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IZA DP No. 18135

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ABSTRACT

Beyond the Border: Labor Market Effects of U.S. Immigration Enforcement Policies in El Salvador*

By 2020, one in four Salvadorans lived abroad, with 88 percent residing in the United States. The remittances to GDP ratio was about 25 percent, highlighting the country's dependence on migration. This paper examines the effects of a major U.S. immigration enforcement program—Secure Communities—on migration and labor market outcomes in El Salvador. Using a shift-share identification strategy, we find that larger exposure to the program decreases the likelihood that a household includes a migrant, consistent with increased forced returns. These effects lead to lower income among male workers, particularly low-educated, informal workers, and those in agriculture. We also document a decline in the probability of receiving remittances. The findings suggest that a closure of migration opportunities can increase labor market competition and strain local economies. Effects are most pronounced in municipalities with limited absorptive capacity, underscoring the unintended consequences that U.S. immigration enforcement generates abroad.

JEL Classification: F22, F24, N16, R23

Keywords: immigration policies, remittances, labor markets, El Salvador

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1 Introduction

The economic benefits of international migration for both origin and destination countries have been well-documented in the literature (Clemens, 2011). But what happens when migrants' fortunes are reversed? Recent shifts in immigration policies —particularly those in the United States— have presented significant challenges to migrants and their families. A prominent U.S. immigration enforcement policy, Secure Communities, was designed to increase deportations by expanding the role of local law enforcement and U.S. Immigration and Customs Enforcement (ICE). This program was responsible for a significant rise in deportations between 2005 and their peak in 2013 (East et al., 2023).

Previous studies have largely focused on the effects of this policy on outcomes for U.S.-born individuals.[1] However, its consequences are also deeply felt in countries with strong migration ties to the U.S., such as Mexico and countries of Northern Central America. For example, recent research has shown how precariousness created by deportations can translate into lower educational levels of children (Caballero, 2022) and feed into the dynamics of violent crime (Ambrosius, 2021; Rozo et al., 2021; Ambrosius, 2024). In this paper, we examine the labor market effects of immigration enforcement policies in migrant-sending countries, focusing on El Salvador. These policies can influence economic outcomes in countries of origin through several mechanisms, including increased deportations, reduced emigration, and changes in remittance flows.[2] Our hypothesis is that the primary channel operates through increased deportations. Nevertheless, our main estimates should be interpreted as reduced-form effects of immigration enforcement policies. In supplementary analyses, we assess the relative importance of these mechanisms using alternative empirical strategies.

[1]This literature includes effects on labor outcomes (East et al., 2023; East and Velásquez, 2022), elder and child-care (East and Velásquez, 2022; Ali et al., 2024; Almuhaisen et al., 2024), health outcomes (Watson, 2014; Tome et al., 2021; Alsan and Yang, 2022; Martinez-Donate et al., 2024), education (Bellows, 2021), and local conditions, such as crime and housing prices (Miles and Cox, 2014; Hines and Peri, 2019; Gonçalves et al., 2024; Howard et al., 2024).

[2]A related strand of research shows that labor supply shocks from labor mobility restriction can affect labor outcomes and local conditions at origin (Mansour, 2010; Cinque et al., 2025).

The effects of immigration enforcement policies on origin countries are not theoretically unambiguous, and recent empirical evidence from Mexico illustrates the complexity of these impacts. Several studies have examined how return migration driven by U.S. enforcement policies affects labor market and development outcomes, reaching different conclusions. Diodato et al. (2023) estimate deportation-driven return migration and find heterogeneous effects: while individuals in similar occupations experience short-term wage declines, likely due to intensified labor competition, others benefit from the human capital brought back by returnees. Similarly, Osuna Gomez and Medina-Cortina (2023) exploit variation in the rollout of Secure Communities to predict deportations and find that firms more exposed to returning migrants see increased profits and longer survival, consistent with productivity gains. Bucheli and Fontenla (2022), while not focusing directly on labor markets, document positive effects of return migration on human development indicators, reinforcing the idea that returning migrants bring valuable resources to their communities. In contrast, Pearson (2023), using a similar identification strategy, finds that greater exposure to deportees leads to wage declines, particularly among less-educated male workers and in informal sectors —those most similar to returnees—suggesting that labor market competition is a key mechanism. Finally, Caballero (2022) observes a higher school dropout in municipalities being more exposed to Secure Communities, driven by a decline in remittances. These divergent findings point to multiple mechanisms —labor market competition, human capital transfer, and changes in household resources—through which deportation shocks may affect origin economies.

A likely reason for the seemingly contradictory findings is that outcomes depend heavily on country-specific and heterogeneous migration contexts and the labor markets to which migrants return. The transmission of human or physical capital via return migration, for instance, depends on migrants' sociodemographic profiles and their lengths of stay abroad. Conditions of return also matter: voluntary returnees are more likely to bring capital and savings. However, it is empirically challenging to separate voluntary from forced returns, especially during periods of increased deportations and simultaneous voluntary return, as was the case following the 2008 Global Financial Crisis. The absorptive capacity of labor markets is also likely to shape the outcomes of deportation. Theories on dual labor markets in developing countries that go back to Lewis (1954) have emphasized how traditional sectors in developing countries –typically the rural and agricultural sectors– are characterized by a labor surplus with small or zero marginal productivity of labor. These sectors function as a pool of labor for modern (typically industrial) sectors. In emigration economies, it is often external labor markets that absorb surplus labor, not other domestic sectors. From this angle, we would expect a decrease in average incomes if labor supply increases, especially in sectors characterized by high levels of underemployment.

Beyond labor competition and human capital transfer, remittances represent a third potential channel. On the labor supply side, households facing a decline in remittance income may increase labor force participation to compensate for lost income. On the demand side, reduced remittances may lower household consumption, which in turn can dampen local demand for goods and services and reduce labor demand. Empirical evidence on the relationship between deportations and remittances remains mixed. Caballero (2022) finds that deportations reduce remittance flows, while Pearson (2023) documents an increase in the share of households receiving remittances following deportations. One possible explanation is that returnees themselves begin receiving remittances from family members who remain in the United States, which could mask any aggregate decline in remittance inflows. These contrasting findings further highlight the importance of local context and household dynamics in shaping the consequences of immigration enforcement policies abroad.

We focus on the Central American country of El Salvador, a country profoundly shaped by migration to the United States. The first large wave of migration from El Salvador to the U.S. had its roots during the Salvadoran Civil War in the 1980s, when many fled to the U.S. to seek refuge and opportunity. These flows have persisted and are sustained by migrant networks that lower migration costs. Ongoing violence, high unemployment, and weak economic conditions in El Salvador continued to drive emigration. Today, an estimated 25% of Salvadoran-born individuals reside outside the country. With approximately 2.3 million Hispanics of Salvadoran origin, these group constitutes the third-largest Hispanic-origin immigrant population in the U.S. (Noe-Bustamante et al., 2019). An estimated 741,000 Salvadorans in the U.S. are undocumented, making them vulnerable to deportation (Migration Policy Institute, 2019). Alongside Honduras, El Salvador has been among the most affected by forced returns in relative terms: Between 2000 and 2020, El Salvador received 315,000 deportees—equivalent to nearly 5% of its current population.[3] Finally, it is crucial to highlight that remittances account for 25% of El Salvador's GDP, reflecting El Salvador's enormous dependence on migrant income and underlining the central role of migration in its economy.

Several characteristics make El Salvador a particularly suitable case to examine the consequences of immigration enforcement policies implemented in the U.S.. Unlike Mexico, voluntary return played a negligible role during our period of analysis, removing a major source of empirical ambiguity. Second, deportees to El Salvador tend to have low education levels, and more than 80% had been in the U.S. for less than a year, according to Bandiera et al. (2023). This implies limited accumulation of human or physical capital abroad. Third, the labor market conditions in El Salvador offer few reintegration opportunities, where returnees can potentially add to a pool of reserve labor in rural sectors with low productivity. Small farmers - who represent 87% of agricultural producers - typically work on plots averaging just 1.2 hectares, and only 1.4% of agricultural land is irrigated. In addition, the largely subsistence agriculture-based economy in El Salvador is highly vulnerable to climate and weather shocks. These conditions have spurred rural migration to the United States and underscore the environmental stressors at play (Ibáñez et al., 2022). Many communities are deeply reliant on migrants' financial support: In some, 80% of households report

[3] Yearbooks of Immigration Statistics by Department of Homeland Security: https://ohss.dhs.gov/topics/immigration/yearbook

having a relative in the U.S. (International Organization for Migration, 2023). Economic activity is restricted by the territorial control of criminal organizations that have created informal borders and significantly curtailed mobility (Melnikov et al., 2020). These barriers further exacerbate the economic hardship faced by returnees and limit their opportunities of economic reintegration in a country that overwhelmingly relies on income earned abroad.

In a recent study, Bandiera et al. (2023) examined how formal labor markets in El Salvador respond to the arrival of deportees. Their results are consistent with the more adverse findings of Pearson (2023) in the Mexican context: deportations contribute to wage declines and reductions in formal employment, particularly in sectors with greater exposure to informal competition (e.g. agriculture, construction, wholesale and retail, and restaurants).

Our study builds on this research and makes several contributions to the literature. First, we use individual-level microdata from nationally representative surveys to examine a broad set of labor market outcomes, including both formal and informal employment, wage labor, self-employment, and employers, across multiple sectors and sociodemographic groups. This allows us to assess whether enforcement shocks lead to socioeconomic gains for certain groups, and if so, which ones. We also examine impacts at the household level, which provides insight into how individual family members are affected by migration policy shocks. Complementing the analysis of Bandiera et al. (2023), our approach expands the scope to investigate heterogeneous effects across sectors and various population groups. This broader perspective allows us to explore additional mechanisms behind labor market changes, including shifts in household labor supply, declines in remittance income, and increased participation in precarious employment.

Second, our causal identification strategy builds on prior studies in the Mexican case (Bucheli and Fontenla, 2022; Caballero, 2022; Pearson, 2023; Osuna Gomez and Medina-Cortina, 2023; Ambrosius, 2024), exploiting the staggered implementation of immigration enforcement policies intended to facilitate the deportation of unauthorized migrants. We employ Salvadoran consular data on migrants' counties of residence in the U.S. and their municipalities of birth to construct a shift-share index of municipalities´ average exposure to deportation threats in the United States. Since we know the distribution of Salvadoran migrants across the U.S. for 255 Salvadoran municipalities of origin, we can calculate an average annual exposure at the level of Salvadoran municipalities by connecting data on the implementation of Secure Communities in U.S. counties to migrants´ municipalities of birth. We then match this municipality level index to household and individual-level labor market data from the Multiple Purpose Household Survey (EHPM) of El Salvador from 2009 to 2019.

Our findings offer a nuanced view of the unintended consequences of immigration enforcement policies implemented in the destination country. We observe strong negative effects on income among wage workers, particularly in agriculture and among younger men — those most similar to deportees. The effects are strongest in precarious jobs (those lacking contracts and social security) and are not offset by gains in other sectors or among employers or self-employed. Simulations of hypothetical returns show that the size of the estimated effects cannot be explained by deportees' selection into low-wage work alone. We also find decreases in the likelihood of remittance reception, mainly driven by households whose head is a wage worker or self-employed, but not employers who actually exhibit an increase. Although we cannot fully disentangle all causal channels, the most likely mechanism behind the observed effect is an increase in labor supply without a corresponding rise in demand, resulting in broad wage declines. Our interpretation is that the closure of employment opportunities abroad, together with a decline in remittances, forces individuals to accept low-paid, insecure jobs they could previously afford to reject. We conclude that in economies strongly dependent on remittances, characterized by inflexible labor markets and low-skilled returnees, deportation shocks have deeply negative effects. This contrasts with the situation in U.S. labor markets, where immigrant labor is often complementary to native labor (Peri, 2016) and labor demand tends to be elastic. Recent studies have shown that removals also hurt native labor in the U.S. (East et al., 2023; East and Velásquez, 2022), creating the paradox that forced removals of migrant labor have negative wage effects on labor markets in both origin and destination countries.

The remainder of the paper describes the enforcement policies and our empirical model in Section 2. Section 3 describes our data and the main outcomes. We present our results in Section 4 and conclude in Section 5.

2 Enforcement Policies and Empirical Model

Our identification strategy leverages plausibly exogenous variation in migrants' exposure to immigration enforcement policies at their U.S. destinations. Specifically, we combine consular data on the geographic distribution of Salvadoran migrants across the United States with information on the staggered and unequal rollout of immigration enforcement policies at the county level. The timing of these policies allows us to create an indicator of policy exposure at the level of Salvadoran municipalities, which we used to explain labor market outcomes for households and individuals in migrant-sending communities.

