses the TPS program and the history of TPS events and redesignations. Section 3 outlines the data and empirical strategy. Section 4 presents our results on the labor market effects of TPS and the indirect effects on migration. Section 5 concludes.

2 Temporary Protected Status

2.1 Background to the program

Temporary Protected Status (TPS) was established in the Immigration Act of 1990, providing a statutory framework for temporary protection from deportation. It formalized protections that had previously been granted through ad hoc executive actions such as Extended Voluntary Departure (Wilson, n.d.; Martin, 1991). The program allows nationals from designated countries who are already in the United States to remain and work legally when conditions in their home country—armed conflict, environmental disaster, or other extraordinary and temporary conditions—make return unsafe (Weitzhandler, 1993). A major rationale for establishing TPS was an influx of Central American immigrants, particularly El Salvadoreans, fleeing civil war and political turmoil (Pear, 1990). El Salvador became the first country designated under the new statute.[2] Unlike earlier legalization programs such as IRCA or NACARA, which provided permanent residence, TPS was designed as a temporary measure with no automatic path to a green card (Kaushal, 2006). By 2025, approximately 1.3 million people from 16 countries held TPS.[3]

The Secretary of Homeland Security designates countries for TPS for initial periods of 6, 12, or 18 months. Before expiration, the government must decide whether to extend (continue protections for existing beneficiaries), redesignate (update the arrival cutoff date, mak-

migration. They show that TPS increases remittances, and apprehensions and asylum applications decrease when remittances increase.

[2]The original TPS designation for El Salvador expired in 1992. El Salvador then received Deferred Enforced Departure until 1995. El Salvador received a second grant of TPS in 2001 after the 2001 earthquakes. We include the second designation in the analysis in the paper.

[3]Subsequent termination attempts involving over 600,000 TPS holders have been challenged in court.

ing more recent arrivals eligible), or terminate (end protections and restore beneficiaries to their prior immigration status). In practice, many designations have been extended repeatedly—some for decades. Concerns that TPS would become a magnet for immigration and that temporary status would be extended indefinitely were raised in the initial congressional debate (Weitzhandler, 1993; Frelick, 2020). Between 2000 and 2023, there were twelve initial designations, four redesignations, four terminations, and six termination attempts.

Eligibility is determined by arrival cohort: only individuals who were physically present in the United States by the government-set cutoff date can apply for TPS. As such, one of the defining features of TPS is that it only provides relief to individuals already inside the US; TPS was not designed to provide relief for people actually experiencing a natural disaster or conflict, but rather as a respite from deportation for people who could not be returned to their home country because of a crisis. This creates sharp distinctions between "early arrivals" (potentially eligible) and later arrivals from the same country (ineligible unless a redesignation occurs). Eligible individuals must file Form I-821 and pay fees to receive both protection from deportation and also work authorization. TPS holders can obtain Social Security numbers and state identification, but are excluded from major federal safety-net programs such as SNAP and TANF, and have no direct path to permanent residence or family-sponsored migration.[4]

TPS operates alongside other protection mechanisms but fills a distinct gap. Asylum and refugee status both require demonstrating individual persecution based on one of five specific grounds: race, religion, nationality, political opinion, or membership in a particular social group (Bergeron, 2014).[5] Simply experiencing a natural disaster or being from a country in civil war does not automatically make an individual eligible for asylum or refugee status unless they can demonstrate harm on one of these five bases. TPS addresses this gap by providing country-level relief based on conditions that make return unsafe, regardless of whether individuals meet asylum's individualized persecution requirements.[6]

[4]In some states, TPS holders receive access to subsidized health insurance.

[5]Refugees differ from asylum seekers in that they apply from outside the United States, while asylum seekers present themselves at the US border seeking asylum.

[6]The executive branch can also grant Deferred Enforced Departure or country-specific humanitarian parole (such as the Biden administration's program for Cuba, Haiti, Nicaragua, and Venezuela), which provide similar temporary protections but without statutory constraints. Other programs have provided permanent legal residence for populations from specific countries, often as a result of past discrimination in asylum cases, such as the Nicaraguan Adjustment and Central American Relief Act and the Haitian Refugee Immigrant Fairness

2.2 TPS designations, redesignations, and terminations: 1991-2025

Figure 1 shows the evolution of Temporary Protected Status (TPS) designations, redesignations, and terminations across all eligible countries from 1990 to 2025 using official Federal Register Notices and Congressional Research Service Reports. This timeline allows us to identify which immigrant cohorts were eligible for work authorization and deportation protection in each survey year. Details of the data construction process are in Appendix B.

The reasons for TPS designation vary, but it is generally triggered by a single major event, such as an earthquake or hurricane, that attracts humanitarian attention to the country, although other designations respond to long-running conflict or political crises. TPS began in the early 1990s with designations for countries like El Salvador, Liberia, and Somalia, with a second wave of designations at the end of the decade in Africa and Central America. Many countries experienced repeated redesignations, reflecting the cyclical nature of TPS policy. Permanent terminations were concentrated in the early 2000s for countries such as Angola, Kosovo, and Bosnia, whereas termination attempts between 2018 and 2020 for El Salvador, Haiti, Honduras, Nicaragua, and Sudan were subsequently halted by court orders before they went into effect. During the 2nd Trump term, there was a subsequent wave of terminations and attempted terminations, some of which went into effect.

Figure 2 shows how different arrival cohorts get treated by TPS. Panel (a) shows whether an arrival cohort gets any TPS as a function of when the cohort arrives relative to the first designation. All cohorts who arrive prior to the first designation are eligible for TPS, which is shown on the graph as a horizontal line at 1. However, many cohorts who arrive after the initial designation ex-post end up getting TPS protection through later redesignations. For example, 80% of the time, an individual who arrives the year after the first designation will be covered by a later redesignation, and in 20% of cases, an individual who arrives four years after the first designation will end up eventually being covered by TPS protections. Panel (b) shows the length of coverage (truncating at 2025 for countries that still have active protection). On average, initially-eligible individuals receive nine years of TPS coverage. Individuals who arrive one year after the initial designation receive six years, and individuals who arrive four years after the initial designation receive two years of coverage.[7]

Act (Kaushal, 2006).

[7]Figure 2 focuses on the subsample of 12 countries used in Table 2. The same figure for the universe of

That some individuals who migrate after an initial TPS designation later become eligible for TPS has led to concerns that TPS acts as a magnet for unauthorized migration. However, the extension of TPS to later arrivals may also reflect an ex post policy adjustment to protect individuals who experienced the disaster in their home country and only migrated to the US afterward. Given the lack of alternative channels to provide relief for people experiencing natural disasters, TPS redesignation may serve as one available policy lever, despite nominally "rewarding" individuals who migrated without authorization.

We examine whether TPS leads to independent migration over and above the push factors by testing whether migration responds to the timing of TPS redesignations, which typically occur several months after the initial disaster and so the direct push factor is less likely to be relevant. We also examine whether migration responds to TPS terminations, which may signal a permanent end to the possibility of TPS.

3 Data and Empirical Strategy

This section describes the data. Our primary source of data for estimating the economic effects of TPS is the ACS. We also use apprehension and immigration data on inflows and visa denials, approvals, and overstay rates to estimate migratory responses.

3.1 ACS data

We begin by identifying immigrants in the 2000 Decennial Census subsample and the 2001-2023 American Community Survey. We obtain their year of arrival to the U.S. and construct a sample of 110 countries, 12 of which received TPS designations.[8] We restrict our sample to non-citizen immigrants ages 18-64 living in households rather than group quarters who arrived in the U.S. after 1990.[9] We identify different labor market outcomes, such as wages, employment, and labor force participation, as well as other outcomes such as location. We

countries is given in Appendix Figure 1. Average TPS length is longer, reflecting that some of the early designations were constantly extended (e.g., both Nicaragua and Honduras received TPS designations in 1999 that remained active until 2025, a total of 26 years.

[8]Immigrants from 4 TPS-designated countries were not surveyed during our period of interest. These countries are Angola, Burundi, Montserrat, and Guinea-Bissau.

[9]Cohorts before 1990 may have been affected by various legalization programs such as NACARA

also count annual stocks of immigrants from each country of origin.

Given TPS provides deportation protections to people who do not have legal status, our primary sample is the sample of people who are likely undocumented. We use the methodology in Borjas (2017) to identify these individuals in the ACS. We consider robustness to alternative sample definitions such as all non-citizen immigrants, as well as undocumented immigrants besides likely refugees.[10]We then construct a pseudo-panel of average labor market outcomes by pooling immigrant cohorts into cells at the country-of-origin $\times$ year of arrival $\times$ survey calendar year level. We describe this process in detail in Appendix B.

Although the ACS is the preeminent source of data on economic outcomes for residents of the US, we acknowledge some data quality issues with immigration data in ACS. Van Hook and Bachmeier (2013) finds some evidence of citizenship misreporting, particularly among recent arrivals and Mexicans. Jensen et al. (n.d.) find that the ACS may undercount recent Hispanic immigrants, particularly male ones. An additional concern is that the stock of individuals from country by year-of-arrival sometimes increase over time, which is impossible given that new arrivals belong to later cohorts.[11] Our identification assumption throughout the analysis will be that any differential response or reporting is invariant to TPS status.

3.2 Immigration data

Arrivals. To measure arrivals, we use data from the I-94 International Visitor Arrivals program, produced by the National Travel and Tourism Office in coordination with the Department of Homeland Security and Customs and Border Protection (International Trade Administration, n.d.). The data consists of monthly counts of international visitors to the

[10]Liu and Song (2020) compares statistics about DACA immigrants from government data based on two methods of determining who is undocumented: the method in Borjas (2017) or all Hispanic immigrants. The authors show that the Borjas method yields estimates that line up more closely with benchmark figures from other data sources. To adjust for likely refugee counts, we follow the procedure in **?** to construct an indicator for likely refugee status by country of origin and arrival cohort. Using refugee arrival statistics from Department of Homeland Security reports and immigrant arrival counts from the ACS, we compute the ratio of refugee to total immigrant arrivals for each country year cell and classify as likely refugees all immigrants from country years in which refugees account for at least half of new arrivals.

[11]See Appendix Figure 2 for an illustration of this in between the 2000 and 2010 ACS. Harris and Jerch (2025) also report this issue and suggest it is due to survey response. Lubotsky (2007) discuss how the wording of the arrival question may be unclear for individuals who have made multiple trips. Using matched administrative data, Akee and Jones (2024) estimate that up to 40% of recent migrants have left the US after ten years.

United States by country of residence or citizenship for the 2000-2024 period, based on electronic I-94 records for foreign visitors who stay at least one night and enter under temporary travel visas. We construct a country-by-month measure using total visitor arrivals that we map to our data on the timing of TPS-related decisions.

Visa overstays. To study how TPS shapes the behavior of short-term visitors after arrival, we extract data from the Department of Homeland Security's Entry/Exit Overstay Reports (U.S. Customs and Border Protection, 2025). These reports match electronic I-94 arrival and departure records for air and sea travelers and provide country-specific estimates of nonimmigrant overstays. The data covers fiscal years 2014 to 2024 for all countries. We observe the number of expected departures for B1/B2 visitors and other nonimmigrant classes and the total number of overstays.[12] We focus on the suspected in country overstay rate, defined as suspected in country overstays over expected departures. Some countries exhibit high overstay rates, of up to 10%. In the DHS overstay data, a person who extends or changes status, (e.g., by obtaining TPS or being granted asylum), should no longer be counted as an overstay, although some may still appear as suspected in country overstays if their status change is not fully captured in the administrative records due to delays or incomplete matching across systems.

Visa refusals. To study how TPS may affect consular screening, we use data on nonimmigrant visa refusals from the U.S. Department of State's Visa Office (U.S. Department of State, n.d.).We obtain a measure of adjusted refusal rates for B class tourist and business visas by nationality from 2006 to 2024.[13] This allows us to get counts of refusals by comparing with issued visas.

3.3 Summary statistics

Table 1 shows summary statistics for the sample of individuals who are eligible for TPS. On average, an individual who is eligible for TPS has spent 6.5 years in the US at the time of designation, with this varying from 3.7 years (Afghanistan) to 13.3 years (Guinea). Around 42%

[12]The data is decomposed into out of country overstays, where the traveler eventually departed late, and suspected in country overstays, where the authorized period of admission has expired and DHS has no recorded departure or status change. Out of country overstays mainly reflect timing issues in exit recording.