2.1 Exposure to Secure Communities

Our analysis focuses on the period from 2009 to 2019, with particular attention to the Secure Communities program, a federal initiative launched in 2008. Secure Communities (SC) was designed to enhance the detection and deportation of undocumented immigrants by enabling the FBI to share fingerprint data from local law enforcement agencies with immigration authorities, who could then take enforcement action, such as issuing detainer requests. SC was implemented nationwide between 2008 and 2014, replaced by the Priority Enforcement Program in 2015, reinstated in 2018, and ultimately suspended in 2021. Our identification strategy exploits geographic variation in SC's rollout between 2008 and 2014, before its nationwide coverage. Crucially, SC implementation was not voluntary—counties did not opt into the program—mitigating concerns about policy endogeneity. A second set of policies intended to facilitate deportations were the 287(g) Agreements. Under Section 287(g) of the Immigration and Nationality Act (INA), different models had been in place that differed in terms of their implementation. All of them deputize local law enforcement to perform certain functions of federal immigration officials. Our analysis centers on Secure Communities due to its broader geographic coverage and the exogeneity in its timing of implementation. Unlike Secure Communities, the implementation of 287(g) policies was optional, as local jurisdictions could choose whether to adopt[4]. Data on the staggered implementation of enforcement policies come from The Urban Institute (Bernstein et al., 2022).

We use consular data with information on the U.S. counties of residence and the municipalities of birth of Salvadoran migrants, to map migrants' exposure to the risk of deportation to their municipalities of origin. Salvadoran consular data has been used previously to estimate the strength of migration networks (Anzoategui et al., 2014; Ambrosius, 2021; Contreras, 2023). Because passports can be used as identification for purposes such as sending remittances or opening a bank account, many undocumented migrants seek to obtain or renew their passports at one of the 17 Salvadoran consulates in the U.S. In our case, we rely on the stock of 569,000 Salvadoran migrants who requested a passport before 2008, prior to our period of analysis. Half of the Salvadoran population in the United States, approximately 750,000 individuals, is estimated to lack official documents (Migration Policy Institute, 2019).

To assess the quality of the data, we follow Contreras (2023) and compare the distribution of Salvadoran migrants as estimated from consular data with estimates of the Salvadoran-born population as obtained from the American Community Survey (ACS 2005–2009). Since the Public Use Microdata Sample (PUMS) sampling units in the ACS are not identical with counties, we use a crosswalk to approximate matches between PUMS units and

[4]We control for exposure to 287(g) policies, and the results are robust to including these controls.

counties. The visual comparison in Figure 1 indicates a strong correlation between both data sources, shown on a logarithmic scale. The strength of this correlation increases with the size of the diaspora[5] . Circle sizes in the figure are scaled relative to the population size of counties. We added the names of the four top destinations of Salvadorans: Los Angeles, Maryland (Montgomery), Dallas and Harris County in Texas.

We then calculate exposure to SC for 255 Salvadoran municipalities following a shift-share approach:[6]

$$SCPolicyExposure_{m,t} = \sum_{k=1} SC_{k,t} D_{k,m} \tag{1}$$

For each Salvadoran municipality m, the share of its diaspora D in destination county k is multiplied by a variable that indicates whether enforcement policy SC was in place in destination county k during year t. The variable SC takes the value 1 if the county of residence had SC in place. Values are then summed up across all destinations K. A value of 1 would indicate that all migrants from municipality m lived in U.S. counties that implemented SC. A value of 0 indicates that no migrants lived in counties that had adopted this policy. As an illustration, imagine migrants from a municipality of origin in El Salvador were distributed equally across three counties in the US. If one of these destinations had early adopted Secure Communities in 2010, then the Salvadoran municipality of origin would be assigned a deportation risk indicator of 0.33 for the year 2010. Figure 2 illustrates variation in exposure over time and across different municipalities: on the left axis, we show the average SC exposure between the years 2000 and 2020 for each of the 255 municipalities on which we have data as black lines. The vertical axis on the right depicts total annual deportations from SC to El Salvador. Peaks coincide with a stronger average exposure to SC policies

[5] Also, the 17 consulates are closer to counties with a larger size of the Salvadoran diaspora (Contreras, 2023), suggesting that consular data is less accurate in U.S. counties with fewer Salvadorans. Since the Salvadoran diaspora is relatively concentrated in few main destinations as evidenced in Figure 1 and confirmed by ACS data, this should not have a strong effect on the precision of our estimates.

[6] In the consular data, we use 255 out of a total of 262 Salvadoran municipalities. One municipality reported no data on migrants. Six Salvadoran municipalities have repeated names and could not be clearly assigned in the data

in the US. Figure 2 also shows that variation in exposure to Secure Communities between municipalities in El Salvador was limited to the years between 2009 and 2013. Figure 3 highlights the geographical and temporal variation in the exposure to Secure Communities in El Salvador between 2009 and 2013.

Our empirical strategy builds on several recent studies examining the Mexican case (Caballero, 2022; Bucheli and Fontenla, 2022; Ambrosius, 2024; Osuna Gomez and Medina-Cortina, 2023; Pearson, 2023). All of these studies leverage variation from U.S. immigration enforcement policies designed to increase deportations of undocumented immigrants by linking deportation threats in the United States with migrants' municipalities of birth via consular data (called "matrículas consulares" in the case of Mexico). Some of these studies have used exposure to policy implementation (Bucheli and Fontenla, 2022; Caballero, 2022; Ambrosius, 2024), while others have used predicted deportations by calculating municipalities exposure to the number of deportees reported at migrants´ destination in the United States (Osuna Gomez and Medina-Cortina, 2023; Pearson, 2023).

We construct our explanatory variable based on exposure to enforcement policies rather than observed deportations, following Caballero (2022); Bucheli and Fontenla (2022) and Ambrosius (2024). The number of deportations may itself depend on migration patterns, which could be influenced by poor socioeconomic conditions in El Salvador. If worse local labor market conditions or higher violence levels in certain Salvadoran municipalities drive increased migration to specific U.S. counties, this could in turn lead to higher deportation rates from those counties. In this scenario, poor labor outcomes would cause higher migration and subsequent deportations, reversing the intended causal direction of deportation shocks affecting Salvadoran labor markets. By using policy exposure as our explanatory variable, we rely on a less demanding exogeneity assumption: the timing of policy implementation across U.S. states must be independent of economic conditions in migrants' municipalities of origin, not necessarily the number of migrants. For instance, the fact that Harris County in Texas implemented SC earlier should be independent of labor market conditions in El Salvador. This assumption would hold even if more Salvadoran migrants from specific locations in El Salvador would have arrived in Harris County, and therefore, more persons would be potentially vulnerable to deportation.

Our second rationale for using exposure to enforcement policies, rather than the number of deportations, is that deportations represent just one channel through which the threat of removals might impact labor markets in origin municipalities. Other relevant channels include reduced emigration rates, increased voluntary return migration, and changes in remittance flows. Higher deportation risks may deter potential migrants or alter migrants' labor market outcomes, thereby indirectly affecting remittances (Osuna Gomez and Medina-Cortina, 2023; Caballero, 2022). Our analysis explicitly incorporates these broader channels.

To assess the relevance of deportations as the likely mechanism behind the observed effects as well as a way to take into account varying intensities of deportations, we complement model (1) with an instrumental variable approach in which we use migrants´ average exposure to SC policies as a predictor for exposure to the number of SC deportations in a first stage, and then use predicted SC deportations to explain labor market outcomes. Although our empirical model estimates the reduced-form effect of exposure to immigration enforcement policies, we assume identification primarily arises from increased deportations driven by Secure Communities.

2.2 Identifying Assumptions: Exogeneity of Shifts

Our strategy relies on the premise that municipalities receive more deportees when their migrants reside in U.S. states adopting policies that elevate deportation risk. Thus, our explanatory variable follows the shift-share or "Bartik" instrument approach (Bartik, 1991). Whereas the shifting variable in our case derives from short-term changes in policies at the level of U.S. states, the share variable is defined via historically grown migration corridors that create spatial variation in terms of migrants' exposure to these policies. Recent advances in the literature on Bartik-like variables have classified strategies depending on the source of exogenous variation either coming from shares (Goldsmith-Pinkham et al., 2020) or from shifts (Borusyak et al., 2022). Following the identification framework laid out by Borusyak et al. (2022), identification in our setting comes from the "shifts", not from the "shares". Two conditions must be met for this strategy to be valid. First, the number of shifting variables should be large. This condition is met, since we calculate shifts from the implementation of enforcement policies across 1,685 counties in the U.S. over a period of ten years and for which we record a presence of Salvadoran migrants. Second, the shifts must be exogenous with respect to the outcome variable. Unlike the "classical" version of Bartik shift-share variables, shifts in our case are defined externally from the timing of policies at the level of U.S. counties, and the shares are used to map these policies back to Salvadoran municipalities of origin. Although historical migration corridors (our share variable) are the result of migrants' intentional sorting across the US, the exogeneity assumption holds as long as the shifter—the unequal and staggered implementation of Secure Communities— is exogenous with respect to changes in labor market outcomes at origin. In other words, what matters for our identification strategy is that the drivers of adoption in the U.S. are unrelated to variation within El Salvador.

Previous literature has noted that early adopters of SC were predominantly counties with large Hispanic populations (Cox and Miles, 2013). Following Caballero (2022), we re-estimate our models excluding these early adopters and confirm that our results remain robust. Later adoptions of SC, however, can be considered effectively random (East and Velásquez, 2022; East et al., 2023). Crucially for our identification strategy, the timing of SC implementation should be unrelated to municipality-specific trends in our outcomes of interest in El Salvador. Theoretically, this is likely to hold because the timing of enforcement policies at the level of U.S. counties should not be related to whether historical migration corridors are connected to, say, the municipality of Santa Ana in the West or the municipality of Santiago de María in the East. It is essential to clarify that the "share" component of our Bartik instrument is established prior to any policy changes. Consequently, it is not influenced by short-term sorting of migrants into different destinations —particularly the possibility that migrants may avoid states that employ more hostile policies (Leerkes et al., 2013; Orrenius and Zavodny, 2022).

Methodologically, we complement previous evidence for El Salvador. Our approach differs from the shift-share strategy employed by Bandiera et al. (2023). In their study on the effect of deportations on formal wages in El Salvador, temporal shifts are obtained from time-varying aggregate deportations to El Salvador, while historical deportation rates at the local level provide variation at the level of municipalities. Their identification relies on the exogeneity of historical deportation rates (the "shares") at the local level, not the shifts. By using exposure to the policy itself rather than deportations directly, our approach further enables examination of the importance of alternative mechanisms beyond deportations alone, offering additional insights into how immigration enforcement policies affect labor market outcomes.

Typically, to test the validity of the identification assumption, we would test the parallel trend assumption. Since our outcome variables are collected starting only from 2009, we are not able to directly test differences in pre-exposure trends for our outcomes of interest. Therefore, we use a second-best variable with annual variation that allows us to test for parallel trends as far back as the year 2000: value-added tax measured at the municipality level. The amount of value-added tax (VAT) should be closely related to labor market indicators (levels of employment and wage levels), especially in the formal sector. To test for pre-exposure differences, we estimate linear trends in VAT for each municipality prior to exposure. Specifically, we predict the log of per capita VAT using year-fixed effects, municipality-fixed effects, and an interaction between year and municipality indicators. The coefficients on these interactions capture the municipality-specific linear pre-exposure trends.

Secure Communities started operating in 2008; thus, we estimate pre-trends over the period 2000–2007 for this policy. We then run a balance test estimating the effect of the intensity of exposure for SC on municipalities' pre-exposure linear trend. Intensity of exposure is defined as the cumulative exposure over the period 2008 to 2014. Pre-trends show no statistically significant correlations with exposure (Table A1).

2.3 Empirical Model

We are interested in the effect of migrants' exposure to enforcement policies at their destination counties in the U.S. on the behavior of households and individuals in their municipalities of origin. To this end, we estimate the following empirical model using OLS regression for the years 2009–2019:

$$Y_{imt} = \beta_0 + \beta_1 SCPolicyExposure_{mt-1} + X'_{mt-1}\beta_2 + C'_{it}\beta_3 + \gamma_m + \delta_t + t\omega_m + u_{imt} \qquad (2)$$

where Y_{imt} is the outcome of interest of individual or household i, living in municipality m in El Salvador, in year t. The unbalanced sample includes information from 231 municipalities, while the balanced sample covers 201 municipalities from which households were drawn for the survey measuring the outcomes of interest. We provide further details in the next section. For the remainder of the paper, we focus on the balanced sample. In the robustness checks, we also estimate the main outcomes using the unbalanced sample, and the results remain consistent. $SCPolicyExposure_{mt-1}$ is defined as explained above, and we estimate the effect of exposure to immigration enforcement policies in $t-1$ on the outcomes of interest measures at time t in El Salvador. This is because the effect of the policies might not be immediate. However, as a robustness test, we explore empirical models with an alternative timing in the deportation risk variable. Our model also includes a vector of time-varying shift-share measures (X'_{mt-1}), constructed using the same structure as our main exposure variable for Secure Communities, to account for local unemployment rates in the U.S. and exposure to

287(g) policies, another federal policy intended to facilitate deportations. Participation in 287(g) policies was voluntary and unemployment was a major driver of adoption (Barrera et al., 2025). We control for exposure to 287(g) to isolate the effect that stems from Secure Communities.