[13]The adjusted refusal rate is constructed by the Visa Office and it relates refusals to applications in a way that is consistent across years and is used for evaluating eligibility for the Visa Waiver Program.

of TPS-eligible individuals are recent arrivals with less than five years in the US. At the time of TPS, 71% of the sample is employed, earning an average of $12.24 per hour.[14] Average years of education is 11.3, and 28% of individuals are fluent in English. The results are highly similar when expanded to all immigrants (and not just likely undocumented immigrants) as in Appendix Table A1.

Appendix Table A2 presents summary statistics for the likely undocumented population in 2010, splitting out by TPS status. In 2010, those with active TPS had been in the US on average 14 years. People who would eventually be eligible for TPS had been in the US for 7 years. On average, those eligible for TPS had better economic outcomes (for example, employment rates of 79%) than those who either were not eligible (67%), were eventually eligible TPS (65%), or arrived after a TPS designation (72%).

How many people actually take up TPS designation? Appendix Table A3 shows different estimates of the size of the eligible population by year. Column (1) reports the estimates of the eligible population that the Federal Register provides at the time of designation. Column (2) provides the number of immigrants from that country in the ACS, and columns (3) and (4) make adjustments to the ACS number to give the estimated size corresponding to the samples of likely undocumented immigrants and the likely undocumented net of refugees. Column (5) provides the number of citizens who have received TPS approximately two years after the initial redesignation, according to government documents. Columns (6) and (7) then provide estimates of the TPS take-up rate, using either the federal estimate as the denominator or the ACS likely undocumented population as the denominator.

The federal estimates of the population size and the cohort size in the ACS are often inconsistent. For example, the federal estimate for the number of Afghans eligible for TPS was 72,500. In the ACS, the total population size was 173,000. However, applying the likely undocumented restrictions reduce the estimate of the number to 13,400. Two years after the designation, 8,245 individuals had taken up TPS. This leads to a take-up rate between 62% (using the ACS likely undocumented number as the denominator) and 11% (using the federal estimate as the denominator). Overall, the take-up rate of TPS two years after the initial designation ranges from 11% to 110% using federal estimates of the size of the population, and from 8%-120% using ACS estimates of the size of the population.

[14] Wages are adjusted to 2006 equivalents.

3.4 Empirical Strategy

We study the causal effects of Temporary Protected Status (TPS) on immigrant outcomes, such as employment, wages, and overall presence in the US. We exploit quasi-experimental variation from the timing of TPS designations across origin countries, together with cohort-specific eligibility cutoffs that depend on immigrants' arrival dates.

We index our data at the country of origin c, arrival cohort a, and observation year t level. For each (a, c, t) cell we observe an outcome Y_{act}, for example the employment rate of immigrants from country c who first arrived in year a and are observed in survey year t.

For each TPS country we denote by T_c^D the calendar year of its first TPS designation, by T_c^T the year in which TPS is terminated, and by A_c^{*D} the continuous residence cutoff that determines eligibility at the time of the initial designation. Arrival cohorts with $a \leq A_c^{*D}$ are "early arrivals" who may qualify for TPS, while cohorts with $a > A_c^{*D}$ are "late arrivals" who remain ineligible at the initial designation.

An eligible cohort gains TPS treatment beginning the calendar year immediately after the TPS designation.[15] We define the treatment indicator D_{act}^D as follows:

$$D_{act}^D = \begin{cases} 1 & \text{if } a \leq A_c^{*D} \text{ and } t > T_c^{\mathrm{D}} \text{ and } t < T_c^{\mathrm{T}} \\ 0 & \text{otherwise.} \end{cases}$$

We consider these cohorts as treated, regardless of how many people in them ultimately apply for or acquire TPS status. A TPS termination closes eligibility for all cohorts from that specific country. If no termination takes place, cohorts remain treated for the rest of the sample period. In our analysis, we trim any survey years after a TPS termination for treated cohorts.

In the difference-in-difference, we compare treated groups (early arrivals who become eligible for TPS) to a control group consisting of (a) not-yet-treated individuals (individuals from the same countries before their country was designated for TPS) and (b) never treated individuals (individuals from a country that never gets TPS, restricted to Asia, Africa, Latin America, and non EU Europe). We drop always-treated countries (those with TPS prior to

[15] The one year lag reflects administrative delays in TPS approvals and work authorizations and the idiosyncratic timing of notices at different points of the year.

2001).[16] We exclude late arrivals in ever-designated TPS countries from the analysis because their migration decision may be itself a response to the TPS designation. Finally, we exclude observations from a country once a successful termination has taken effect.

We estimate the following difference-in-difference model:

$$Y_{act} = \beta D_{act} + \lambda_{ac} + \tau_t + \varepsilon_{act} \tag{1}$$

where Y_{act} is an outcome (e.g. employment rate). λ_{ac} are country-by-arrival cohort fixed effects, which absorb time invariant differences across specific country cohort cells, such as selection into migration or composition at entry.[17] τ_t are year fixed effects accounting for common shocks to cohorts from all countries in a specific year. Standard errors are clustered at the country by arrival year level.

Our coefficient of interest is β, which is a within-cohort estimator, identifying the difference in outcomes of early arrival cohorts in TPS countries before and after a TPS designation to the evolution of outcomes of analogous cohorts in never treated and not yet treated countries. The identification assumption is that, conditional on the fixed effects, the outcomes of early arrivals in TPS countries would have followed the same trajectory as the comparison groups in the absence of TPS. We estimate equation 1 using the estimator from Gardner et al. (2024), which is closely related to the imputation method of Borusyak et al. (2024). The imputation method delivers an estimate of β that avoids the weighting issues that arise in conventional two way fixed effects estimators with staggered treatment timing. The coefficient on TPS, β, estimates the total effect of receiving TPS. This may be driven through the legal protections of TPS. However, it may also be the case that a natural disaster, and not just TPS status, leads immigrants to independently increase labor market outcomes.

We estimate event studies using the following model:

$$Y_{cat} = \sum_{k \neq -1} \gamma_k \, \mathbf{1}\{a \leq A_c^{*D}, \; a - T_c^D = k\} + \lambda_{ca} + \tau_t + \varepsilon_{cat} \tag{2}$$

[16] We also drop Liberia and South Sudan because of their complicated TPS histories. Liberia has been covered by a patchwork of TPS and Deferred Enforcement Departure, with TPS being terminated and re-granted over time. South Sudan split from Sudan when Sudan had TPS, meaning that many South Sudanese people would have been covered by Sudanese TPS before officially receiving it.

[17] These fixed effects account for country-specific time-invariant characteristics and differences in outcomes driven by being in the US for a longer time (e.g., more time to learn English or build a job network).

where k indexes the years between the cohort's arrival year and the year of its country's TPS designation. The indicator $\mathbf{1}\{a \leq A_c^*,\ a - T_c^D = k\}$ equals one for TPS-eligible cohorts whose arrival year is k years away from the designation. The coefficients γ_k track the path of outcomes for eligible cohorts relative to this reference cohort, which allows us to assess pre-trends and the dynamics of TPS effects over time.

A key attribute of TPS is that individuals already have made immigration decisions before the declaration of TPS. Therefore, any endogenous decision into whether to migrate, and what type of people migrate, is not affected by TPS.

Aggregate treatment and terminations. For outcomes observed only at the country-of-origin by year level, such as arrivals, visa overstays, and visa refusals, we define treatment at the time (month or year) of the TPS designation. Under this definition we abstract from individual eligibility requirements and assume that treatment operates through migration incentives for residents of TPS-designated countries.

For these outcomes we also extend the design to study the effect of TPS termination announcements. We distinguish between successful terminations and announcements that are later halted or overturned in court, which we code as termination attempts. Depending on the specification, we leverage between 3 and 9 terminations and 5 or 6 termination attempts. In these regressions we compare outcomes in terminated TPS countries to those in not-yet-terminated and never-terminated TPS countries.

4 Results

4.1 Labor market results: Initial designations

We start by considering the effects of initial designations on the labor market.

Figure 3a reports event study estimates of the effect of TPS designations on employment. Before the first TPS designation, employment rates for immigrants who will later receive TPS move closely with those of the control group. After designation, the employment rate for the treated group increases. Figure 3b considers the same plot for the logarithm of hourly wages. Pre-designation coefficients are close to zero, while post-designation effects are mixed and not precisely estimated. Finally, 3c describes the effect of TPS on population size, measured

as the number of individuals in each country–by–arrival year cohort cell. Population can change because of a large initial cohort or reductions in out-migration or deportation. In this case we observe a negative pre-trend, which works against finding positive effects. Despite this, there is a clear increase in cohort size following TPS designation.

Table 2 reports the regression estimates. Column (1) shows that TPS designation raises the probability of employment by 4.5 percentage points, from a baseline employment rate of 69.8%. Column (2) allows the effect to vary with years in the United States and years of TPS eligibility. The coefficient on years with TPS implies that employment increases by about 0.1 percentage points for each additional year under the program, while the coefficient on years in the United States is of similar magnitude and opposite sign, suggesting that more established migrants already have stronger employment relationships and so the benefit of formal legal status is smaller.

Columns (3) and (4) present the results for the logarithm of hourly wages. Column (3) shows no average wage effect of TPS when we pool all workers, but this masks important heterogeneity by time in the United States and time with TPS. In Column (4), each additional year of TPS is associated with higher wages, while the wage effect is smaller for migrants who have been in the United States longer, again indicating that the gains from legal status are concentrated among less established migrants.

Finally, Columns (5) and (6) ask whether TPS leads to an increase in the population size. We expect TPS to matter through different channels. First, relief from deportation mechanically raises the number of people who remain in the United States. Second, TPS is often correlated with disasters in the home country, which may reduce the rate of return migration. TPS may also make staying in the United States more attractive because of better job prospects and the security of legal status due to protection from deportation. Consistent with these mechanisms, we observe an average increase in cohort size of approximately 21% after TPS. The population response is smaller for more established immigrants and larger for cohorts that spend more years under TPS protection.

Appendix Table A4 shows that the wage and population responses to TPS are stronger for men, with smaller but still significant effects on employment. By contrast, Appendix Table A5 shows only modest heterogeneity in labor market responses by education, while population effects are larger for individuals without a high school diploma.

Robustness. Appendix Figure 3 compares our event study estimates across several difference in differences estimators that account for heterogeneity in treatment effects; the patterns for employment, wages, and population are similar across methods. Appendix Table A6 replicates the baseline regressions for broader immigrant samples, including non citizen immigrants in Columns 4–6 and all immigrants in Columns 7–9. The results are stable under these alternative definitions and, if anything, wage effects become larger. Importantly, Columns 10-12 validate our treatment by showing that the results are statistically insignificant when applying a sample of immigrant citizens, who should not be affected by TPS. Appendix Table A7 shows that the results are unchanged when using concurrent ACS weights or no weights. Table A8 shows that the results remain similar when excluding likely refugees from the sample.

4.2 Labor market results: Attempted terminations

The effects of receiving TPS were positive for individuals. What about the effect of removing TPS protections?

Over the sample period we observe 4 terminations (Bosnia-Herzegovina, Burundi, Montserrat, and Angola) and 6 attempted terminations (El Salvador, Haiti, Honduras, Nicaragua, Nepal, and Sudan). The four successful terminations are for countries with very few people in the United States or located at the very beginning of the sample period, creating issues with sample size. Thus, we focus on the effects of attempting a TPS termination. These attempted TPS terminations all occurred between 2017 and 2020 during the first Trump administration. All six of those attempts were halted or reversed by the courts before they went into effect. Although the terminations were eventually not finalized, it took approximately 10 months for the court to issue an injunction guaranteeing continued protections.[18]The resulting media coverage and altered expectations of migrants and employers about future legal status may plausibly have affected labor market outcomes.

We analyze the impact of attempting to remove TPS protections using the ACS cohort framework, focusing on likely undocumented immigrants from countries that received TPS

[18]The terminations were announced October 2017 (Sudan), November 2017 (Haiti), December 2017 (Nicaragua), and January 2018 (El Salvador). A preliminary injunction was issued in this case October 3, 2018, which allowed TPS recipients to maintain their legal status ACLU (2018).

through an initial designation. The treatment indicator captures the first year in which TPS for a country is subject to an attempted termination; cohorts from those countries are treated after the attempt, while cohorts from other TPS countries without an attempt serve as controls (either never receiving a termination or not receiving one yet). We estimate the specification in equation 1 with country-by-arrival cohort fixed effects and year fixed effects, so the coefficient on the termination indicator measures the change in outcomes for TPS recipients in countries facing a termination threat relative to recipients in countries where TPS continues without challenge.

Figure 4 shows the event study of the effect of attempted TPS terminations on employment, wages, and population. Across all three outcomes, pre-event coefficients are tightly estimated around 0. After the announced termination, employment and wages decline (Figures 4a and 4b, respectively). In Figure 4c, the effects on population are inconclusive, with both negative and positive effects over the next five years.