We control for municipality fixed effects, γ_m, survey year fixed effects, δ_t, and municipality-specific time trends, $t\omega_m$. Since many of the household or individual level variables are themselves affected by the enforcement policies, our baseline model does not include household level controls. In estimations at the individual level, we control for gender, age and years of education (individual controls C_{it}). Finally, since our explanatory variable of interest is measured at the level of municipalities, we cluster standard errors at the municipality level. For ease of interpretation, we use OLS in all regressions, including for binary outcomes.[7]

Identifying variation from the variable $SCPolicyExposure$ comes from the years 2008 to 2013, because all municipalities were equally exposed to deportation risk after 2014. We still use the full ten-year period 2009 to 2019 in the regression analysis for two reasons: For one, we are interested in deviations from longer-term trends as a result of exposure to enforcement. By using a longer period, we can include municipality-specific time trends in the regression that allow us to control for longer trends in labor market indicators. Second, including later years allows us to assess the lagged and persistent effects of exposure to these policies.

When estimating the regression, we use analytic weights based on the size of the diaspora at the municipal level, employing consular data from before 2008. This approach ensures that larger diaspora communities have a proportionally larger influence on the results. As a robustness check, we also re-estimate our main results using analytic weights that rely on the size of the diaspora as a proportion of the population at the municipality level, based on the 2007 census of El Salvador.

[7]Since none of our binary outcomes has distributions at the extreme (i.e. very rare or very common), we consider the use of OLS on binary outcomes to be a pragmatic choice with low costs in terms of precision.

2.4 Instrumental Variable Strategy

As mentioned in the previous section, our baseline estimation strategy relies on a reduced-form approach that captures the total effect of exposure to U.S. immigration enforcement policies on labor market outcomes in El Salvador. As discussed, several mechanisms may explain how these policies generate effects in migrant-sending countries like El Salvador. To explore the specific role of deportations as one such mechanism, we implement an instrumental variable (IV) strategy in which deportations, a potentially endogenous variable, are instrumented with exposure to immigration enforcement policies. The first stage of the IV model estimates the effect of exposure to the SC program on the number of deportations attributable to the policy, as follows:

$$SCDeportations_{mt} = \phi_0 + \phi_1 SCPolicyExposure_{mt} + X'_{mt-1}\phi_2 + C'_{it}\phi_3 + \gamma_m + \delta_t + t\omega_m + u_{imt} \tag{3}$$

The outcome variable $SCDeportations_{mt}$ in the first stage regression is an approximation of how many Salvadorans have been deported via SC, for each Salvadoran municipality m at time t, following the approaches used by Pearson (2023) and Osuna Gomez and Medina-Cortina (2023). This indicator is constructed using the same weighting procedure described above for $SC\ Policy\ Exposure_{mt}$, in which predicted deportations at the municipality level are calculated as a weighted average based on the distribution of Salvadoran migrants across U.S. counties. Data on SC removals at the county level have been obtained from the Transactional Records Access Clearinghouse (TRAC).

We then use predicted Secure Communities deportations in a second-stage regression to explain outcome variables Y_{imt}, as follows:

$$Y_{imt} = \alpha_0 + \alpha_1 \widehat{SCDeportations_{mt-1}} X'_{mt-1}\alpha_2 + C'_{it}\alpha_3 + \gamma_m + \delta_t + t\omega_m + u_{imt} \tag{4}$$

For the IV approach to be a valid causal estimate, we must assume that policy exposure affects labor market outcomes only through its impact on deportations. This exclusion restriction may be violated, given that exposure to enforcement policies is likely to affect labor outcomes through other channels too. As such, the IV results should be interpreted with caution. Nonetheless, comparing the reduced-form and IV estimates can help assess the relative importance of this channel.

3 Data and Outcome Variables

Our outcome and control variables come from the Multiple Purpose Household Survey (EHPM), is a yearly cross-sectional survey collected by El Salvador's official statistics agency that includes information on household members' sociodemographic characteristics, housing, income, employment, and migration, among other elements. The survey is nationally representative and also representative for 50 selected municipalities. These municipalities include provincial capitals as well as others that are particularly notable or distinct based on their sociodemographic characteristics. The sample in the estimations covers 209,644 households and 489,676 adults for 2009–2019. We are using information from up to 201 municipalities out of which households were sampled and for which we have data on exposure to Secure Communities in the balanced sample (231 in the unbalanced sample). Descriptive statistics of our sample are shown in Appendix Table A2 and A3 at the individual and household levels, respectively. Among our sample, most of the participants did not complete high school (72%) and are employed (62%). Among households, 16% report having a migrant at the time of the survey and 24% reported receiving remittances [8].

3.1 Migration

We begin by assessing the effect of municipality-level exposure to Secure Communities on various household-level measures of international migration. We do this to confirm that our

[8]The share of households reporting remittances is larger than those who report a migrant in the U.S., because respondents do not necessarily count close relatives living abroad as household members.

identifying variable has the expected effect on migration. We measure household migration using three indicators. First, we create an indicator of whether at least one household member was living outside the country during the survey year.[9] This measures the impact on both return migration and deterrence to emigrate. Unfortunately, our data does not allows us to distinguish both of these channels. Second, we estimate the effect on the total number of migrants in the household. Third, we create an indicator of minors with parental absence due to migration. We expect a lower likelihood of parental absence as a result of municipality-level enforcement shocks either because previously absent parents have recently returned; or because fewer parents are departing towards the U.S..

3.2 Labor Outcomes

We next examine how local labor markets in El Salvador respond to U.S. immigration enforcement policies. Several mechanisms suggest that these policies can influence labor market outcomes in migrant-sending countries.

First, enforcement policies may increase labor supply in origin communities through two channels: forced return migration and reduced emigration. An increase in the number of returnees or a decline in out-migration raises labor market competition, potentially depressing wages and employment outcomes for non-migrants—particularly for those with similar profiles to returnees. Prior studies show that return migrants are predominantly working-age males with low levels of education, many of whom reenter the informal sector upon return (Bandiera et al., 2023). In our context, we are unable to separate return migration from deterrence effects, but we expect both to contribute to observed increases in the labor supply.

Second, returnees may generate spillovers of financial or human capital. Migrants who return with savings or skills acquired abroad may stimulate local economies through increased consumption, investment, or knowledge transfer (Bucheli and Fontenla, 2022).

[9] In our period of interest, between 93 percent and 95 percent of household members living abroad resided in the United States.

However, if deportees return with limited experience or resources, as appears to be the case in El Salvador (Bandiera et al., 2023), these potential gains may be limited, and spillovers could even be negative.

Third, enforcement policies may affect remittance flows. The loss of remittances following deportations can reduce household consumption and investment, reinforcing negative labor supply and demand effects. Caballero (2022) finds that immigration enforcement in the U.S. reduced the likelihood of receiving remittances in Mexico, with important consequences for school enrollment and child labor. In contrast, Pearson (2023) documents an increase in remittance receipt, as migrants may respond to worsening conditions at home by increasing financial transfers for their families.

Taken together, these mechanisms suggest that the net effect of enforcement policies on origin-country labor markets depends on the relative strength of supply- and demand-side forces. This, in turn, is shaped by the characteristics of returnees—including their skills, time abroad, and available resources—as well as the absorptive capacity of local labor markets (Bucheli and Fontenla, 2022; Caballero, 2022; Bandiera et al., 2023; Pearson, 2023).

We focus on a set of labor market outcomes that capture both extensive and intensive margin responses and provide insight into the mechanisms outlined above. First, we examine whether individuals are employed at the time of the survey. Second, we analyze the sector of employment, distinguishing between agriculture, construction, services, and other sectors. Among the employed, we assess job quality through hours worked, labor income, and access to employment benefits such as formal contracts and social security. We also consider the type of employment—wage work, self-employment, or business ownership—to detect changes in labor force composition and shifts between formal and informal employment.

3.3 Well-being Indicators at the Household Level

We next examine how U.S. immigration enforcement policies affect household-level outcomes in El Salvador, focusing on both direct and indirect channels. As a primary measure of household well-being, we study total income and total expenditure, as well as spending on food. Changes in these outcomes may be explained by multiple mechanisms. On the one hand, households may experience direct income losses due to reduced remittances if family members abroad are deported or face worse labor market conditions. On the other hand, enforcement shocks may indirectly affect household income through local labor market dynamics as discussed in the previous section. Expenditure patterns may also change in response to income shocks, changes in household composition, or a higher uncertainty about future migration opportunities.

4 Results

4.1 Migration

Table 1 shows the results of equation 2 on migration-related outcomes. Specifically, it examines the impacts on the probability of having at least one migrant in the household (column 1), the total number of migrants in the household (column 2), and the likelihood of having at least one minor with an absent parent (column 3).

All migration-related indicators show the expected signs: exposure to SC is associated with negative and statistically significant effects on migration outcomes.[10] Specifically, the results in column 1 show that exposure to SC reduces the probability that a household has at least one migrant. As a reminder, the shift-share instrument equals one when all migrants from municipality m reside in U.S. counties that implemented SC in year $t-1$. Full exposure

[10] The results remain robust when we include each control sequentially. For the sake of brevity, Appendix Table A4 presents only the estimates from column 1 of the main specification, adding each control one at a time

to SC leads to a 11 percentage point decrease in this probability, compared to households in municipalities without any exposure. Typically, the differences in exposure intensity between the most and least affected municipalities within a given year are smaller (see Figure 4). We therefore interpret this and other coefficients at average levels of exposure relative to no exposure. At the average exposure level of 0.51 for SC, this corresponds to a reduction of 5.6 percentage points. When examining the number of migrants per household in column 2, full exposure to SC is associated with 0.2 fewer migrants per households, which corresponds to a 33% reduction evaluated at the mean of the outcome and exposure. Results are consistent when analyzing the probability that a minor lives without a parent in the household in column 3. Exposure to SC is associated with a statistically significant decrease of 15.8 percentage points on the probability of parental absence, or 8.1 percentage points evaluated at the mean of exposure, suggesting that increased enforcement may reduce the separation of children from their parents—potentially by limiting new migration or increasing return migration.[11] Overall, these results indicate that enforcement programs reduce the presence of migrants in the household. These patterns may reflect deterrence effects, return migration, or a combination of both, each with distinct implications for the labor market.

4.2 Labor Market Outcomes

We begin by examining the effects of enforcement exposure on the probability of employment in Table 2.[12] Panel A reports the results for all working-age respondents, while Panels B and C present the disaggregated effects for men and women, respectively. Differences by sex may reflect the extent to which male and female workers operate in distinct labor market segments with varying degrees of substitutability with deportees.

Column 1 of Panel A shows that exposure to SC in $t-1$ is associated with a lower probability of being employed. This effect appears to be temporary, with no evidence of a

[11] We interpret these results with caution, as the sample size is reduced by nearly half for this outcome.

[12] Results for the probability of employment with sequentially added controls are shown in Table A5. The findings remain robust across alternative specifications.

persistent decline beyond the first year (Figure A1 and Table A6). The significant effects are concentrated among men (Panel B), although the point estimates for women (Panel C) are of similar magnitude. This suggests that the underlying patterns are similar across sexes, although the estimates for women are less precisely estimated, likely due to smaller effective sample sizes or greater variability. When disaggregating by employment category, the coefficients are imprecisely estimated but reveal suggestive patterns: the probability of employment appears to decline across all categories. A plausible interpretation is that, in the short run, some sectors are unable to absorb the additional labor supply, and displaced workers face constraints in reallocating to less affected categories. Consistent with this interpretation, the sector-level analysis shows no statistically significant differences.

We next examine the effects of enforcement exposure on salaries and overall labor income. Table 3 shows the impact on hourly labor income conditional on being employed, disaggregated by employment type across columns and by sex across panels. When examining the results for all workers in Panel A, we find no statistically significant effects on earnings for employers or the self-employed, but we do observe a statistically significant decline in hourly wages among wage workers.[13] At average levels of exposure, these reductions correspond to declines of 32.6%, relative to individuals in municipalities with no exposure. As shown in Panel B, these effects are driven by declines among men. For these respondents, we find statistically significant declines in both self-employment income (a 21% reduction at the mean level of exposure) and wage work income (a 54% reduction at the mean). This is consistent with the fact that most deportees are men, and thus competition in the labor market is concentrated among these workers. The lack of a significant effect among women, despite similar coefficient magnitudes, suggests that women are less exposed to the competitive pressures triggered by enforcement exposure. It also suggests segmentation of labor markets in El Salvador by sex. Together, these findings lend support to the labor market competition

[13]Results for the income per hour of wage workers with sequentially added controls are shown in Table A5. The findings remain robust across alternative specifications.

channel.

Results across sectors further suggest that men who remain in the labor force may either transition into lower-quality jobs or experience wage reductions within their current employment. These new or adjusted positions tend to be less well-paid, leading to persistent earnings losses that extend beyond the initial enforcement exposure. The wage effects are sustained over the medium term and intensify in the second and third year following exposure (For conciseness, we restrict the presentation to the effects for wage workers in Figure A2 and Table A6).