Turning to the regression tables, Table 3 presents the estimates. Column (1) shows that an attempted termination reduces employment by 12.6 percentage points, off a base of 74%. Column (3) shows that wages decrease by 30%. Column (5) shows an insignificant effect on the population size. Overall, the labor market responses to attempted terminations are substantially larger than the estimated effects of receiving TPS, with the estimated effect of removing TPS protections is three times larger than the effect of receiving initial designation.[19]

One potential explanation for the difference in magnitude between the effects of receiving TPS and the effects of attempting to remove it is that the sample of countries in each experiment are different. Appendix Table A10 re-estimates the effect of initial designations for TPS on the sample of 6 countries that later receive attempted terminations. This does not explain the difference—the labor market effect of initial designations for the subset of countries is very similar to the overall sample, and still about a third the size as the estimated effect of removing TPS.

Taken together, the results suggest that uncertainty about the continuation of TPS weak-

[19]We show estimates for each country separately in Appendix Table A9. The four Latin American/Carribean countries (El Salvador, Honduras, Haiti, and Nicaragua) display larger negative wage and employment effects than Nepal and Sudan.

ens the labor market position of beneficiaries without triggering clear changes in population. In turn, these adverse effects more than undo the employment and earnings gains documented after initial designations, even when legal protection remains in place.

4.3 Migration incentives

The second large policy concern around TPS is that by providing legal protections for deportation for people who are in the country without authorization, TPS may encourage additional unauthorized migration. The two methods to become an authorized migrant are to either enter the country illegally or to enter legally but then overstay a visa. The share of unauthorized immigrants who are visa overstayers has been increasing. For example, the Pew Research Center estimates that there were 680,000 unauthorized immigrants from India in the US in 2023, almost all of whom are visa overstayers (Pew Research Center, 2021, 2025).

In this section we examine the migration response to TPS. We first look at overall legal arrivals, measured by I-94 administrative data. We then consider visa overstays. Finally, we turn to apprehension data to approximate attempts at illegal entry.

4.3.1 Arrival and overstays

The US government tracks the number of legal non-immigrant visitors through I-94 data. These data are at the country-by-month level. Non-immigrant arrivals by themselves do not indicate unauthorized migration, but are a measure of the flow of people into the country, some of whom may later overstay their visas.

Figure 5 plots event studies of arrivals around initial designations, redesignations, and actual terminations. Arrivals drop immediately before the designation, followed by an increase in arrivals after the designation. For redesignations and terminations, the pre-event coefficients are less precisely estimated but, if anything, suggest declining arrivals before the event.

Regression results are reported in Columns (1)-(3) of Table 4. After the initial designation, I-94 arrivals increase by approximately 20%. We estimate no change in I-94 arrivals after redesignation or termination events.

We next look at visa overstays. Figure 6 shows event studies for the log number of in-

dividuals overstaying their visa. After initial designations, we observe an increase in the number of visitors who remain beyond their authorized stay. We can reject the null that all pre-event coefficients are equal to zero, however there is no consistent pre-trend pattern. We cannnot reject the null that all pre-event coefficients are equal to zero around redesignation events.

Columns (4)-(6) of Table 4 report the effects on overstays. We observe an increase of 26% in visa overstays after an initial designation. However, we cannot rule out that this may reflect the direct effect of natural disasters rather than TPS incentives overall. We observe a large negative decrease in overstays around the first redesignation of 74% and a 60% reduction in overstays around the termination events. Results are robust to the inclusion of countries affected by the travel ban in the first Trump administration, as shown in Appendix Table A11.

I-94 arrivals—and indirectly, visa overstays—may reflect an endogenous screening process carried out by US consular officers. We present evidence on consular behavior around TPS events. To approximate changes in consular behavior, we examine visa refusal rates, defined as the share of B-class tourist and business visa applications that are denied in a given year. Appendix Figure 4 shows that refusal rates rise with a delay following initial TPS designations: the event study points to higher refusal rates two or more years after designation, although the average post-designation effect is positive but imprecise. For terminations, we do not observe a consistent change in refusal rates in the period immediately following the policy change. For attempted terminations, refusal rates drop around the announcement, but the magnitudes are small and not stable across specifications. Overall, the evidence suggests only limited adjustments in consular screening practices in response to TPS. We also note that event study estimates for visa refusal rates are stable in the years preceding TPS designations, suggesting that consular screening does not systematically tighten or loosen in the years leading up to these events. This indicates that anticipatory shifts in consular screening do not alter the composition of the arrival cohorts that later become TPS eligible in our main labor market and population results.

4.3.2 Apprehensions data

For countries in Central and South America, the main channel to become an unauthorized migrant is to enter the country illegally. We look at impacts on Southern Border apprehensions as a proxy for attempted entries. Between October 2019 to May 2025 we observe apprehensions at the monthly level; between 2007-2024 at the annual level. Apprehensions occur when the Border Patrol temporarily controls or detains somebody crossing the border without authorization.[20] Apprehension counts do no include inadmissible immigrants, such as asylum seekers and participants in parole programs.

Figure 7 presents event studies for apprehensions, separately for TPS events in Venezuela and Haiti. For Venezuela, we examine the initial designation in 2021 and the redesignation in 2023. For Haiti, we focus on the second redesignation in 2023, since the initial designation in 2010 and first redesignation in 2011 fall outside the period with monthly data. We observe an increase in apprehensions for Venezuela starting three months before the initial designation, likely reflecting the worsening civil crisis. Apprehensions increase sharply after the TPS designation. Pre-trends around the redesignation for Venezuela are unclear due to substantial month to month volatility. For Haiti, pre-trends before redesignation exhibit a downward trend.

Table 5 reports the estimated difference-in-difference results. We find a positive increase in apprehensions for Venezuela around the initial designation. One particular threat to the validity of this results is that the designation coincides by a few months with the start of the Biden administration, which could have disproportionately encouraged Venezuelans to migrate and shift enforcement conditions. By contrast, we observe large declines in apprehensions around the two redesignation events.

Taken together, the different indicators of unauthorized migration show increases around initial TPS designations in both the visa overstay and apprehension data.[21] However, it is challenging to isolate the effect of TPS on out-migration since these events coincide with the underlying shocks that motivated the designation. We therefore examine migration re-

[20] This is separate from the expulsions under Title 42 authority that were permitted during the pandemic and involved briefer contact with the Border Patrol.

[21] Work is ongoing with alternative estimation methods to account for noisy estimates of pre-event coefficients.

sponses to redesignation and termination events. Around these events we find large negative responses in inflows, suggesting that extending or ending TPS does not generate additional unauthorized migration.

5 Conclusion

Temporary Protected Status provides temporary work authorization and protection from deportation. A country is designated as eligible for TPS if it has experienced armed conflict, an environmental disaster, or other extraordinary and temporary conditions and the US Secretary of Homeland Security determines it is unsafe for individuals to return home.

This paper first asks how TPS affects labor market outcomes. We study the labor market impacts of initially receiving TPS and then of the threat of TPS being removed. To answer this question, we use data on all TPS events between 2001 and 2024. We find that receiving TPS leads to an increase in employment, with minimal changes to wages. As expected, given deportation protection and a natural disaster at home, return migration decreases. Studying a set of attempted terminations of TPS during 2018, we find larger effects when TPS protections were threatened with termination than when they were introduced. Employment effects of threatening to remove TPS are three times the size of introducing it. This suggests that the economic consequences of removing legal authorization, even if not finalized, are substantial.

The second question we focus on is whether TPS leads to an increase in migration. One feature of TPS is that only individuals who are currently in the US are eligible for protection. Therefore, TPS does not necessarily provide relief for people who have experienced the disaster, but rather for those who migrated away from the country earlier. However, in 80% of cases, a country that receives TPS later receives a redesignation, which extends the arrival window for those eligible for protection. As a result, in the majority of the cases, an individual who migrates to the US without authorization immediately after the disaster will ex-post be given temporary protection at a later date. Consequently, there are concerns that the policy incentivizes unauthorized migration. We study unauthorized migration by considering the two main channels for becoming an unauthorized migrant: entering the country illegally and overstaying a visa. In general, both measures respond to the initial designation of TPS.

However, we argue that the migration responses to initial designations are confounded by the migration response to the natural disaster. A clearer test of the incentives for migration to legal protections is to examine migration responses around redesignation events where the eligible date of arrival to the US for TPS protection is extended. Here we find no evidence (indeed, primarily negative effects) of migration in response to TPS redesignations. These results suggest that policy concerns about an increase in unauthorized immigration in response to TPS may be overstated.

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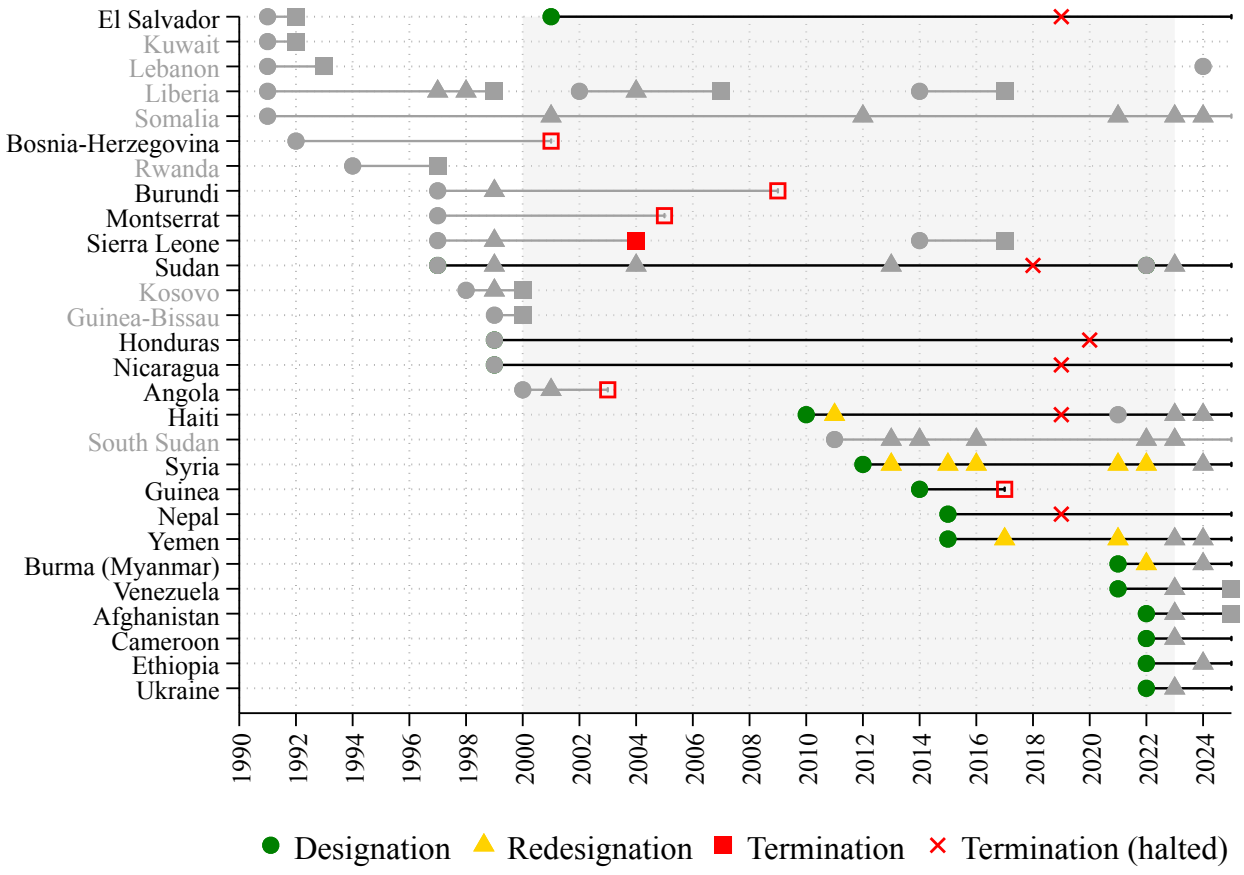
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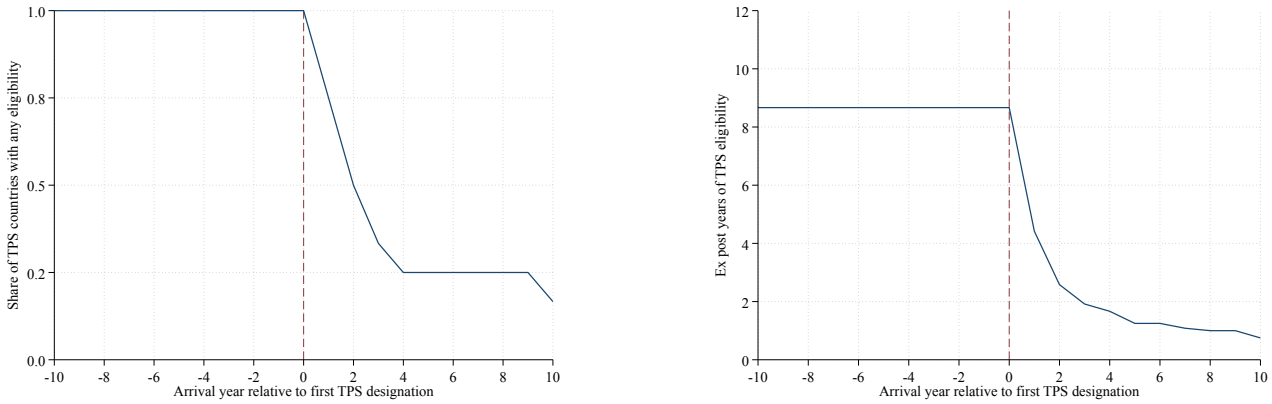
Figures and Tables