As mentioned above, a potential mechanism behind these results is the arrival of return migrants who, upon reentering the local labor force, compete for jobs in sectors where substitution is more likely, particularly in informal segments of the economy with few entry barriers. However, the observed decline in labor income may also reflect compositional changes in the labor force. For instance, if deported individuals return to lower-paying jobs, the average wage could decline not because of increased competition affecting existing workers, but due to a shift in the earnings distribution. In this case, part of the observed effect may be driven by selection, capturing the relatively low earnings of deported individuals rather than wage declines experienced by non-migrant workers.

We conduct a simple exercise presented in Appendix Table A7 to assess the extent to which the observed effect could be attributed to a selection mechanism—specifically, the possibility that returnees earn lower wages than the general population. We simulate hypothetical scenarios in which varying numbers of returnees (1%, 5%, and 10% of the working-age population) are added to the working population, with assumed wages at different points in the wage distribution (fixed at zero, the first decile, and the first quartile). The results indicate that even under the most extreme scenario—where returnees make up 10 percent of the workforce and earn wages at the very bottom of the distribution—the resulting decline in average wages of wage workers still falls short of explaining the magnitude of the estimated coefficient in column 3 of Table 3[14]. This holds true even if the official number of deportees substantially underestimates the true number of returnees, for example, if deportees return with family members who are not captured in official statistics.

We conclude that selection into low-wage employment alone is unlikely to account for the observed effect and that the substantial negative impact is more plausibly driven by broader labor market dynamics. First, the increase in labor supply may be significantly larger than the number of deportees, as the risk of deportation also deters new emigration. Second, the loss of remittance income may compel family members who previously relied on these transfers to enter the labor force and to accept low-paid jobs. Third, intensified competition for a fixed number of jobs may exert downward pressure on all wages. Taken together, these mechanisms likely contribute to the pronounced negative effect of exposure to deportation risk on wages. In the next sections, we provide suggestive evidence aimed at disentangling these mechanisms more clearly.

4.2.1 Heterogeneity by Demographic and Employment Characteristics

To better understand the mechanisms underlying the observed labor market effects, we next explore heterogeneity in the results across key worker characteristics. Specifically, we disaggregate the analysis by education level and sector of employment to assess whether the effects are concentrated among groups more likely to compete directly with return migrants. If labor market competition is a relevant channel, we would expect the strongest effects among workers with lower levels of education and those employed in sectors with high informality and limited entry barriers—such as agriculture, construction, or domestic services.

In the next sections, we focus on wage workers, as this group shows the most consistent and robust effects across different dimensions of heterogeneity. The results for self-employed men show similar patterns, with a strong and significant decline in hourly income and com-

[14] In this scenario, wages would decrease by 22% at most. As a comparison: The results in Table 3 show a decrease of 32% for municipalites with average exposure to SC compared to municipalities with no exposure.

parable results by education to those observed for wage workers. However, further disaggregation by sector of employment or job characteristics does not show statistically significant results for this group, likely reflecting smaller sample sizes. In addition, because most self-employed workers are informal, it is not possible to examine outcomes by contract type or access to social security. For self-employed women, we do not observe negative effects, and the estimated coefficients are close to zero.

Effect by Demographic Groups: Gender and Age Consistent with the previous results, Figures 4 and Appendix Table A8 show that the negative effects on wage income are concentrated among men. This aligns with prior findings by Pearson (2023) for the case of Mexico, who shows that workers with demographic profiles similar to those of deportees experience the largest labor market impacts. In the context of El Salvador, return migrants are primarily men who seek employment in sectors such as agriculture, construction, and other labor-intensive industries with high levels of informality, where men are overrepresented (Bandiera et al., 2023). The increased competition in these sectors likely exacerbates the wage decline for local male workers. However, the patterns by age do not reveal substantial differences across groups.

Effect by Employment Status The competition mechanism is also evident when examining the effects by sector and employment type. The negative impacts on wage income are significantly larger for workers in the informal sector, those without access to social security benefits, or those who lack a formal contract (Figure 5 and Table A9). These findings are consistent with our expectation that return migrants are more likely to enter the informal labor market, intensifying competition among workers in these sectors. Informal sector jobs typically require fewer specialized skills, making them accessible to a larger pool of workers, including return migrants, and driving down wages for incumbent workers. This pattern is also consistent with the overall negative effect found for self-employed men; however, as discussed earlier, we cannot stratify self-employment outcomes by access to formal contracts given that most self-employed workers are informal.

Effect by Education The competition hypothesis is further supported by the results disaggregated by education levels (Figure 6 and Table A10). We observe significant negative effects on wage incomes for men at the lower end of the educational distribution, while college graduates experience a positive effect. This suggests that the labor market impacts of deportations are uneven, with lower-educated workers, who are likely closer substitutes for return migrants, bearing the brunt of increased competition. Conversely, the positive effects observed for college-educated workers may indicate that reduced production costs, driven by a cheaper and more competitive labor force in certain sectors, can boost overall production and benefit specific groups of workers. In this case, higher-educated individuals, whose roles may complement rather than compete with return migrants, appear to gain from these dynamics. However, since the number of observations for college-educated workers is relatively small, we are cautious in drawing strong conclusions from these coefficients.

Effects by Sector The negative effects on wage income are particularly pronounced in the agricultural sector (Figure 7 and Table A11), which is characterized by high levels of informality and a labor force that likely overlaps with the demographic profile of return migrants. These results strengthen our interpretation that deportations disproportionately impact men working in informal and low-skill sectors. Moreover, the magnitude and significance of the effects are especially strong in rural areas, characterized by fewer employment opportunities outside agriculture (Table A12). Again, these results align with those of Pearson (2023) for the case of Mexico, who also finds particularly strong effects in the agricultural sector.

4.2.2 Heterogeneity by Municipality Characteristics

We now turn to examining whether certain characteristics of municipalities mitigate or exacerbate the observed effects. To address this, we focus on five baseline characteristics. Table A13 presents the effects on hourly wages among all wage workers. For each characteristic, we estimate the effects separately for municipalities below and above the median of the distribution. Specifically, we consider the poverty rate (column 1); the share of the population employed in agriculture (column 2); the youth unemployment rate (column 3); homicides per capita (column 4); and the presence of gangs (column 5). Further details on these variables and their sources can be found in the notes of Table A13. In El Salvador, territorial control by gangs, particularly in urban areas, may influence how communities experience the effects of return migration. As noted by Melnikov et al. (2020), gangs control not only their own neighborhoods but also nearby areas, imposing restrictions on mobility, extortion, and other forms of violence that affect both individuals and firms. These dynamics could amplify the negative labor market impacts of deportation if returnees face greater barriers to reintegration, or they could attenuate them if returnees are unable to access the labor market, thereby reducing potential competition. All but gang presence are measured prior to the period of analysis to avoid endogeneity concerns.

Results suggest that the negative effects on the income of wage workers are concentrated in municipalities below the median of the poverty distribution, that is, in less poor areas prior to the enforcement shock; where the agricultural sector was smaller prior to the enforcement shock; where rates of youth unemployment are higher; and where violence and gangs are widespread. Consistent with our broader narrative regarding labor reallocation, constrained absorption capacity, and the vulnerability of communities heavily dependent on migration, this reflects the vulnerability of rural economies that rely heavily on migration and remittances and where few alternative income opportunities exist. On the one hand, in many rural economies, international migration served as a key pathway out of poorly paid agricultural work, leading to higher household income, reduced poverty, and declining reliance on low-productive agricultural activities (Gammage, 2006). Forced returns may reverse this trend, pushing workers back into informal and low-wage agricultural employment. We also see a stronger effect in municipalities affected by the presence of gangs and high rates of unemployment among the youth, both of which are indications of weaker labor market conditions and greater structural frictions. Apparently, in these contexts, enforcement shocks force the population into low-paid (agricultural) wage labor that they were previously able to reject.

4.3 Mechanisms

As hypothesized, one of the main channels through which exposure to U.S. immigration enforcement policies affects labor outcomes in El Salvador is an increase in deportations, which raises labor market competition. The heterogeneity patterns documented above suggest that competition is indeed an important channel, as the individuals most affected are those most likely to compete in the labor market with deported migrants. Another channel is a reduction in remittance flows. The following sections provide additional suggestive evidence on the relevance of each mechanism.

Deportations: IV strategy To assess the role of deportations in explaining our observed effects on local labor markets in El Salvador, we estimate an instrumental variable (IV) model as outlined in equations 3 and 4. In this approach, the first stage estimates the impact of Secure Communities exposure on the predicted number of SC deportations, while the second stage examines the effect of predicted SC deportations on labor outcomes. This strategy allows us to explore whether increased deportations are a key mechanism through which U.S. immigration enforcement affects labor market conditions in El Salvador.

The lower panel of Table 4 presents the IV results for hourly income among wage workers and across different groups. The upper panel reports the reduced-form estimates discussed earlier, included here to facilitate comparability of the results. However, due to different scales of the explanatory variable, we can not directly compare the size of the coefficients and instead focus on comparing their sign and significance. The IV results show significant negative effects of deportations on hourly income for both wage workers and self-employed individuals in the same subgroups where the reduced form identified significant effects, particularly among men. This consistency between the reduced-form and instrumented second stage results strengthens the interpretation that deportations and labor market competition are a central mechanism through which U.S. immigration enforcement generates labor market disruptions in origin countries. The F-statistics exceed the conventional threshold of 10 in all specifications, confirming the strength of the instrument. However, as mentioned previously, enforcement exposure could affect labor outcomes through other channels too, such as changes in remittances or migration intentions. Since these "backdoor" mechanisms of enforcement shocks violate the exclusion restriction, our main emphasis lies on the reduced form estimates of the upper panel.

Remittances Another hypothesized mechanism through which exposure to immigration enforcement policies may affect economic outcomes in El Salvador is through a decline in remittance flows. Remittances may decrease directly due to deportations or indirectly due to a worsening of employment conditions for migrants in the United States, as documented by East and Velásquez (2022) and East et al. (2023). In Table 5, we estimate the effects of enforcement exposure on two outcomes: the likelihood of receiving remittances at the household level (column 1) and the natural log of the annual amount received, conditional on receipt (column 2).

The results in column 1 show that exposure to Secure Communities in period $t-1$ is associated with a 20% reduction in the probability of receiving remittances, evaluated at the mean levels of exposure and the outcome. This suggests that immigration enforcement policies in the U.S. have a sizable negative effect on the likelihood that households in El Salvador receive financial support from abroad. Among households that do receive remittances, the coefficient on the amount received is positive but imprecisely estimated.[15] As such, while enforcement exposure appears to reduce the extensive margin of remittance receipt, we find no clear evidence of an effect on the intensive margin.

[15] We test different transformations to estimate the effects on the amount of remittances received, but across specifications, the results remain statistically insignificant.

4.4 Expenditure

Having documented the effects of enforcement exposure on labor market outcomes and remittances, we now turn to examining whether these shocks translate into broader measures of household well-being. In particular, we analyze impacts on total household expenditure and food expenditure. This is a critical next step, as reductions in household spending—especially on food—may signal constrained consumption smoothing in response to income shocks. Such adjustments could have lasting implications, particularly for children's nutritional intake and long-term human capital development.

Table 6 presents the estimated effects of enforcement exposure on household income per capita, total expenditure per capita, and food expenditure per capita. While all coefficients are negative, they are imprecisely estimated when measured at the household level. Importantly, the lack of positive effects on total household expenditure provides suggestive evidence against one of the alternative mechanisms discussed earlier—namely, increased consumption from return migrants arriving with financial resources. If returnees were bringing back savings and using these resources in their communities of origin, we would expect to observe increases in household consumption, particularly in non-essential spending. The lack of evidence supporting increased consumption in our data suggests that this mechanism may not be a dominant factor in the Salvadoran context, at least in the short term. Instead, the evidence appears more consistent with labor market pressures.

4.5 Robustness Checks

We conducted several robustness checks to assess the validity of our findings (Table A15). First, we test the sensitivity of our results to alternative samples in columns 2–5. For comparison, our main results are presented in column 1. The results are robust when using the complete number of observations, and therefore a unbalanced sample of municipalities (column 2), and when excluding San Salvador, the capital and largest urban center in the country (column 3). This latter check addresses the possibility that San Salvador's distinct economic structure could be driving the results. Similarly, our results are robust when considering only 50 self-represented municipalities, which are defined as provincial capitals and other municipalities that are particularly relevant and distinct due to their sociodemographic characteristics (column 4). One potential concern is that the observed effects may be influenced by exposure to Secure Communities in U.S. counties that adopted the program during its early years, specifically in 2008 and 2009, which may have been more selective. To address this, we re-estimated our results by excluding the top 5% of municipalities with the highest exposure to Secure Communities in those years (see column 5), and our results remained consistent. This consistency further supports the robustness of our main estimates. Our findings are also robust after incorporating additional controls for homicide rates (column 6) and high temperatures (column 7) in the previous year. We also show that our results remain consistent when using an alternative measure to account for the size of the diaspora. Instead of using the number of consular data emitted before 2008, we re-estimate our main results using the number of migrants as a proportion of the population at the municipal level using the national census of 2007 (see column 8). Our results are robust.