Figure 1: Timeline of TPS events



Notes: The figure plots all TPS designations, redesignations, terminations, and termination attempts from 1991 to 2024 for countries that have ever received TPS. Green circles mark initial designations, yellow triangles mark redesignations, red squares mark terminations, and red crosses mark termination attempts that were later halted or reversed. Gray line segments show the periods during which each country held TPS. The shaded vertical band indicates the years covered by our ACS sample and by the administrative migration data. Hollow markers identify TPS events that occur within this sample period for countries that are not observed in the ACS and are used only in the administrative analysis. Extension decisions are not shown because they do not change eligibility cutoffs or generate additional variation for our design. Liberia and South Sudan are excluded from the analysis for the reasons discussed in Section 2.

Figure 2: Expected TPS eligibility by arrival cohort

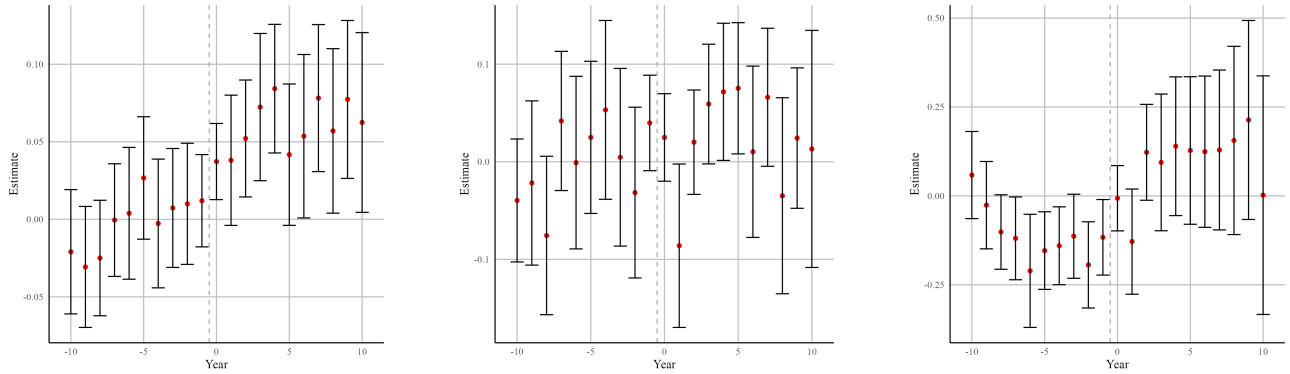


(a) Extensive Margin

(b) Intensive Margin

Notes: The figure summarizes ex-post TPS eligibility for cohorts of immigrants indexed by arrival year relative to the year of their country's first TPS designation. Panel (a) plots the share of TPS countries in which a cohort arriving in that relative year ever becomes eligible for TPS at least once. The sample includes the 12 countries that had a TPS designation in our sample, and relative arrival years from ten years before to ten years after the first designation. Panel (b) plots, for each relative arrival year, the average number of years during which that cohort is eligible for TPS, using the full subsequent history of designations, redesignations, and terminations in each country.

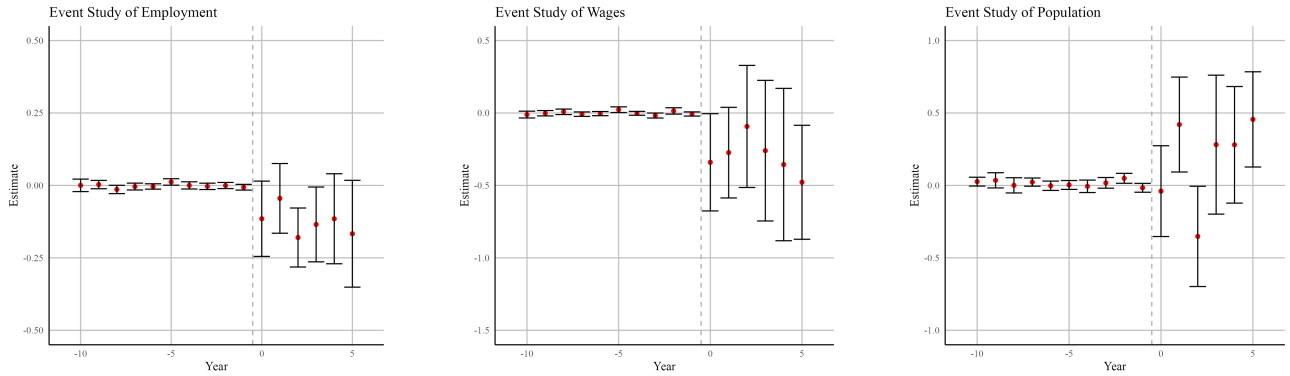
Figure 3: Labor market effects of initial designations: Event studies



(a) Employment (b) Log(Real Wages) (c) Log(Population)

Notes: Each panel reports point estimates and 95 percent confidence intervals for γ_k from equation 2. Event time k indexes calendar years relative to the initial TPS designation in the origin country, with $k = -1$ (the year before designation) omitted from the regression and serving as the reference period. The sample period is 2000–2023. The unit of observation is country of origin by arrival cohort by year. The sample is restricted to early arrival cohorts from countries that receive an initial TPS designation after 2000 and to never treated comparison countries in Asia, Africa, Latin America, and non EU Europe. Countries that already had TPS by 2000, Liberia and South Sudan, and members of the OECD Development Assistance Group are excluded. Late arrival cohorts ($a > A_c^{*D}$) from TPS countries are also excluded. Panel (a) presents the dynamic effects of TPS on employment. Panel (b) shows the effects on log real wages. Panel (c) shows the effects on log population (the stock of immigrants in the corresponding country cohort cell). Estimates are obtained using the Gardner imputation estimator. All regressions include country of origin-by-arrival cohort and year fixed effects and are weighted by the stock of immigrants from the origin country in 2000. Standard errors are clustered at the country of origin-by-arrival year level. Panels (a) and (b) have a p-value > 0.1 in a joint test for pre-trends being equal to 0, while panel (c) has a significant pre-trend with a p-value < 0.01. Event studies are normalized by the never treated units.

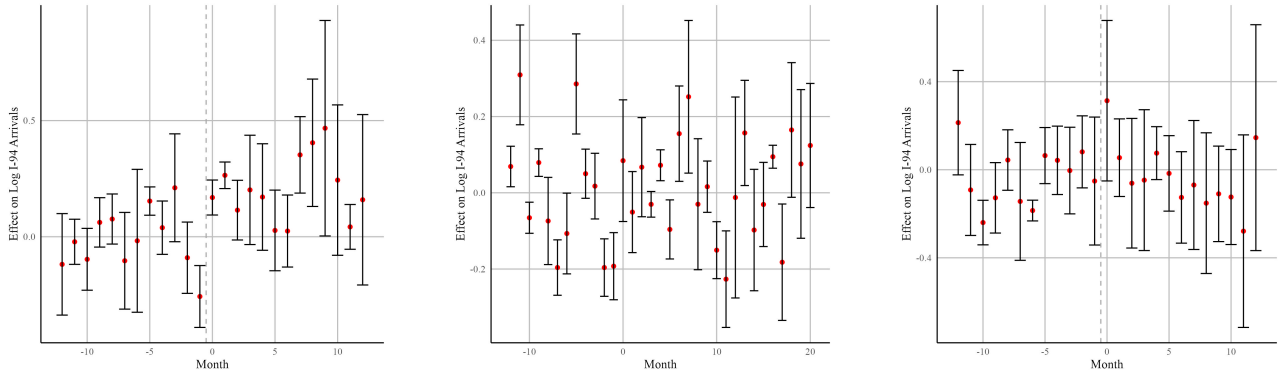
Figure 4: Labor market effects of attempted terminations: Event studies



(a) Employment (b) Log(Real Wages) (c) Log(Population)

Notes: Each panel reports point estimates and 95 percent confidence intervals for γ_k from equation 2. Event time k indexes calendar years relative to the initial TPS designation in the origin country, with $k = -1$ (the year before designation) omitted from the regression and serving as the reference period. The sample period is 2000–2023. The unit of observation is country of origin by arrival cohort by year. The sample is restricted to countries that either currently had TPS or had received a TPS designation that was in effect before 2000. Liberia and South Sudan are excluded. Panel (a) presents the dynamic effects of TPS on employment. Panel (b) shows the effects on log real wages. Panel (c) shows the effects on log population (the stock of immigrants in the corresponding country cohort cell). Estimates are obtained using the Gardner imputation estimator. All regressions include country of origin-by-arrival cohort and year fixed effects and are weighted by the stock of immigrants from the origin country in 2000. Standard errors are clustered at the country of origin-by-arrival year level. All p-values are > 0.1 in a joint test for pre-trends being equal to 0. Event studies are normalized by the never treated units.

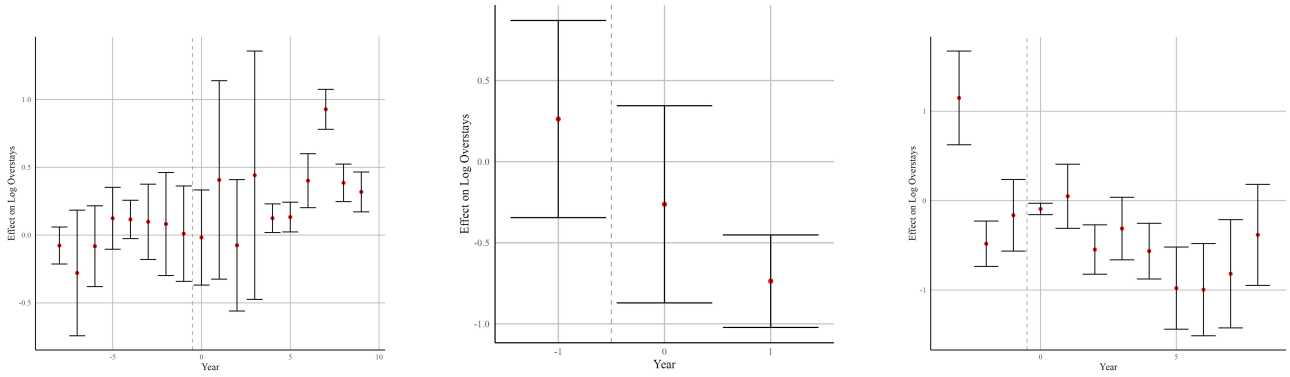
Figure 5: Migration incentives: I-94 arrivals



(a) Initial Designations (b) Redesignations (c) Terminations

Notes: Each panel reports point estimates and 95 percent confidence intervals for γ_k from a variation of equation 2. Standard errors are clustered by country. I-94 arrival data comes from the National Travel and Tourism Office. The sample period is January 2000 - December 2019; later data exists but has not been used because of COVID-induced disruptions. Data is weighted by January 2000 arrivals. Values for treated countries more than 12 months from the date of treatment in either direction have been excluded. Liberia and South Sudan have been excluded, as have countries in the OECD's Development Assistance Committee, including the EU. Countries affected by the first travel ban in the first Trump administration have been excluded. For terminations, countries that never had TPS or did not yet have TPS have been excluded. For redesignations, countries that have not received TPS have been excluded, as have ones that have already experienced their second redesignation or experienced their first redesignation before the time period. For initial designations, countries that have been actually terminated have been excluded. All models have significant pre-trends with p-values <0.01 in a joint test. Event studies are normalized by the never treated units.

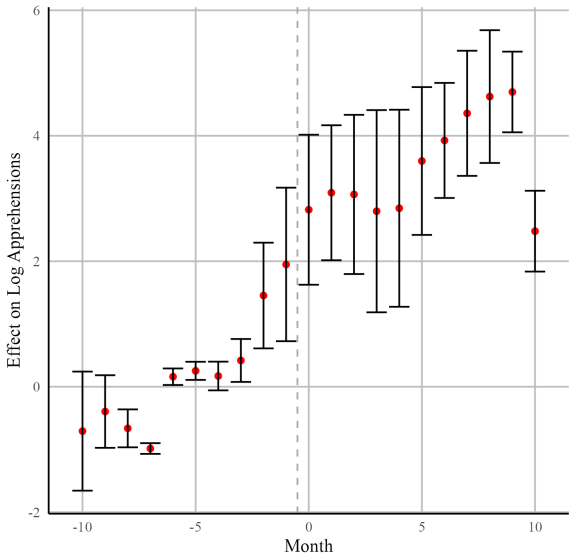
Figure 6: Migration incentives: visa overstays



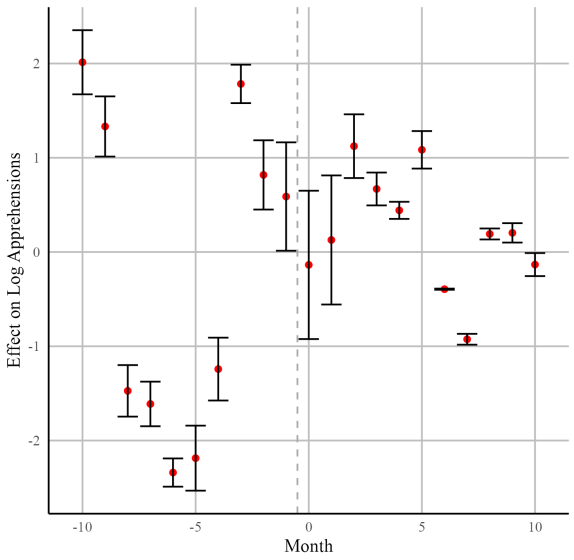
(a) Initial Designations (b) Redesignations (c) Terminations

Notes: Each panel reports point estimates and 95 percent confidence intervals for γ_k from a variation of equation 2. Visa overstay data is from the estimates in the Department of Homeland Security's Entry/Exit Overstay Reports. The sample period for overstays is 2014 to 2024. Standard errors are clustered by country. The overstay rate is the proportion of people whom the government suspects have overstayed within the country. The data is weighted by the total number of overstays in 2014. Liberia and South Sudan have been excluded, as have countries in the OECD's Development Assistance Committee, including the EU. Countries affected by the first travel ban in the first Trump administration have been excluded. For terminations, countries that never had TPS or did not yet have TPS have been excluded. For redesignations, countries that have not received TPS have been excluded, as have ones that have already experienced their second redesignation or experienced their first redesignation before the time period. For initial designations, countries that have been actually terminated have been excluded. All models have significant pre-trends with p-values <0.01 in a joint test, except for subfigure (b), redesignations, which has a p-value > 0.1. Event studies are normalized by the never treated units.

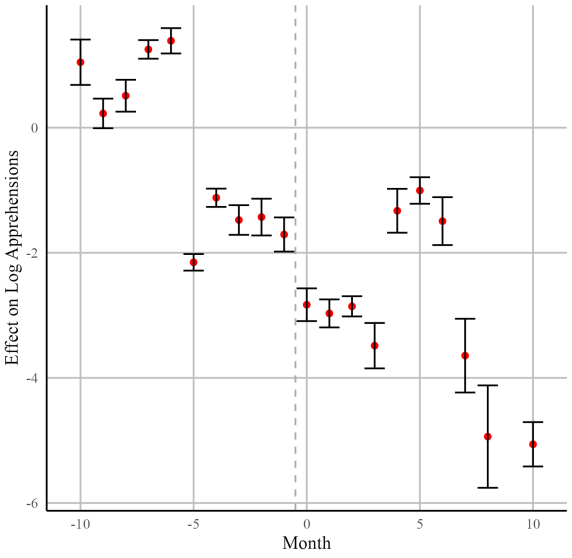
Figure 7: Apprehensions



(a) Initial Designation (Venezuela)



(b) Redesignation (Venezuela)



(c) Redesignation (Haiti)

Notes: This graph shows an event study of the monthly apprehension count on the Southern border for Venezuela before and after TPS, compared to other countries in Latin America that never received TPS, based on data from U.S. Customs and Border Patrol. The time frame is April 2020 to September 2022 for initial designations, which is extended to August 2025 for both redesignations. For redesignations, the controls are instead all TPS units that have not received a redesignation (i.e. the Venezuelan model omits Haiti, but uses El Salvador, Haiti, and Nicaragua; the Haitian model includes those as well as Venezuela until it gets its re-designation). Population is a raw count. This uses Gardner et al.'s imputation method. All observations are weighted as of October 2019, the first month in the data. All models have significant pre-trends with $p < 0.01$ in a joint test of significance of pre-trends. Event studies are normalized by the never treated units.

Table 1: Summary Statistics for Likely Undocumented Sample, Year of TPS Designation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Country	Year	Est. Population	Average Years in U.S.	<5 Years in U.S.	Employment	Hourly Wage	Years of Education	Fluent in English
Afghanistan	2022	13402	3.74	0.726	0.547	18.65	10.11	0.315
Burma (Myanmar)	2021	22445	7.07	0.251	0.807	11.88	8.19	0.289
Cameroon	2022	9647	5.89	0.476	0.768	17.75	13.63	0.689
El Salvador	2001	241251	5.7	0.411	0.715	9.55	8.63	0.14
Ethiopia	2022	33098	8.09	0.397	0.753	15.53	12.62	0.457
Guinea	2014	3420	13.32	0.108	0.705	5.99	12.13	0.547
Haiti	2010	102550	8.43	0.286	0.595	11.14	11.04	0.329
Nepal	2015	32013	5.76	0.555	0.698	10.53	12.78	0.431
Syria	2012	6361	6.61	0.48	0.424	8.98	12.75	0.372
Ukraine	2022	41642	7.78	0.395	0.69	19.04	14.22	0.337
Venezuela	2021	155357	5.92	0.512	0.775	14.46	14.42	0.342
Yemen	2015	2912	7.39	0.399	0.438	8.54	9.26	0.364
Average			6.49	0.422	0.706	12.24	11.26	0.281

Notes: Fluent in English corresponds to being listed as a monolingual English speaker or speaking English very well. Average is weighted by population. All data is from the American Community Survey. The population corresponds to the population in the main sample, i.e. filtered for likely undocumented immigrants between 18 and 64, not in group quarters.

Table 2: Effects of Initial TPS Designations, Likely Undocumented

	Employment		Log(Real Wages)		Log(Population)	
	(1)	(2)	(3)	(4)	(5)	(6)
TPS	0.045**	0.163***	0.019	0.160***	0.218*	0.301***
	(0.014)	(0.029)	(0.029)	(0.043)	(0.123)	(0.076)
TPS x Years of TPS		0.010*		0.035***		0.132**
		(0.005)		(0.009)		(0.041)
TPS x Years in US		-0.010***		-0.019***		-0.050***
		(0.002)		(0.004)		(0.008)
TPS x Years of TPS x Years in US		-0.000		-0.001**		-0.002*
		(0.000)		(0.000)		(0.001)
Mean Dep. Variable	0.698	0.698	2.548	2.548	1.974	1.974
Number of Events	12	12	12	12	12	12
R Squared (First-Stage)	0.264	0.264	0.607	0.607	0.961	0.961
Observations	32,982	32,982	27,272	27,272	32,982	32,982
Year FE	X	X	X	X	X	X
Year of Arrival x Country FE	X	X	X	X	X	X

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$

The sample period is 2000-2023, with cohorts arriving from 1990 to 2023. The unit of observation is arrival cohort by country-of-origin by calendar year. Log(Real Wages) is the mean of the log of real wages, not the log of the mean. The sample is constructed based on averages from individual level data for immigrants aged 18-64, not in group quarters. Weights are by the population from a given country in the year 2000. Standard errors are clustered at the arrival year by country-of-origin level. The method of estimation is the Borusyak, Jaravel, and Speiss imputation estimator. Population is constructed as the sum of the individual weights, but is also weighted. Liberia and South Sudan have been excluded, as have countries in the OECD's Development Assistance Committee, including the EU. Data from countries that received a TPS designation before 2000 have been excluded. Similarly, data from a country after any successful termination event for that country has been excluded. Data from cohorts that arrived after the first TPS designation for their country have been excluded.

Table 3: Effects of Attempted TPS Terminations, Likely Undocumented

	Employment		Log(Real Wages)		Log(Population)	
	(1)	(2)	(3)	(4)	(5)	(6)
Termination	-0.126**	0.026	-0.299**	0.047	0.175	-0.180
	(0.039)	(0.077)	(0.119)	(0.197)	(0.127)	(0.213)
Termination x Years of TPS		-0.003		0.005		0.042***
		(0.003)		(0.003)		(0.012)
Termination x Years in US		-0.003		-0.015***		-0.023**
		(0.003)		(0.004)		(0.011)
Termination x Years of Termination		-0.006		-0.021		0.056
		(0.022)		(0.055)		(0.036)
Mean Dep. Variable	0.737	0.737	2.342	2.342	2.595	2.595
Number of Events	6	6	6	6	6	6
R Squared (First-Stage)	0.301	0.301	0.312	0.312	0.931	0.931
Observations	1,760	1,760	1,505	1,505	1,760	1,760
Year FE	X	X	X	X	X	X
Year of Arrival x Country FE	X	X	X	X	X	X

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$

The sample period is 2000-2023, with cohorts arriving from 1990 to 2023. The unit of observation is arrival cohort by country-of-origin by calendar year. Log(Real Wages) is the mean of the log of real wages, not the log of the mean. The sample is constructed based on averages from individual level data for immigrants aged 18-64, not in group quarters. Weights are by the population from a given country in the year 2000. Standard errors are clustered at the arrival year by country-of-origin level. The method of estimation is the Borusyak, Jaravel, and Speiss imputation estimator. Population is constructed as the sum of the individual weights, but is also weighted. Liberia and South Sudan have been excluded, as have countries in the OECD's Development Assistance Committee, including the EU. Data from countries that got terminated in actuality have been excluded.. All data is for people who received TPS as the result of the initial designation for their country. Treatment corresponds to TPS having an attempted termination.

Table 4: Effects of TPS Events on Visa Overstays and Arrivals

	Log(I-94 Arrivals)			Log(Overstays)		
	(1)	(2)	(3)	(4)	(5)	(6)
Initial Designation	0.206**			0.258*		
	(0.103)			(0.155)		
First Redesignation		-0.032			-0.737***	
		(0.021)			(0.141)	
Termination			-0.059			-0.569***
			(0.040)			(0.125)
Mean Dep. Variable	6.044	8.406	8.575	5.586	7.628	7.716
R Squared (First-Stage)	0.967	0.894	0.884	0.948	0.930	0.904
Observations	33,140	996	1,039	1,358	47	77
Year FE				X	X	X
Month FE	X	X	X			
Country FE	X	X	X	X	X	X

* p < 0.1, ** p < 0.05, *** p < 0.001

Visa overstay data is from the estimates in the Department of Homeland Security's Entry/Exit Overstay Reports. Visa refusal data comes from the State Department's Visa Office and combines B class tourist and business visas. I-94 arrival data comes from the National Travel and Tourism Office, monthly from 2000 to 2019 (later data exists but has not been used because of the disruption caused by COVID-19). The sample period for overstays is 2014 to 2024 and the sample period for refusals is 2006 to 2024. Standard errors are clustered by country. For I-94 arrivals, values for a country that is treated are only included within 12 months of the treatment date. Countries affected by the Trump administration's first travel ban have been excluded. Overstays are weighted by 2014 overstays, refusals are weighted by 2006 refusals, and arrivals are weighted by January 2000 arrivals. Liberia and South Sudan have been excluded, as have countries in the OECD's Development Assistance Committee, including the EU. For attempted terminations and terminations, countries that never had TPS or did not yet have TPS have been excluded. For attempted terminations, countries that were actually terminated were excluded after the termination. For initial designations, countries that have been actually terminated have been excluded.