5 Conclusions

This paper contributes to the growing literature on the economic effects of immigration enforcement policies by examining how forced return migration shapes labor market outcomes in origin countries. While existing studies have documented mixed effects of deportations on local economies, especially in the case of Mexico, evidence from other contexts remains limited. We use the case of El Salvador, a country with deep migratory ties to the United States, high remittance dependence, and limited reintegration capacity, to assess the labor market consequences of exposure to U.S. deportation policies.

Our analysis draws on rich household survey data linked to an original measure of municipal-level exposure to enforcement policies in the U.S., constructed using administrative data from Salvadoran consulates. We exploit plausibly exogenous variation in enforcement intensity driven by the staggered implementation of a major U.S. immigration program, Secure Communities, to estimate its effect on labor market outcomes in El Salvador between 2009 and 2019.

We find robust and economically significant negative effects on income from male workers, particularly among young, low-educated men, and workers without contracts or social security coverage. These effects are driven by workers in agriculture, those residing in more violent municipalities, and in municipalities with higher rates of youth unemployment. These are the municipalities and sectors in which capacities to reintegrate returnees are particularly weak. Importantly, we find no offsetting increases in income from self-employment or business ownership, and no gains among employers. These patterns suggest that the return of migrants does not lead to positive spillovers through capital accumulation or entrepreneurship, in contrast to some findings in the Mexican context. Simulations with hypothetical returnees show that the observed effects on average wages cannot be fully explained by the low earning potential of deportees alone. In contexts characterized by limited labor market flexibility and high dependence on migration, policies that restrict access to employment abroad can generate negative economic effects in countries of origin, beyond the individuals directly affected. Our results point to a combination of competition in the labor market and a reduction in remittances as key mechanisms. These findings underscore the need for policies to facilitate the reintegration of deportees, to create conditions for investment and employment in countries of origin, and to mitigate the social and economic disruptions associated with forced return.

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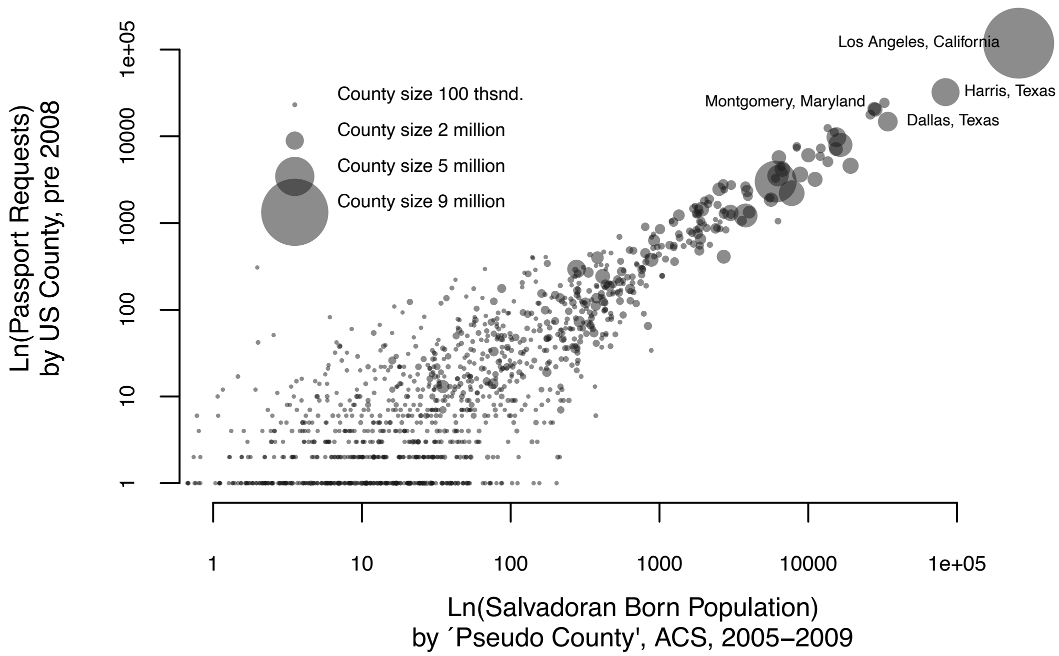
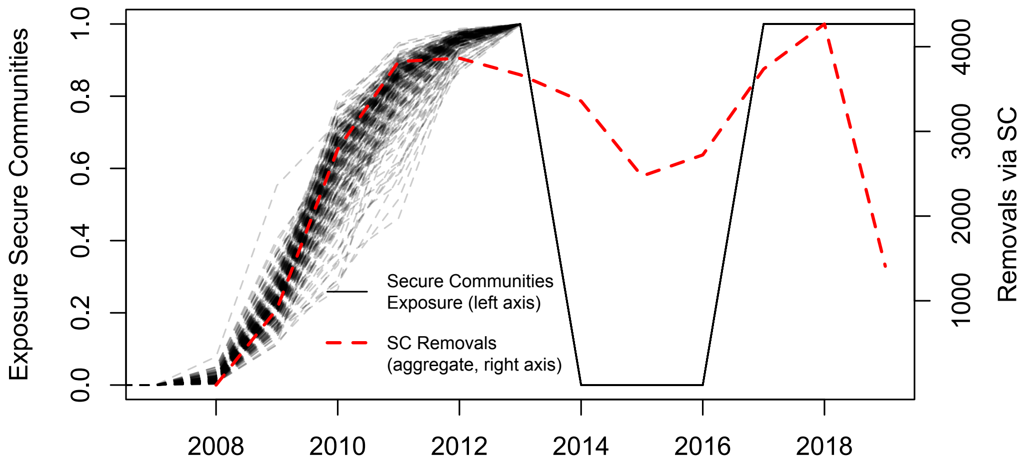
6 Figures

Figure 1: Correlation of Consular Data with Salvadoran Population



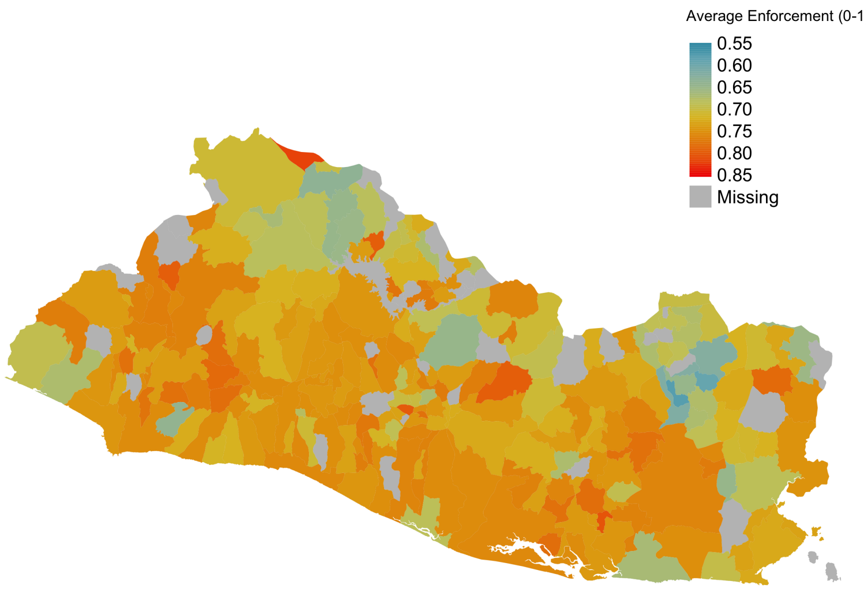
Source: The vertical axis depicts the distribution of the Salvadoran population according to a stock of 569,000 Salvadoran migrants who requested a passport before 2008. The horizontal line depicts the distribution of Salvadoran-born population as estimated from the American Community Survey 2005-2009, using a crosswalk to match PUMA sampling units to counties. The circle size is drawn proportional to the population size of counties.

Figure 2: Exposure to Secure Communities and Total Deportations via Secure Communities



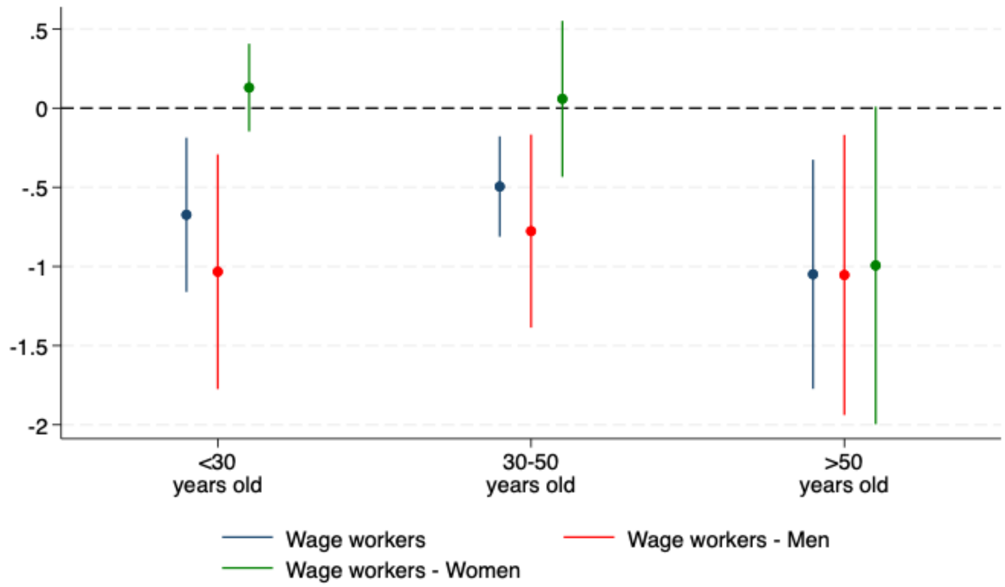
Source: Each black line represents a municipality in El Salvador. The vertical axis depicts the average exposure of migrants from these municipalities to the immigration enforcement policy Secure Communities lagged by one year. The red line is the annual deportations via the Secure Communities program to El Salvador. Data on Secure Communities comes from Gelatt et al. (2017) and data on the number of Salvadorans deported via SC comes from Transactional Records Access Clearinghouse (TRAC).

Figure 3: Average Secure Communities Exposure by Municipality between 2009 and 2013



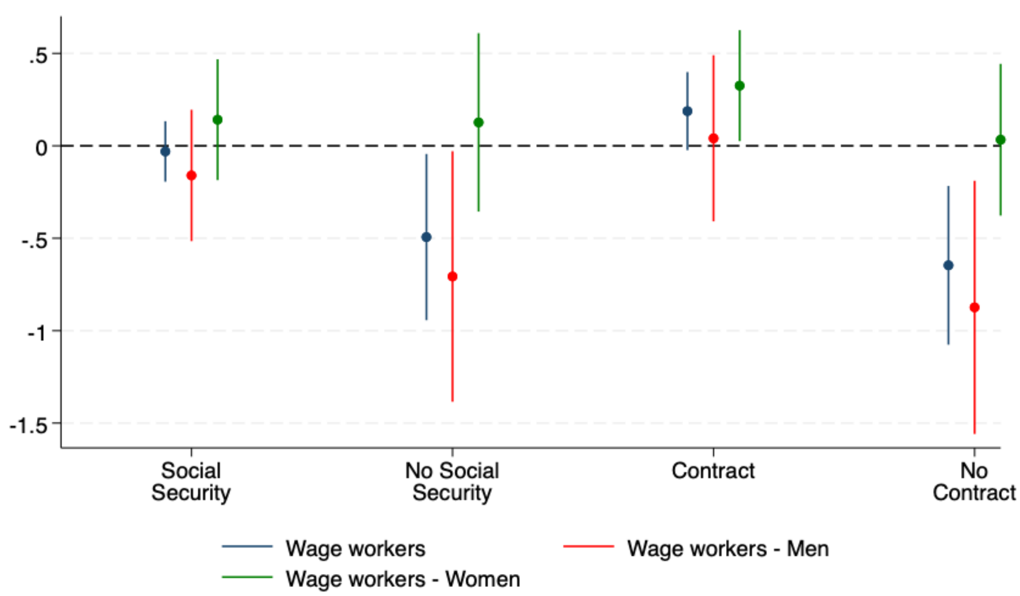
Source: Own elaboration. Exposure to Secure Communities is created from data on the implementation of Secure Communities at the level of U.S. counties and comes from Gelatt et al. (2017). Consular data to connect U.S. counties and Salvadoran municipalities of origin comes from División General de Migración y Extranjería.

Figure 4: Effect of Secure Communities on Ln(Income per Hour) of Wage Workers – By Age and Gender



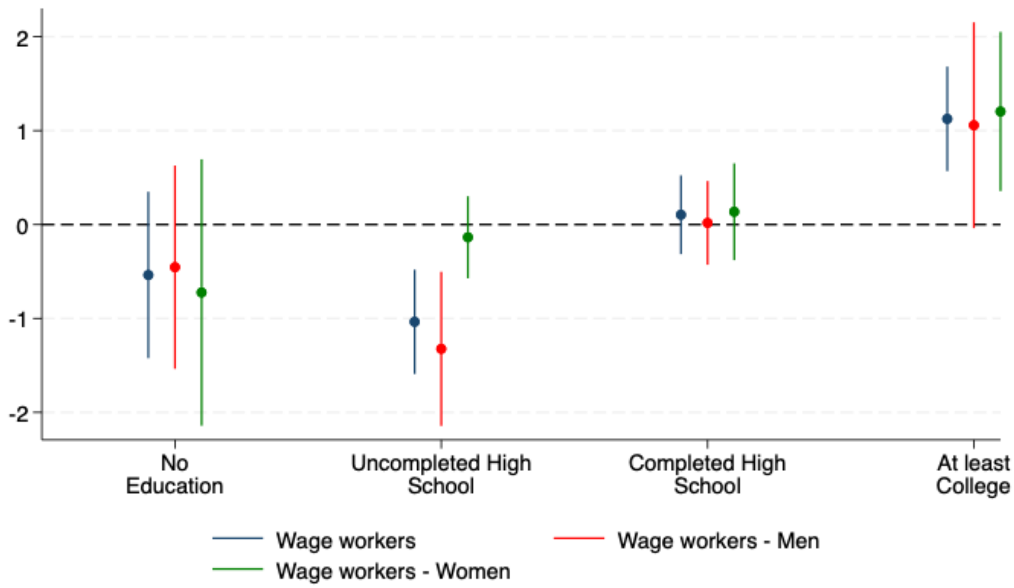
Notes: Data from 2009–2019 of El Salvador's Multiple Purpose Household Survey (EHPM). Each line represents a different estimation. The plot graphically summarizes the corresponding estimates reported in Table A8.

Figure 5: Effect of Secure Communities on Ln(Income per Hour) of Wage Workers – By Access to Social Security and a Formal Contract



Notes: Data from 2009–2019 of El Salvador's Multiple Purpose Household Survey (EHPM). Each line represents a different estimation. The plot graphically summarizes the corresponding estimates reported in Table **??**.

Figure 6: Effect of Secure Communities on Ln(Income per Hour) of Wage Workers – By Education



Notes: Data from 2009–2019 of El Salvador's Multiple Purpose Household Survey (EHPM). Each line represents a different estimation. The plot graphically summarizes the corresponding estimates reported in Table **??**.

Figure 7: Effect of Secure Communities on Ln(Income per Hour) of Wage Workers – By Economic Sector of Employment

Notes: Data from 2009–2019 of El Salvador's Multiple Purpose Household Survey (EHPM). Each line represents a different estimation. The plot graphically summarizes the corresponding estimates reported in Table **??**.

7 Tables

Table 1: Effect of Secure Communities on Migration Indicators at the Household Level

	(1) At least one migrant	(2) Number of migrants	(3) At least one minor with an absent parent
Secure Communities (t-1)	-0.119*	-0.226*	-0.163***
	(0.069)	(0.128)	(0.061)
Constant	2.468	8.009	-0.389
	(4.008)	(11.753)	(2.316)
Mean Outcome	0.163	0.303	0.084
Number of Observations	211245	211245	128890
Clusters	201	201	201
Municipal & Year FE	X	X	X
Municipality Time Trends	X	X	X
Weights	X	X	X
287(g)	X	X	X
Unemployment US	X	X	X

Notes: Household-level data from 2009–2019 of El Salvador's Multiple Purpose Household Survey (EHPM). The dependent variables are as follows: in column 1, a dummy indicating whether a household member migrated; in column 2, the number of migrants at the household level; and in column 3, a dummy indicating that there is at least one minor in the household with a parent absent due to migration. In columns 1 and 2, the sample includes all households, while the estimations in column 3 only include households with at least one minor and excludes information collected in 2009 since in this year this question was not asked. The explanatory variable of interest is a shift-share instrument between 0 and 1 that accounts for exposure to Secure Community policies in the previous year. We also include a time-varying shift-share measure constructed using the same structure as our variable of interest, which takes into account exposure to 287(g) policies from the previous year, as well as local unemployment rates in the United States for the same year. All regressions are estimated via OLS and include municipality and year fixed effects, as well as municipality-specific time trends. We utilize analytic weights based on the size of the diaspora at the municipal level, using consular data prior to 2008. Standard errors are clustered by municipality and year. *p<0.1; **p<0.05; ***p<0.01

Table 2: Effect of Secure Communities on the Probability of Employment – By Type of Employment

	(1) Employed	(2) Employer	(3) Self-employed	(4) Wage Worker
Panel A: All				
Secure Communities (t-1)	-0.092**	-0.021	-0.058*	-0.013
	(0.042)	(0.015)	(0.035)	(0.039)
Constant	2.678*	0.532	0.570	1.573
	(1.576)	(0.564)	(1.108)	(0.984)
Mean Outcome	0.619	0.024	0.195	0.400
Number of Observations	497144	497144	497144	497144
Clusters	201	201	201	201
Panel B: Men				
Secure Communities (t-1)	-0.119**	-0.035	-0.024	-0.060
	(0.047)	(0.029)	(0.044)	(0.066)
Constant	5.290***	0.005	0.004	5.281***
	(1.634)	(0.975)	(1.859)	(1.709)
Mean Outcome	0.807	0.037	0.207	0.562
Number of Observations	225781	225781	225781	225781
Clusters	201	201	201	201
Panel C: Women				
Secure Communities (t-1)	-0.061	-0.008	-0.084**	0.031
	(0.061)	(0.015)	(0.042)	(0.051)
Constant	0.496	0.991**	1.191	-1.692
	(2.692)	(0.422)	(1.556)	(1.597)
Mean Outcome	0.463	0.013	0.185	0.266
Number of Observations	271363	271363	271363	271363
Clusters	201	201	201	201
Municipal & Year FE	X	X	X	X
Municipality Time Trends	X	X	X	X
Weights	X	X	X	X
287(g)	X	X	X	X
Unemployment US	X	X	X	X
Individual controls	X	X	X	X

Notes: Individual-level data from El Salvador's Multiple Purpose Household Survey (EHPM) for people 18–75 years old from 2009–2018. The dependent variables are as follows: in column 1, a dummy indicating whether the individual is employed (i.e. has a job); in column 2, a dummy indicating whether the individual is an employer; in column 3, a dummy indicating whether the individual is self employed; and in column 4, a dummy indicating whether the individual is a wage worker. Column 1 includes all individuals, while Columns 2 to 4 restrict the sample to employed individuals. Panel A shows results for all individuals; Panels B and C show results for men and women separately. The explanatory variable of interest is a shift-share instrument between 0 and 1 that accounts for exposure to Secure Community policies in the previous year. Fixed effects, municipality time trends, municipality controls, and analytical weights are the same as in Table 1 Individual controls are age, gender, and years of education. Standard errors are clustered by municipality and year. *p<0.1; **p<0.05; ***p<0.01

Table 3: Effect of Secure Communities on Ln (Income per Hour) – By Type of Employment

	(1) Employer	(2) Self-Employed	(3) Wage Workers
Panel A: All			
Secure Communities (t-1)	0.129	-0.115	-0.640***
	(0.373)	(0.182)	(0.197)
Constant	12.529	17.198**	-9.931
	(9.137)	(6.722)	(8.527)
Mean Untransformed Outcome	6.452	3.290	1.544
Number of Observations	7670	70987	176155
Clusters	196	201	201
Panel B: Men			
Secure Communities (t-1)	-0.050	-0.398*	-0.951***
	(0.365)	(0.208)	(0.333)
Constant	21.898*	-3.846	-8.157
	(11.601)	(16.942)	(11.253)
Mean Untransformed Outcome	6.811	3.598	1.499
Number of Observations	4704	23791	114917
Clusters	185	201	201
Panel C: Women			
Secure Communities (t-1)	0.698	0.022	0.028
	(0.801)	(0.243)	(0.180)
Constant	-8.412	27.102***	-12.273
	(22.751)	(6.801)	(7.528)
Mean Untransformed Outcome	5.888	3.134	1.628
Number of Observations	2940	47196	61238
Clusters	164	201	201
Municipal & Year FE	X	X	X
Municipality Time Trends	X	X	X
Weights	X	X	X
287(g)	X	X	X
Unemployment US	X	X	X
Individual controls	X	X	X

Notes: Individual-level data from El Salvador's Multiple Purpose Household Survey (EHPM) for people 18–75 years old from 2009–2018. The dependent variable is the natural logarithm of the income per hour of employers (column 1), self-employed (column 2), and wage workers (column 3). The mean values before transformation are reported. Panel A shows results for all individuals; Panels B and C show results for men and women separately. The variable of interest is a shift-share instrument between 0 and 1 that accounts for exposure to Secure Community policies in the previous year. Fixed effects, municipality time trends, municipality controls, and analytical weights are the same as in Table 1. Individual controls are age, gender, and years of education. Standard errors are clustered by municipality and year. *p<0.1; **p<0.05; ***p<0.01

Table 4: Effect of Secure Communities on Ln (Income per Hour) of Wage Workers – Different specifications

	(1) All	(2) Men	(3) No social security	(4) No contract	(5) Uncompleted High School	(6) Agriculture	(7) Rural
Panel A: Reduced form							
Secure Communities (t-1)	-0.640***	-0.951***	-0.494**	-0.646***	-1.036***	-1.941**	-0.802**
	(0.197)	(0.333)	(0.228)	(0.218)	(0.282)	(0.755)	(0.310)
Constant	-9.931	-8.157	-8.820	-8.812	-4.281	-15.613	-0.396
	(8.527)	(11.253)	(10.871)	(9.932)	(11.286)	(21.160)	(12.150)
Mean Untransformed Outcome	1.544	1.499	1.135	1.200	1.230	0.955	1.277
Number of Observations	176155	114917	98699	118343	94272	30526	71195
Clusters	201	201	201	201	201	199	173
Panel B: Instrumental Variable							
Secure Communities Salvadorian deportations / 100 (t-1)	-0.135***	-0.203***	-0.139***	-0.219***	-0.440***	-0.043	-0.202***
	(0.036)	(0.064)	(0.041)	(0.054)	(0.159)	(0.027)	(0.073)
Constant	0.190	0.436	0.479	0.396	1.474	-0.471**	1.220**
	(0.450)	(0.684)	(0.425)	(0.496)	(1.313)	(0.225)	(0.495)
Mean Untransformed Outcome	1.544	1.499	1.200	1.230	0.955	1.725	1.277
Number of Observations	176155	114917	118343	94272	30527	104960	71195
Clusters	201	201	201	201	200	176	173
Municipal & Year FE	X	X	X	X	X	X	X
Municipality Time Trends	X	X	X	X	X	X	X
Weights	X	X	X	X	X	X	X
287(g)	X	X	X	X	X	X	X
Unemployment US	X	X	X	X	X	X	X
Individual controls	X	X	X	X	X	X	X

Notes: Individual-level data from El Salvador's Multiple Purpose Household Survey (EHPM) for people 18–75 years old from 2009–2018. The dependent variable is the natural logarithm of the income per hour of wage workers. The mean values before transformation are reported. In panel A, the variable of interest is the number of a shift-share instrument between 0 and 1 that accounts for exposure to Secure Community policies in the previous year. We refer to this estimation as the reduced form. Panel B reports instrumental variable estimates, where deportations due to Secure Communities (per 100 population) are instrumented using the shift-share variable. Fixed effects, municipality time trends, municipality controls, and analytical weights are the same as in Table 1. Individual controls are age, gender, and years of education. Standard errors are clustered by municipality and year. *p<0.1; **p<0.05; ***p<0.01

Table 5: Effect of Secure Communities on Remittances at the Household Level

	(1) Received remittances	(2) Ln (Remittances)
Secure Communities (t-1)	-0.105*	0.152
	(0.054)	(0.185)
Constant	1.761	-23.922**
	(3.326)	(11.780)
Mean Outcome*	0.244	2152.086
Number of Observations	211245	49218
Clusters	201	201
Municipal & Year FE	X	X
Municipality Time Trends	X	X
Weights	X	X
287(g)	X	X
Unemployment US	X	X

Notes: Household-level data from El Salvador's Multiple Purpose Household Survey (EHPM) from 2009–2018. The dependent variables are as follows: in column 1, a dummy indicating whether the household received remittances, and in column 2, the natural logarithm of the annual value of remittances among the sample of households that received remittances. In column 2, the mean values before transformation are reported. The variable of interest is a shift-share instrument between 0 and 1 that accounts for exposure to Secure Community policies in the previous year. Fixed effects, municipality time trends, municipality controls, and analytical weights are the same as in Table 1. Standard errors are clustered by municipality and year. *p<0.1; **p<0.05; ***p<0.01