Table 5: Effects of TPS on Log Southern Border Apprehensions

	Initial Designation (Venezuela)	Redesignation (Venezuela)	Redesignation (Haiti)
	(1)	(2)	(3)
TPS	3.438***	-0.473***	-2.853***
	(0.480)	(0.077)	(0.093)
Mean Dep. Variable	6.846	7.564	7.390
R Squared (First-Stage)	0.737	0.951	0.947
Observations	240	248	285

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$

This shows results for the monthly apprehension count on the Southern border for Venezuela before and after TPS, compared to other countries in Latin America that never received TPS, based on data from U.S. Customs and Border Patrol. The time frame is April 2020 to September 2022 for initial designations, which is extended to August 2025 for both redesignations. For redesignations, the controls are instead all TPS units that have not received a redesignation (i.e. the Venezuelan model omits Haiti, but uses El Salvador, Haiti, and Nicaragua; the Haitian model includes those as well as Venezuela until it gets its redesignation). Population is a raw count. This uses Gardner et al.'s imputation method. All observations are weighted as of October 2019, the first month in the data.

APPENDIX: FOR ONLINE PUBLICATION ONLY

Appendix

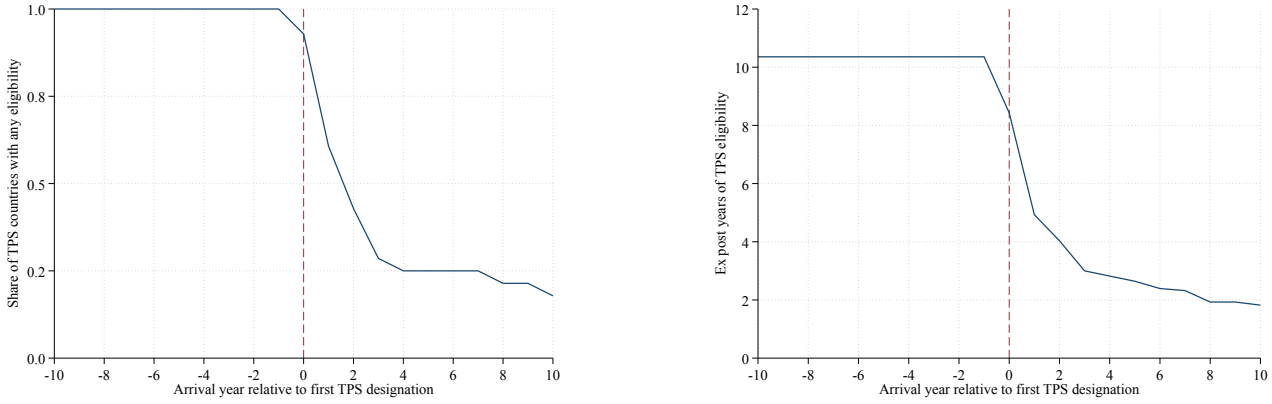
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A Additional Figures and Tables

Appendix Figure 1: Expected TPS eligibility by arrival cohort, all designated countries

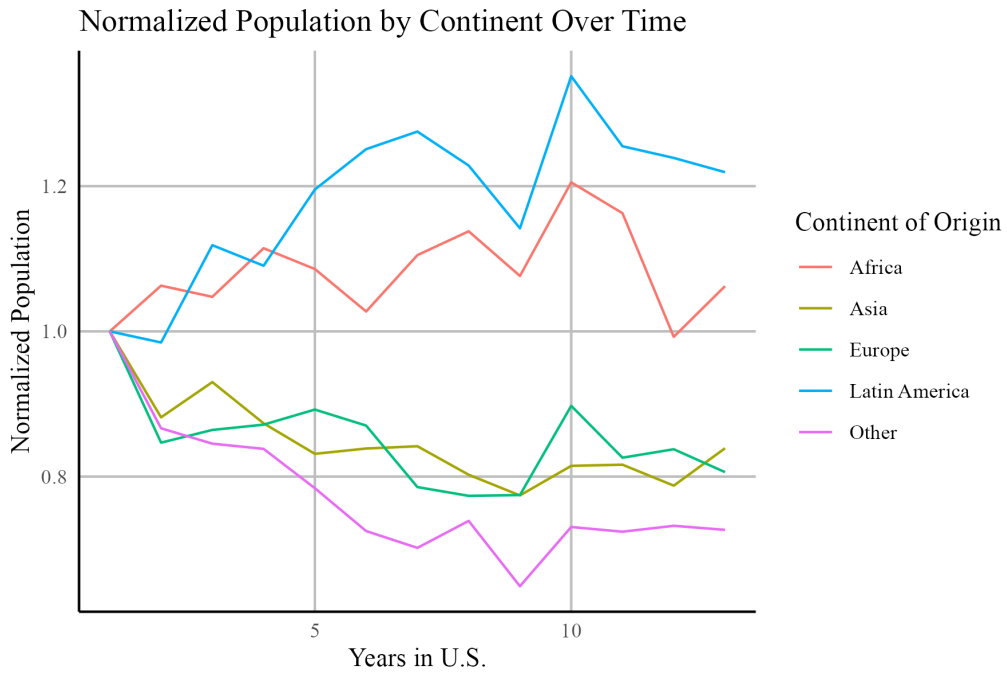


(a) Extensive Margin

(b) Intensive Margin

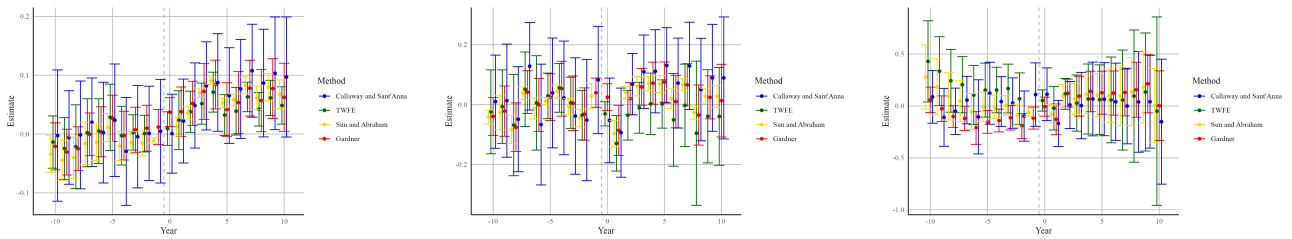
Notes: The figure summarizes ex-post TPS eligibility for cohorts of immigrants indexed by arrival year relative to the year of their country's first TPS designation. Panel (a) plots the share of TPS countries in which a cohort arriving in that relative year ever becomes eligible for TPS at least once. The sample includes 28 ever TPS-designated countries, and relative arrival years from ten years before to ten years after the first designation. Panel (b) plots, for each relative arrival year, the average number of years during which that cohort is eligible for TPS, using the full subsequent history of designations, redesignations, and terminations in each country.

Appendix Figure 2: Average normalized cohort size, relative to year of immigration



Notes: Figures show the average cohort size from a given country or continent, divided by the population observed for the first full year in the country, for cohorts arriving between 2000 and 2010 as observed in the American Community Survey. The entire population is included, including both citizen and non-citizen immigrants and people of all ages.

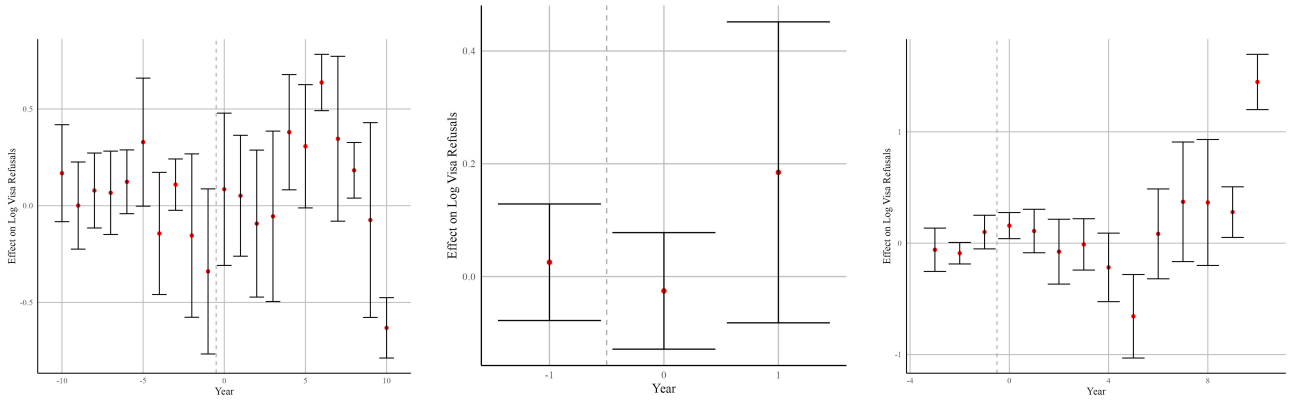
Appendix Figure 3: Event Studies for Initial Designations: Robustness to DiD estimates



(a) Employment (b) Log(Real Wages) (c) Log(Population)

Notes: Each panel reports point estimates and 95 percent confidence intervals for γ_k from equation 2, estimated with four alternative difference in differences methods. Event time k indexes calendar years relative to the initial TPS designation in the origin country, with $k = -1$ omitted and serving as the reference period. The sample period is 2000–2023. The unit of observation is country of origin by arrival cohort by year. The sample is restricted to early arrival cohorts from countries that receive an initial TPS designation after 2000 and to never treated comparison countries in Asia, Africa, Latin America, and non EU Europe. Countries that already had TPS by 2000, Liberia and South Sudan, and members of the OECD Development Assistance Committee are excluded. We also exclude late arrival cohorts ($a > A_c^{*D}$) from TPS countries. Panel (a) shows estimates for employment, panel (b) for log real wages, and panel (c) for log population. In each panel, we report event study coefficients obtained using the Callaway and Sant'Anna (2021) estimator, a two way fixed effects estimator, the **?** estimator, and the Gardner et al. (2024) imputation estimator. All specifications include country of origin–by–arrival cohort and year fixed effects and are weighted by the stock of immigrants from the origin country in 2000. Standard errors are clustered at the country of origin–by–arrival year level.

Appendix Figure 4: Event Studies for Log Refusals



(a) Initial Designations (b) Redesignations (c) Terminations

Notes: Each panel reports point estimates and 95 percent confidence intervals for γ_k from a variation of equation 2. Standard errors are clustered by country. Visa refusal data comes from the State Department's Visa Office and combines B class tourist and business visas. The refusal rate is the proportion of visa applications that were refused. The sample period is 2006 to 2024. Data is weighted by 2006 refusals. Liberia and South Sudan have been excluded, as have countries in the OECD's Development Assistance Committee, including the EU. For terminations, countries that never had TPS or did not yet have TPS have been excluded. Countries affected by the first travel ban in the first Trump administration have been excluded. For initial designations, countries that have been actually terminated have been excluded. (a) fails the pre-trend test with a p-value of <0.01 in the joint test for pre-trend differences from 0, while (b) and (c) have p-values > 0.1. Event studies are normalized by the never treated units.

Appendix Table A1: Summary Statistics for All Immigrants, Year of TPS Designation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Country	Year	Est. Population	Average Years in U.S.	<5 Years in U.S.	Employment	Hourly Wage	Years of Education	Fluent in English	Likely Undocumented	Citizen
Afghanistan	2022	173178	7.17	0.502	0.382	15.58	8.65	0.454	0.1122	0.3115
Burma (Myanmar)	2021	147779	11.41	0.12	0.626	15.17	9.04	0.295	0.1694	0.552
Cameroon	2022	76470	10.8	0.238	0.726	21.75	13.57	0.792	0.1579	0.5768
El Salvador	2001	357246	5.81	0.389	0.605	9.82	8.28	0.227	0.8025	0.0795
Ethiopia	2022	278939	13.57	0.146	0.67	17.96	12.54	0.591	0.1356	0.6406
Guinea	2014	11500	11.05	0.147	0.648	11.14	11.56	0.51	0.3158	0.2125
Haiti	2010	364269	9.58	0.24	0.51	12.12	10.72	0.451	0.3539	0.3365
Nepal	2015	119407	6.6	0.447	0.553	14.27	11.73	0.548	0.3172	0.229
Syria	2012	42173	10.53	0.252	0.417	19.27	12.2	0.49	0.1894	0.5432
Ukraine	2022	367163	16.11	0.188	0.601	23.54	13.89	0.504	0.1475	0.615
Venezuela	2021	486760	9.51	0.36	0.634	17.21	13.45	0.482	0.4103	0.3211
Yemen	2015	31874	10.9	0.248	0.375	11.53	8.69	0.379	0.0991	0.5731
Average			10.32	0.288	0.584	16.11	11.48	0.455	0.3324	0.3917

Fluent in English corresponds to being listed as a monolingual English speaker or speaking English very well. Average is weighted by population. The population includes all immigrants, including children and seniors as well as those living in group quarters and those who are citizens or likely documented, all of which are not included in the likely undocumented sample. Hourly wage is CPI-adjusted.