Table 6: Effect of Secure Communities on Income and Expenditure at the Household Level

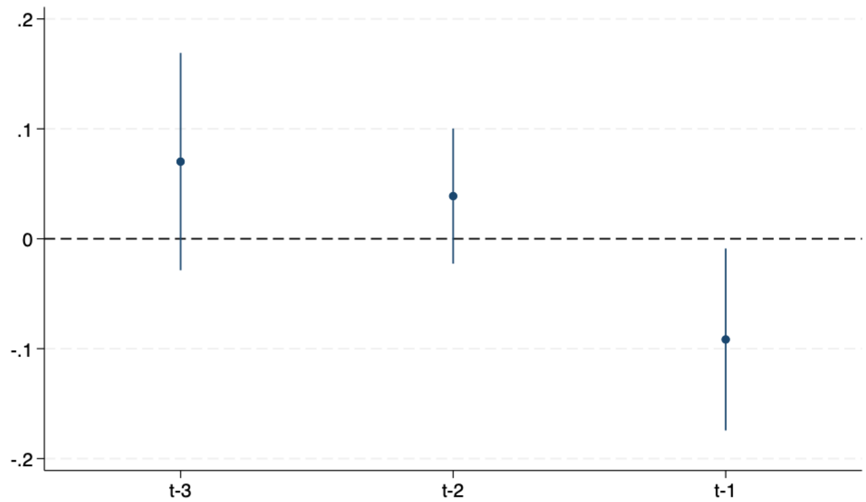
	(1) Ln (Income per capita)	(2) Ln(Expenditure per capita)	(3) Ln(Food Expenditure per capita)
Secure Communities (t-1)	-0.138	-0.139	-0.153
	(0.133)	(0.138)	(0.113)
Constant	13.488*	15.559**	9.317*
	(8.155)	(6.222)	(4.776)
Mean Untransformed Outcome	136.962	97.470	42.978
Number of Observations	211245	211245	211245
Clusters	201	201	201
Municipal & Year FE	X	X	X
Municipality Time Trends	X	X	X
Weights	X	X	X
287(g)	X	X	X
Unemployment US	X	X	X

Notes: Household-level data from El Salvador's Multiple Purpose Household Survey (EHPM) from 2009–2018. The dependent variables are as follows: in column 1, the natural logarithm of the annual income per capita, in column 2, the natural logarithm of the annual expenditure per capita; and in column 3, the natural logarithm of the annual food expenditure per capita. The mean values before transformation are reported. The variable of interest is a shift-share instrument between 0 and 1 that accounts for exposure to Secure Community policies in the previous year. Fixed effects, municipality time trends, municipality controls, and analytical weights are the same as in Table 1. Standard errors are clustered by municipality and year. *p<0.1; **p<0.05; ***p<0.01

8 Appendix

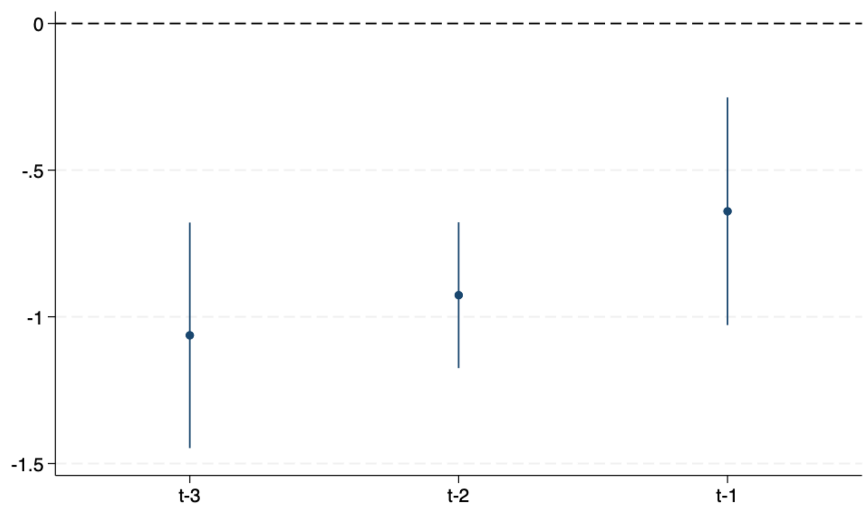
8.1 Figures

Figure A1: Effect of Secure Communities on Likelihood of Employment



Notes: Data from 2009–2019 of El Salvador's Multiple Purpose Household Survey (EHPM). Each line represents a different estimation. The plot graphically summarizes the corresponding estimates reported in Table A6.

Figure A2: Effect of Secure Communities on Ln(Income per Hour) for Wage Workers



Notes: Data from 2009–2019 of El Salvador's Multiple Purpose Household Survey (EHPM). Each line represents a different estimation. The plot graphically summarizes the corresponding estimates reported in Table A6.

8.2 Tables

Table A1: Predicted Exposure Using Pre-Trends

	Ln(VAT): Pre-Exposure Trends
Secure Communities	0.384
	(0.284)
Sample size	250

Notes: We calculate linear trends for each municipality for the log of VAT per capita, based on the pre-exposure period 2000 to 2007. Coefficients measure correlations between linear trends before exposure, and the mean intensity of exposure to SC between 2007 and 2014. Standard errors in parentheses are statistically insignificant at conventional levels.

Table A2: Descriptive Statistics at Individual Level

	Sample EHPM (Individual Level)
Age(years)	38.74
Men	0.45
No formal education	0.15
Less than High School	0.56
High School	0.26
At least College	0.03
Employed	0.62

Notes: Individual-level data from El Salvador's Multiple Purpose Household Survey (EHPM) for people 18–75 years old from 2009–2018.

Table A3: Descriptive Statistics at Household Level

	Sample EHPM (Household Level)
At least one migrant	0.16
Number of migrants	1.86
Parent migrated	0.06
Receives remittances	0.24
Value remittances	2152.09
Income per capita	136.96
Expenditure per capita	97.47
Food expenditure per capita	42.98
Number of members	3.87
Age of the HH	49.00
HH is a man	0.64
HH years of education	5.54
HH is employed	0.71

Notes: Household-level data from 2009–2019 of El Salvador's Multiple Purpose Household Survey (EHPM).

Table A4: Effect of Secure Communities on Migration Likelihood at Household Level – Adding Controls

	(1)	(2)	(3)	(4)	(5)	(6)
Secure Communities (t-1)	-0.033***	-0.145***	-0.142***	-0.188**	-0.123*	-0.119*
	(0.003)	(0.044)	(0.045)	(0.077)	(0.069)	(0.069)
Constant	0.180***	0.238***	-0.536	-0.344	1.777	2.468
	(0.010)	(0.023)	(2.250)	(4.814)	(3.946)	(4.008)
Mean Outcome	0.163	0.163	0.163	0.163	0.163	0.163
Number of Observations	211245	211245	211245	211245	211245	211245
Clusters	201	201	201	201	201	201
Municipal & Year FE		X	X	X	X	X
Municipality Time Trends			X	X	X	X
Weights				X	X	X
287(g)					X	X
Unemployment US						X

Notes: Household-level data from 2009–2019 of El Salvador's Multiple Purpose Household Survey (EHPM). The dependent variable is a dummy indicating whether a household member migrated and is estimated via OLS. The explanatory variable of interest is a shift-share instrument between 0 and 1 that accounts for exposure to Secure Community policies in the previous year. Fixed effects, municipality time trends, municipality controls, and analytical weights are the same as in Table 1. Standard errors are clustered by municipality and year. *p<0.1; **p<0.05; ***p<0.01

Table A5: Effect of Secure Communities on Employment Indicators – Adding Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Probability of employment							
Secure Communities (t-1)	0.007***	-0.047*	-0.053*	-0.077*	-0.078*	-0.080**	-0.092**
	(0.002)	(0.028)	(0.029)	(0.045)	(0.042)	(0.040)	(0.042)
Constant	0.616***	0.644***	2.019*	2.198	2.143	1.852	2.678*
	(0.004)	(0.015)	(1.102)	(1.553)	(1.512)	(1.363)	(1.576)
Mean Outcome	0.619	0.619	0.619	0.619	0.619	0.619	0.619
Number of Observations	497144	497144	497144	497144	497144	497144	497144
Clusters	201	201	201	201	201	201	201
Panel B: Ln (Income per Hour) of Wage Workers							
Secure Communities (t-1)	0.239***	-1.208***	-1.120***	-0.981***	-0.700***	-0.663***	-0.640***
	(0.011)	(0.140)	(0.155)	(0.217)	(0.212)	(0.201)	(0.197)
Constant	-0.030	0.723***	-20.805**	-23.527*	-14.423	-10.890	-9.931
	(0.021)	(0.073)	(10.420)	(13.014)	(10.514)	(9.979)	(8.527)
Mean Untransformed Outcome	1.544	1.544	1.544	1.544	1.544	1.544	1.544
Number of Observations	176155	176155	176155	176155	176155	176155	176155
Clusters	201	201	201	201	201	201	201
Municipal & Year FE		X	X	X	X	X	X
Municipality Time Trends			X	X	X	X	X
Weights				X	X	X	X
287(g)					X	X	X
Unemployment US						X	X
Individual Controls							X

Notes: Individual-level data from El Salvador's Multiple Purpose Household Survey (EHPM) for people 18–75 years old from 2009–2018. The dependent variables are as follows: in panel A, a dummy indicating whether the individual is employed (i.e., has a job), and in panel B, the natural logarithm of the income per hour of wage workers. In panel B, the mean values before transformation are reported. The explanatory variable of interest is a shift-share instrument between 0 and 1 that accounts for exposure to Secure Community policies in the previous year. All regressions are estimated via OLS. Fixed effects, municipality time trends, municipality controls, and analytical weights are the same as in Table 1. Individual controls are age, gender, and years of education. Standard errors are clustered by municipality and year. *p<0.1; **p<0.05; ***p<0.01

Table A6: Effect of Secure Communities on Employment Indicators – Different Lags

	(1)	(2)	(3)
Panel A: Probability of employment			
Secure Communities (t-3)	0.070		
	(0.050)		
Secure Communities (t-2)		0.039	
		(0.031)	
Secure Communities (t-1)			-0.092**
			(0.042)
Constant	1.937	1.881	2.678*
	(1.603)	(1.689)	(1.576)
Mean Outcome	0.619	0.619	0.619
Number of Observations	497144	497144	497144
Clusters	201	201	201
Panel B: Ln(Income per Hour) among Wage Workers			
Secure Communities (t-3)	-1.063***		
	(0.195)		
Secure Communities (t-2)		-0.926***	
		(0.126)	
Secure Communities (t-1)			-0.640***
			(0.197)
Constant	-12.603	-10.453	-9.931
	(8.724)	(8.442)	(8.527)
Mean Untransformed Outcome	1.544	1.544	1.544
Number of Observations	176155	176155	176155
Clusters	201	201	201
Municipal & Year FE	X	X	X
Municipality Time Trends	X	X	X
Weights	X	X	X
287(g)	X	X	X
Unemployment US	X	X	X
Individual controls	X	X	X

Notes: Individual-level data from El Salvador's Multiple Purpose Household Survey (EHPM) for people 18–75 years old from 2009–2018. The dependent variables are as follows: in panel A, a dummy indicating whether the individual is employed (i.e. has a job), and in panel B, the natural logarithm of income per hour of wage workers. In panel B, the mean values before transformation are reported. The variable of interest is a shift-share instrument between 0 and 1 that accounts for exposure to Secure Community policies in the previous year. All regressions are estimated via OLS. Fixed effects, municipality time trends, municipality controls, and analytical weights are the same as in Table 1. Individual controls are age, gender, and years of education. Standard errors are clustered by municipality and year. *p<0.1; **p<0.05; ***p<0.01

Table A7: Change in the Average Value for Hourly Income of Wage Workers – Different Scenarios

Hypothetical number of returnees, as % of the working-age population	Returnee wages fixed at zero	Returnee wages fixed at 1st decile (10%)	Returnee wages fixed at 1st quartile (25%)
1%	-2.7%	-2.3%	-2.0%
5%	-12.3%	-10.5%	-9.0%
10%	-22.0%	-18.6%	-16.1%

Notes: We added hypothetical observations to the full sample of wage receivers over all years to assess how much an additional workforce with below-average incomes (at zero, at the 10% and 25% of the wage distribution) would change average wages. The baseline mean log hourly wage is 0.367 (or 1.44 USD in levels). Table values show the percentage change in average wages after adding hypothetical returnees.