Appendix Table A2: Summary Statistics, Likely Undocumented, 2010

	(1)	(2)	(3)	(4)	(5)
	Treated	Never Treated	Not Yet Treated	Post-TPS Arrivals	Terminated
Est. Population Size	355,047	7,232,206	298,960	434,187	40,625
Percent Employed	0.788	0.673	0.645	0.716	0.592
Hourly Wage	11.361	12.299	12.86	9.066	14.858
Age	36.47	34.499	35.729	31.153	37.65
Years of Education	9.082	10.587	12.347	9.246	12.342
Percent Male	0.571	0.551	0.508	0.584	0.52
Percent Head of Household	0.422	0.358	0.352	0.236	0.39
Percent Fluent in English	0.221	0.265	0.367	0.145	0.448
Percent in Labor Force	0.879	0.752	0.765	0.812	0.703
Percent with Any College	0.134	0.253	0.453	0.13	0.348
Years in US	14.007	9.359	7.457	5.388	8.653
Household Size	4.618	4.462	3.934	4.558	3.607
Household Size (Undocumented Members Only)	2.405	2.366	2.107	2.771	2.019

All data from the year 2010, divided by the cohort's TPS status. Never treated and not yet treated cells form the control group for the main specification, while treated cells form the treated group. Terminated and post-TPS arrival cells are dropped from the specification. Fluent in English corresponds to being listed as a monolingual English speaker or speaking English very well.

Appendix Table A3: Population Comparison, Year of TPS Designation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Country	Gov't Eligibility Estimate	Total Population	Likely Undocumented Sample	Likely Undocumented Minus Likely Refugees	Gov't TPS Count 2 Years After	Maximum Gov't TPS Count	Gov't Takeup Rate	ACS Takeup Rate
Afghanistan	72,500	173,178	13,402	13,194	8,245	9,630	0.114	0.6152
Burma (Myanmar)	1,600	147,779	22,445	933	1,760	3,275	1.1	0.0784
Cameroon	11,700	76,470	9,647	9,647	3,265	3,485	0.279	0.3384
El Salvador	NA	357,246	241,251	241,251	NA	NA	NA	NA
Ethiopia	26,730	278,939	33,098	30,710	3,745	3,745	0.14	0.1131
Guinea	2,000	11,500	3,420	2,480	930	2,000	0.465	0.2719
Haiti	150,000	364,269	102,550	79,834	48,000	260,790	0.32	0.4681
Nepal	17,500	119,407	32,013	32,013	14,791	17,500	0.845	0.462
Syria	2,750	42,173	6,361	6,361	2,071	7,014	0.753	0.3256
Ukraine	59,600	367,163	41,642	41,331	50,205	63,425	0.842	1.2056
Venezuela	323,000	486,760	155,357	155,357	182,579	505,400	0.565	1.1752
Yemen	1,225	31,874	2,912	2,912	1,116	2,270	0.911	0.3832

The total population includes all immigrants. The likely undocumented sample removes children, seniors, those with documentation including citizenship, and those in group quarters. The likely undocumented sample minus likely refugees sample additionally removes immigrants who arrived in years where the DHS reports that at least half of immigrants from that country were refugees. The government eligibility estimates are from the congressional record on TPS declarations.

Appendix Table A4: Effects of Initial TPS Designations by Sex, Likely Undocumented

	Employment	Log(Real Wages)	Log(Population)
TPS	-0.027	-0.081**	0.167
	(0.021)	(0.031)	(0.125)
Male	0.153***	0.101***	0.089***
	(0.006)	(0.002)	(0.007)
TPS x Male	0.023*	0.128***	0.122***
	(0.012)	(0.017)	(0.037)
Mean Dep. Variable	0.689	2.530	1.624
Number of Events	12	12	12
R Squared (First-Stage)	0.114	0.520	0.949
Observations	56,621	44,546	56,621
Year FE	X	X	X
Year of Arrival x Country FE	X	X	X

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$

The sample period is 2000-2023, with cohorts arriving from 1990 to 2023. The unit of observation is arrival cohort by country-of-origin by calendar year. Log(Real Wages) is the mean of the log of real wages, not the log of the mean. The sample is constructed based on averages from individual level data for immigrants aged 18-64, not in group quarters. Weights are by the population from a given country in the year 2000. Standard errors are clustered at the arrival year by country-of-origin level. The method of estimation is the Borusyak, Jaravel, and Speiss imputation estimator. Population is constructed as the sum of the individual weights, but is also weighted. Liberia and South Sudan have been excluded, as have countries in the OECD's Development Assistance Committee, including the EU. Data from countries that received a TPS designation before 2000 have been excluded. Similarly, data from a country after any successful termination event for that country has been excluded. Data from cohorts that arrived after the first TPS designation for their country have been excluded.

Appendix Table A5: Effects of Initial TPS Designations by Education, Likely Undocumented

	Employment	Log(Real Wages)	Log(Population)
TPS	0.083***	0.101**	0.266**
	(0.023)	(0.039)	(0.127)
Less than HS Diploma/GED	-0.031***	-0.117***	-0.251***
	(0.002)	(0.010)	(0.049)
TPS x Less than HS Diploma	-0.006	0.032	0.272**
	(0.010)	(0.020)	(0.135)
Mean Dep. Variable	0.679	2.479	1.703
Number of Events	12	12	12
R Squared (First-Stage)	0.188	0.367	0.884
Observations	48,972	37,643	48,972
Year FE	X	X	X
Year of Arrival x Country FE	X	X	X

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$

The sample period is 2000-2023, with cohorts arriving from 1990 to 2023. The unit of observation is arrival cohort by country-of-origin by calendar year. Log(Real Wages) is the mean of the log of real wages, not the log of the mean. The sample is constructed based on averages from individual level data for immigrants aged 18-64, not in group quarters. Weights are by the population from a given country in the year 2000. Standard errors are clustered at the arrival year by country-of-origin level. The method of estimation is the Borusyak, Jaravel, and Speiss imputation estimator. Population is constructed as the sum of the individual weights, but is also weighted. Liberia and South Sudan have been excluded, as have countries in the OECD's Development Assistance Committee, including the EU. Data from countries that received a TPS designation before 2000 have been excluded. Similarly, data from a country after any successful termination event for that country has been excluded. Data from cohorts that arrived after the first TPS designation for their country have been excluded.

Appendix Table A6: Effects of Initial TPS Designations, By Immigration Status

	Likely Undocumented			Immigrant Non-Citizens			All Immigrants			Immigrant Citizens		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
TPS	0.045**	0.019	0.218*	0.049***	0.039	0.236**	0.037**	0.036*	0.152**	-0.007	-0.050	0.099
	(0.014)	(0.029)	(0.123)	(0.012)	(0.025)	(0.113)	(0.012)	(0.019)	(0.077)	(0.024)	(0.104)	(0.163)
Mean Dep. Variable	0.698	2.548	1.974	0.677	2.578	2.359	0.707	2.655	2.882	0.755	2.744	2.189
Number of Events	12	12	12	12	12	12	12	12	12	12	12	12
R Squared (First-Stage)	0.264	0.607	0.961	0.262	0.686	0.964	0.425	0.821	0.980	0.204	0.537	0.870
Observations	32,982	27,272	32,982	39,708	34,837	39,708	43,467	41,059	43,467	36,462	32,403	36,462
Year FE	X	X	X	X	X	X	X	X	X	X	X	X
Year of Arrival x Country FE	X	X	X	X	X	X	X	X	X	X	X	X

* p < 0.1, ** p < 0.05, *** p < 0.001

The sample period is 2000-2023, with cohorts arriving from 1990 to 2023. The unit of observation is arrival cohort by country-of-origin by calendar year. Log(Real Wages) is the mean of the log of real wages, not the log of the mean. The sample is constructed based on averages from individual level data for immigrants aged 18-64, not in group quarters. Weights are by the population from a given country in the year 2000. Standard errors are clustered at the arrival year by country-of-origin level. The method of estimation is the Borusyak, Jaravel, and Speiss imputation estimator. Population is constructed as the sum of the individual weights, but is also weighted. Liberia and South Sudan have been excluded, as have countries in the OECD's Development Assistance Committee, including the EU. Data from countries that received a TPS designation before 2000 have been excluded. Similarly, data from a country after any successful termination event for that country has been excluded. Data from cohorts that arrived after the first TPS designation for their country have been excluded.

Appendix Table A7: Effects of Initial TPS Designations, Likely Undocumented, by Weighting

	Employment			Log(Real Wages)			Log(Population)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
TPS	0.045**	0.012	0.053***	0.019	0.039	0.079**	0.218*	0.199**	0.305*
	(0.014)	(0.015)	(0.011)	(0.029)	(0.029)	(0.040)	(0.123)	(0.063)	(0.157)
Mean Dep. Variable	0.698	0.698	0.698	2.548	2.548	2.548	1.974	1.974	1.974
Number of Events	12	12	12	12	12	12	12	12	12
R Squared (First-Stage)	0.264	0.208	0.412	0.607	0.320	0.714	0.961	0.812	0.974
Observations	32,982	35,724	35,724	27,272	29,052	29,052	32,982	35,724	35,724
Year FE	X	X	X	X	X	X	X	X	X
Year of Arrival x Country FE	X	X	X	X	X	X	X	X	X
Weights by Population	2000	None	Current Year	2000	None	Current Year	2000	None	Current Year

* p < 0.1, ** p < 0.05, *** p < 0.001

The sample period is 2000-2023, with cohorts arriving from 1990 to 2023. The unit of observation is arrival cohort by country-of-origin by calendar year. Log(Real Wages) is the mean of the log of real wages, not the log of the mean. The sample is constructed based on averages from individual level data for immigrants aged 18-64, not in group quarters. Weights are by the population from a given country in the year 2000, the population in that year, or no weights at all. Standard errors are clustered at the arrival year by country-of-origin level. The method of estimation is the Borusyak, Jaravel, and Speiss imputation estimator. Population is constructed as the sum of the individual weights, but is also weighted. Liberia and South Sudan have been excluded, as have countries in the OECD's Development Assistance Committee, including the EU. Data from countries that received a TPS designation before 2000 have been excluded. Similarly, data from a country after any successful termination event for that country has been excluded. Data from cohorts that arrived after the first TPS designation for their country have been excluded.

Appendix Table A8: Effects of Initial TPS Designations, Likely Undocumented without Likely Refugees

	Employment		Log(Real Wages)		Log(Population)	
	(1)	(2)	(3)	(4)	(5)	(6)
TPS	0.058***	0.156***	0.031	0.152***	0.237**	0.249***
	(0.015)	(0.031)	(0.030)	(0.046)	(0.121)	(0.073)
TPS x Years of TPS		0.008		0.037***		0.119**
		(0.005)		(0.009)		(0.042)
TPS x Years in US		-0.008**		-0.020***		-0.044***
		(0.002)		(0.004)		(0.009)
TPS x Years of TPS x Years in US		-0.000		-0.001**		-0.002*
		(0.000)		(0.000)		(0.001)
Mean Dep. Variable	0.696	0.696	2.553	2.553	2.001	2.001
Number of Events	12	12	12	12	12	12
R Squared (First-Stage)	0.276	0.276	0.621	0.621	0.963	0.963
Observations	29,982	29,982	24,777	24,777	29,982	29,982
Year FE	X	X	X	X	X	X
Year of Arrival x Country FE	X	X	X	X	X	X

* p < 0.1, ** p < 0.05, *** p < 0.001

The sample period is 2000-2023, with cohorts arriving from 1990 to 2023. The unit of observation is arrival cohort by country-of-origin by calendar year. Log(Real Wages) is the mean of the log of real wages, not the log of the mean. The sample is constructed based on averages from individual level data for immigrants aged 18-64, not in group quarters. Weights are by the population from a given country in the year 2000. Standard errors are clustered at the arrival year by country-of-origin level. The method of estimation is the Borusyak, Jaravel, and Speiss imputation estimator. Population is constructed as the sum of the individual weights, but is also weighted. Liberia and South Sudan have been excluded, as have countries in the OECD's Development Assistance Committee, including the EU. Data from countries that received a TPS designation before 2000 have been excluded. Similarly, data from a country after any successful termination event for that country has been excluded. Data from cohorts that arrived after the first TPS designation for their country have been excluded.

Appendix Table A9: Effects of Attempted TPS Terminations by Country, Likely Undocumented

	Employment	Log(Real Wages)	Log(Population)
Termination (El Salvador)	-0.147***	-0.306***	0.372***
	(0.015)	(0.044)	(0.049)
Termination (Haiti)	-0.091*	-0.288*	-0.035
	(0.047)	(0.148)	(0.154)
Termination (Honduras)	-0.162***	-0.376***	0.268***
	(0.020)	(0.041)	(0.063)
Termination (Nepal)	-0.067	-0.074	0.123
	(0.945)	(3.063)	(3.131)
Termination (Nicaragua)	-0.111***	-0.204***	-0.176***
	(0.009)	(0.014)	(0.034)
Termination (Sudan)	-0.098	0.146	0.196
	(0.102)	(0.388)	(0.318)
Mean Dep. Variable	0.737	2.342	2.595
Number of Events	6	6	6
R Squared (First-Stage)	0.301	0.312	0.931
Observations	1,760	1,505	1,760
Year FE	X	X	X
Year of Arrival x Country FE	X	X	X

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$

The sample period is 2000-2023, with cohorts arriving from 1990 to 2023. The unit of observation is arrival cohort by country-of-origin by calendar year. Log(Real Wages) is the mean of the log of real wages, not the log of the mean. The sample is constructed based on averages from individual level data for immigrants aged 18-64, not in group quarters. Weights are by the population from a given country in the year 2000. Standard errors are clustered at the arrival year by country-of-origin level. The method of estimation is the Borusyak, Jaravel, and Speiss imputation estimator. Population is constructed as the sum of the individual weights, but is also weighted. Liberia and South Sudan have been excluded, as have countries in the OECD's Development Assistance Committee, including the EU. Data from countries that got terminated in actuality have been excluded. All data is for people who received TPS as the result of the initial designation for their country. Treatment corresponds to TPS having an attempted termination.

Appendix Table A10: Effects of Initial TPS Designations, Likely Undocumented, In Countries With Later Termination Attempts

	Employment		Log(Real Wages)		Log(Population)	
	(1)	(2)	(3)	(4)	(5)	(6)
TPS	0.046**	0.187***	0.031	0.101**	0.249*	0.540***
	(0.015)	(0.039)	(0.030)	(0.048)	(0.131)	(0.104)
TPS x Years of TPS		0.010**		0.036***		0.127**
		(0.005)		(0.009)		(0.041)
TPS x Years in US		-0.013***		-0.014**		-0.075***
		(0.003)		(0.005)		(0.013)
TPS x Years of TPS x Years in US		-0.000		-0.001***		-0.001
		(0.000)		(0.000)		(0.001)
Mean Dep. Variable	0.699	0.699	2.556	2.556	1.977	1.977
Number of Events	3	3	3	3	3	3
R Squared (First-Stage)	0.270	0.270	0.619	0.619	0.961	0.961
Observations	29,952	29,952	24,825	24,825	29,952	29,952
Year FE	X	X	X	X	X	X
Year of Arrival x Country FE	X	X	X	X	X	X

* p < 0.1, ** p < 0.05, *** p < 0.001

The sample period is 2000-2023, with cohorts arriving from 1990 to 2023. The unit of observation is arrival cohort by country-of-origin by calendar year. The events are El Salvador, Haiti, and Nepal receiving designations. Log(Real Wages) is the mean of the log of real wages, not the log of the mean. The sample is constructed based on averages from individual level data for immigrants aged 18-64, not in group quarters. Weights are by the population from a given country in the year 2000. Standard errors are clustered at the arrival year by country-of-origin level. The method of estimation is the Borusyak, Jaravel, and Speiss imputation estimator. Population is constructed as the sum of the individual weights, but is also weighted. Liberia and South Sudan have been excluded, as have countries in the OECD's Development Assistance Committee, including the EU. Data from countries that received a TPS designation before 2000 have been excluded. Similarly, data from a country after any successful termination event for that country has been excluded. Data from cohorts that arrived after the first TPS designation for their country have been excluded.

Appendix Table A11: Effects of TPS Events on Visa Overstays and Arrivals, with Travel Ban Countries

	Log(I-94 Arrivals)			Log(Overstays)		
	(1)	(2)	(3)	(4)	(5)	(6)
Initial Designation	0.199**			0.172		
	(0.101)			(0.145)		
First Redesignation		-0.102			-0.531***	
		(0.086)			(0.132)	
Termination			-0.058			-0.451***
			(0.041)			(0.130)
Mean Dep. Variable	5.990	8.175	6.761	5.564	7.549	6.562
R Squared (First-Stage)	0.967	0.898	0.908	0.947	0.936	0.913
Observations	34,364	1,367	1,650	1,413	52	119
Year FE				X	X	X
Month x Year FE	X	X	X			
Country FE	X	X	X	X	X	X

* p < 0.1, ** p < 0.05, *** p < 0.001

Visa overstay data is from the estimates in the Department of Homeland Security's Entry/Exit Overstay Reports. Visa refusal data comes from the State Department's Visa Office and combines B class tourist and business visas. I-94 arrival data comes from the National Travel and Tourism Office, monthly from 2000 to 2019 (later data exists but has not been used because of the disruption caused by COVID-19). The sample period for overstays is 2014 to 2024 and the sample period for refusals is 2006 to 2024. Standard errors are clustered by country. For I-94 arrivals, values for a country that is treated are only included within 12 months of the treatment date. Countries affected by the Trump administration's first travel ban have been included. Overstays are weighted by 2014 overstays, refusals are weighted by 2006 refusals, and arrivals are weighted by January 2000 arrivals. Liberia and South Sudan have been excluded, as have countries in the OECD's Development Assistance Committee, including the EU. For attempted terminations and terminations, countries that never had TPS or did not yet have TPS have been excluded. For attempted terminations, countries that were actually terminated were excluded after the termination. For initial designations, countries that have been actually terminated have been excluded.

B Data appendix

TPS Events and Eligibility. We construct the dataset of Temporary Protected Status events by collecting all Federal Register notices between 1991 and 2025 for the 28 ever TPS-designated countries. For each country, we compile the complete chronology of designations, redesignations, and terminations, recording announcement dates, effective dates, continuous-residence cutoffs, and eligibility criteria. We cross-check Federal Register data against Congressional Research Service reports to ensure completeness and make corrections to reflect the de facto legal environment faced by migrants. This timeline allows us to identify which immigrant cohorts were eligible for work authorization and deportation protection in each survey year.

We convert the universe of Federal Register notices between 1991 and 2025 into a binary indicator that varies by country of origin, arrival cohort (year of first U.S. entry), and survey year.[22] A cell is coded as treated when migrants from that origin were protected from removal and eligible to work legally. The procedure unfolds as follows:

We begin by compiling, for each TPS-eligible country, the full chronology of initial designations, subsequent redesignations, and terminations.[23] We record the date on which the notice was announced, the effective date of that notice, the scheduled expiration or termination date, the cutoff date by which individuals must already have been in the United States, and any registration or re-registration deadlines. Whenever available, we also include an estimate of how many individuals are likely to be covered, the stated basis for designation (e.g., "ongoing armed conflict," "environmental disaster," or "extraordinary conditions"), and a link to the original source.

Notices that never took practical effect are corrected so the timeline mirrors the de facto legal environment faced by migrants.[24] In addition to the Federal Register, we cross-check

[22]The Federal Register is the official repository for all TPS actions (designations, extensions, terminations, redesignations). The links of each country to these notices can be found here. Note that some links do not go directly to the notice PDF but to a Department of Justice website with the register number and page; these have to be searched manually.

[23]We also collect data on TPS extensions and minor notices, although these do not modify the set of individuals eligible for TPS.

[24]Terminations that were later blocked in court (El Salvador, Haiti, Honduras, Nepal, Nicaragua, Sudan) are relabeled "halted" and ignored for purposes of treatment continuity, but analyzed as attempted terminations. Sudan's 2022 and Haiti's 2021 designation actions are recast as redesignations because they simply moved the continuous-residence cut-off forward without interrupting eligibility. Two Liberian terminations that were

our dataset and fill gaps in older notices using Congressional Research Service (CRS) reports back to 2004.

The legal framework defining eligibility operates through three distinct types of actions.[25] A designation establishes TPS for the first time, making everyone already present in the United States on or before the continuous-residence date potentially eligible. We rely exclusively on continuous-residence dates—rather than continuous-physical-presence dates—because the former ultimately determines eligibility.[26] A redesignation extends TPS to later arrivals by advancing the continuous-residence cut-off while ensuring that all earlier beneficiaries retain their status. In this framework, a cohort is eligible if it arrived on or before the latest cut-off then in place.

Terminations end TPS for the entire country by the recorded effective date. We code TPS ineligibility for every cohort beginning in the first calendar year after the termination takes effect. The only exception occurs when a termination is legally halted, in which case eligibility continues uninterrupted. Since we use annual data for migrants' outcomes, we ignore month-level precision and partial-year exposure, collapsing federal notice cut-off dates to calendar years. When a country receives a fresh designation after a completed TPS cycle, eligibility restarts for all cohorts arriving on or before the new cut-off date.[27]

ACS sample. We construct our analysis sample from the American Community Survey (ACS) and the 2000 Census subsample spanning 2000 to 2023 (Ruggles et al., 2025). In particular, we use the 2000 5% weighted state sample, the 2001–2005 annual ACS weighted samples corresponding to up to 1% of the population (depending on the year), and the 2005–2023 annual ACS weighted samples corresponding to 1% of the population in each year. We restrict these samples to individuals living in households because the 2001-2005 data only reports household residents.

The sample includes non-citizen immigrants ages 18-64 living in households, drawn

formally rescinded are dropped altogether.

[25] We ignore Deferred Enforced Departure (DED) for the moment although these post-TPS episodes are functionally similar to TPS, since they grant work authorisation and deportation relief.

[26] For instance, Liberia's 2002 redesignation required continuous residence by October 2002 even though physical presence was set for August 2004.

[27] With the exception of Liberia, the data contain no redesignations inside these second- or third-cycle designations.

from 178 countries of origin. We define immigrants as individuals who were not born in any US state or outlying US area or territory or born abroad to US citizens. 24 countries of origin have ever received a TPS designation during our sample period. We use the year of immigration variable to assign immigrant cohorts, spanning from 1980 to 2023.

We construct a pseudo-panel by pooling immigrant cohorts at the country-of-origin × year of arrival × survey year level, creating an unbalanced panel structure. For each cell in this panel, we calculate average values of our main variables using person weights provided by IPUMS. Our main variables include:

- *Employment and labor market:* Employment and labor force participation are defined using standard IPUMS definitions. Hourly wages are calculated only for individuals who worked more than one hour per week in the previous year, with allocated wage values removed from the analysis. We adjust wages for inflation using the previous year's Consumer Price Index, with 2006 being the baseline, and trim real earnings per hour below $1 and above $100 to address outliers. Total income includes all sources and is similarly inflation-adjusted.

- *Population counts:* For each country-cohort-year cell, we record both weighted and unweighted counts of individuals to construct stocks and flows of immigrants from each arrival cohort and country-of-origin in a given sample year.

Identifying likely-undocumented immigrants. Undocumented immigrants are more likely to file for TPS benefits. We follow the procedure in **?**. We identify immigrants who are likely to be legal US residents based on any of the following characteristics:

- Arrived in the U.S. before 1980[28]

- Citizens and children under 18 years old reporting at least one U.S.-born parent

- Recipients of Social Security benefits, Supplemental Security Income, Medicaid, Medicare, military insurance, or public assistance

- Households with at least one citizen who received SNAP benefits

[28]Note that immigrants arriving 1980-1990 are dropped in the main specifications

- People in the Armed Forces and veterans
- Working in occupations requiring a license
- Working in occupations that immigrants are likely to be on H-1B or other visas, including computer scientists, professors, engineers, and life scientists
- Government employees and people working in the public administration sector
- Any of the above conditions applies to the householder's spouse
- Any of the above conditions apply to the head of the household, for that person's spouse and children in the household.

Immigrants who do not meet these criteria are classified as undocumented. An alternate specification drops immigrants who arrived from a given country in a given year when many arrivals from that country-year combination were refugees.

Code. R Core Team (2025) and StataCorp (2025) were used to analyze the data. In R, Eddelbuettel (2025), Richardson et al. (2025), Heinzen et al. (2021), Robinson et al. (2025), Barrett et al. (2025), Butts (2021), Bergé (2018), Wickham et al. (2025), Lüdecke et al. (2019), Arel-Bundock (2022), Wickham and Bryan (2025), **?**, Walker (2025), Arel-Bundock (2025), and Garnier et al. (2024) are the packages that were used in any step of the analysis, including preliminary analyses that are not included in this paper.