Table A8: Effect of Secure Communities on Ln(Income per Hour) – By Age and Gender

	(1) 30 years old or less	(2) Between 30 and 50 years old	(3) More than 50 years old
Panel A: Wage workers			
Secure Communities (t-1)	-0.674***	-0.495***	-1.049***
	(0.247)	(0.161)	(0.367)
Constant	-3.556	-15.792*	-5.083
	(11.584)	(8.941)	(14.141)
Mean Untransformed Outcome	1.350	1.692	1.695
Number of Observations	76406	77769	21980
Clusters	201	201	201
Panel B: Wage workers - Men			
Secure Communities (t-1)	-1.033***	-0.777**	-1.054**
	(0.376)	(0.309)	(0.449)
Constant	2.995	-18.285	-8.764
	(14.395)	(11.883)	(16.309)
Mean Untransformed Outcome	1.339	1.635	1.594
Number of Observations	50334	48605	15978
Clusters	201	201	201
Panel C: Wage workers - Women			
Secure Communities (t-1)	0.130	0.059	-0.994*
	(0.140)	(0.250)	(0.508)
Constant	-15.809	-9.488	5.864
	(10.825)	(10.236)	(16.886)
Mean Untransformed Outcome	1.371	1.788	1.968
Number of Observations	26072	29162	5986
Clusters	201	199	172
Municipal & Year FE	X	X	X
Municipality Time Trends	X	X	X
Weights	X	X	X
287(g)	X	X	X
Unemployment US	X	X	X
Individual controls	X	X	X

Notes: Individual-level data from El Salvador's Multiple Purpose Household Survey (EHPM) for people 18–75 years old from 2009–2018. The dependent variable is the natural logarithm of the income per hour. The mean values before transformation are reported. Results are presented by employment type and gender: wage workers (Panel A for all, Panel B for men, Panel C for women) and self-employed workers (Panel D for all, Panel E for men, Panel F for women). Within each panel, columns report results by age group: 18-30 years old (column 1), 31-49 years old (column 2), and 50 years old and older (column 3). The variable of interest is a shift-share instrument between 0 and 1 that accounts for exposure to Secure Community policies in the previous year. All regressions are estimated via OLS. Fixed effects, municipality time trends, municipality controls, and analytical weights are the same as in Table 1. Individual controls are age, gender, and years of education. Standard errors are clustered by municipality and year. *p<0.1; **p<0.05; ***p<0.01

Table A9: Effect of Secure Communities on Ln (Income per Hour) – By Access to Social Security, a Formal Contract, and Gender

	(1) Social security	(2) No social security	(3) Contract	(4) No contract
Panel A: Wage workers				
Secure Communities (t-1)	-0.030	-0.494**	0.188*	-0.646***
	(0.083)	(0.228)	(0.107)	(0.218)
Constant	-9.059**	-8.820	-6.119	-8.812
	(4.112)	(10.871)	(5.245)	(9.932)
Mean Untransformed Outcome	2.066	1.135	2.249	1.200
Number of Observations	77455	98699	57811	118343
Clusters	200	201	200	201
Panel B: Wage workers - Men				
Secure Communities (t-1)	-0.160	-0.707**	0.041	-0.874**
	(0.180)	(0.344)	(0.228)	(0.347)
Constant	-2.194	-13.777	-2.407	-11.702
	(5.144)	(11.822)	(5.486)	(11.040)
Mean Untransformed Outcome	1.977	1.154	2.133	1.216
Number of Observations	48250	66665	35556	79357
Clusters	199	201	197	201
Panel C: Wage workers - Women				
Secure Communities (t-1)	0.141	0.127	0.325**	0.033
	(0.166)	(0.245)	(0.152)	(0.208)
Constant	-18.595***	-0.721	-10.926*	-3.382
	(5.261)	(12.097)	(6.250)	(12.420)
Mean Untransformed Outcome	2.212	1.095	2.435	1.167
Number of Observations	29200	32034	22250	38986
Clusters	195	201	197	201
Municipal & Year FE	X	X	X	X
Municipality Time Trends	X	X	X	X
Weights	X	X	X	X
287(g)	X	X	X	X
Unemployment US	X	X	X	X
Indivudual controls	X	X	X	X

Notes: Individual-level data from El Salvador's Multiple Purpose Household Survey (EHPM) for people 18–75 years old from 2009–2018. The dependent variable is the natural logarithm of the income per hour. The mean values before transformation are reported. Panel A reports the results for all wage workers; Panel B reports the results for men who are wage workers; Panel C reports the results for women who are wage workers. Within each panel, results are disaggregated by social security access (columns 1-2) and formal contract status (columns 3-4), where odd columns represent workers with social security/contracts and even columns represent workers without. The variable of interest is a shift-share instrument between 0 and 1 that accounts for exposure to Secure Community policies in the previous year. All regressions are estimated via OLS. Fixed effects, municipality time trends, municipality controls, and analytical weights are the same as in Table 1. Individual controls are age, gender, and years of education. Standard errors are clustered by municipality and year. *p<0.1; **p<0.05; ***p<0.01

Table A10: Effect of Secure Communities on Ln (Income per Hour) – By Education and Gender

	(1) No Formal Education	(2) Uncompleted High School	(3) Completed High School	(4) At Least College
Panel A: Wage workers				
Secure Communities (t-1)	-0.537	-1.036***	0.104	1.125***
	(0.449)	(0.282)	(0.213)	(0.282)
Constant	-10.229	-4.281	-6.935	-16.725
	(26.011)	(11.286)	(9.263)	(11.844)
Mean Untransformed Outcome	1.044	1.230	1.903	3.481
Number of Observations	14983	94272	58591	8297
Clusters	201	201	201	178
Panel B: Wage workers - Men				
Secure Communities (t-1)	-0.454	-1.324***	0.018	1.057*
	(0.549)	(0.416)	(0.226)	(0.555)
Constant	-26.399	-7.743	8.543	-22.301
	(23.404)	(13.795)	(11.051)	(20.752)
Mean Untransformed Outcome	1.071	1.264	1.869	3.504
Number of Observations	10666	66193	34340	3700
Clusters	198	201	201	159
Panel C: Wage workers - Women				
Secure Communities (t-1)	-0.724	-0.136	0.136	1.204***
	(0.719)	(0.222)	(0.261)	(0.429)
Constant	42.882	1.521	-26.140**	-10.080
	(43.108)	(15.706)	(10.943)	(14.597)
Mean Untransformed Outcome	0.977	1.149	1.951	3.465
Number of Observations	4307	28078	24249	4573
Clusters	187	200	199	159
Municipal & Year FE	X	X	X	X
Municipality Time Trends	X	X	X	X
Weights	X	X	X	X
287(g)	X	X	X	X
Unemployment US	X	X	X	X
Individual controls	X	X	X	X

Notes: Individual-level data from El Salvador's Multiple Purpose Household Survey (EHPM) for people 18–75 years old from 2009–2018. The dependent variable is the natural logarithm of the income per hour. The mean values before transformation are reported. Panel A reports the results for all wage workers; Panel B reports the results for men who are wage workers; Panel C reports the results for women who are wage workers. Within each panel, column 1 presents the results for workers without formal education, column 2 for workers who did not complete high school, column 3 for workers who have completed high school, and column 4 for workers with at least a college degree. The variable of interest is a shift-share instrument between 0 and 1 that accounts for exposure to Secure Community policies in the previous year. All regressions are estimated via OLS. Fixed effects, municipality time trends, municipality controls, and analytical weights are the same as in Table 1. Individual controls are age, gender, and years of education. Standard errors are clustered by municipality and year. *p<0.1; **p<0.05; ***p<0.01

Table A11: Effect of Secure Communities on Ln(Income per Hour) of Wage Workers – By Economic Sector

	(1) Agriculture	(2) Manufacture	(3) Construction	(4) Finances	(5) Services	(6) Other
Panel A: Wage workers						
Secure Communities (t-1)	-1.941**	0.021	0.245	-0.096	0.012	-0.063
	(0.755)	(0.328)	(0.296)	(0.632)	(0.098)	(0.128)
Constant	-15.613	34.249*	23.916	-2.042	-26.871***	-6.210
	(21.160)	(20.249)	(17.808)	(13.847)	(9.079)	(6.630)
Mean Untransformed Outcome	0.955	1.420	1.357	2.153	1.803	1.726
Number of Observations	30526	29211	16104	2402	69234	28635
Clusters	199	196	201	112	201	201
Panel B: Wage workers - Men						
Secure Communities (t-1)	-1.788**	0.110	0.270	-0.458	-0.256	-0.359
	(0.762)	(0.294)	(0.313)	(1.638)	(0.177)	(0.290)
Constant	-16.256	15.118	21.900	-12.001	-19.076*	6.024
	(20.079)	(19.514)	(17.992)	(25.723)	(10.331)	(10.947)
Mean Untransformed Outcome	0.962	1.502	1.350	2.206	1.692	2.213
Number of Observations	27743	17433	15847	1190	40637	12034
Clusters	199	189	201	93	200	197
Panel C: Wage workers - Women						
Secure Communities (t-1)	-1.151	-0.429	-3.994	0.158	0.188	0.105
	(0.766)	(0.591)	(2.956)	(0.807)	(0.254)	(0.198)
Constant	8.976	71.532**	121.130	-4.975	-34.778***	-16.924**
	(32.314)	(28.160)	(138.502)	(21.369)	(9.496)	(8.101)
Mean Untransformed Outcome	0.890	1.299	1.785	2.107	1.962	1.374
Number of Observations	2750	11762	228	1190	28596	16600
Clusters	123	178	56	85	200	201
Municipal & Year FE	X	X	X	X	X	X
Municipality Time Trends	X	X	X	X	X	X
Weights	X	X	X	X	X	X
287(g)	X	X	X	X	X	X
Unemployment US	X	X	X	X	X	X
Individual controls	X	X	X	X	X	X

Notes: Individual-level data from El Salvador's Multiple Purpose Household Survey (EHPM) for people 18–75 years old from 2009–2018. The dependent variable is the natural logarithm of the income per hour. The mean values before transformation are reported. Panel A reports the results for all wage workers; Panel B reports the results for men who are wage workers; Panel C reports the results for women who are wage workers. Columns show the results categorized by economic sectors of employment: agriculture (column 1), manufacturing (column 2), construction (column 3), finance (column 4), services (column 5), and other sectors (column 6). The variable of interest is a shift-share instrument between 0 and 1 that accounts for exposure to Secure Community policies in the previous year. All regressions are estimated via OLS. Fixed effects, municipality time trends, municipality controls, and analytical weights are the same as in Table 1. Individual controls are age, gender, and years of education. Standard errors are clustered by municipality and year. *p<0.1; **p<0.05; ***p<0.01

Table A12: Effect of Secure Communities on Ln(Income per Hour) of Wage Workers – By Urban/Rural

	(1) Urban	(2) Rural
Panel A: Wage workers		
Secure Communities (t-1)	-0.233	-0.802**
	(0.160)	(0.310)
Constant	-17.012*	-0.396
	(9.033)	(12.150)
Mean Untransformed Outcome	1.725	1.277
Number of Observations	104960	71195
Clusters	176	173
Panel B: Wage workers - Men		
Secure Communities (t-1)	-0.645***	-0.900**
	(0.241)	(0.407)
Constant	-12.822	-3.295
	(12.860)	(15.048)
Mean Untransformed Outcome	1.690	1.262
Number of Observations	63791	51126
Clusters	176	173
Panel C: Wage workers - Women		
Secure Communities (t-1)	0.289	-0.389*
	(0.256)	(0.208)
Constant	-21.175**	-0.163
	(10.015)	(8.818)
Mean Untransformed Outcome	1.781	1.315
Number of Observations	41169	20069
Clusters	176	173
Municipal & Year FE	X	X
Municipality Time Trends	X	X
Weights	X	X
287(g)	X	X
Unemployment US	X	X
Individual controls	X	X

Notes: Individual-level data from El Salvador's Multiple Purpose Household Survey (EHPM) for people 18–75 years old from 2009–2018. The dependent variable is the natural logarithm of the income per hour. The mean values before transformation are reported. Panel A reports the results for all wage workers; Panel B reports the results for men who are wage workers; Panel C reports the results for women who are wage workers. Column 1 reports results for household in urban areas, while column 2 reports results for household in rural areas, as identified in the EHPM. All regressions are estimated via OLS. Fixed effects, municipality time trends, municipality controls, and analytical weights are the same as in Table 1. Individual controls are age, gender, and years of education. Standard errors are clustered by municipality and year. *p<0.1; **p<0.05; ***p<0.01

Table A13: Effect of Secure Communities on Ln (Income per Hour) for Wage Workers – Heterogeneity by Municipal Characteristics

	(1) Poverty (2005)	(2) Employed in Agriculture (2005)	(3) Youth Unemployment (2005)	(4) Homicides per Capita (2008)	(5) Gang presence (1999-2016)
Panel A: Below median					
Secure Communities (t-1)	-0.949***	-0.628**	-0.554**	-0.290	-0.521***
	(0.140)	(0.255)	(0.240)	(0.377)	(0.198)
Constant	-0.074	-1.953	-14.173	10.978	2.845
	(9.494)	(9.513)	(10.826)	(12.399)	(12.754)
Mean Untransformed Outcome	1.586	1.591	1.531	1.509	1.498
Number of Observations	141957	136858	112333	38802	63553
Clusters	116	109	108	87	101
Panel B: Above median					
Secure Communities (t-1)	0.268	-0.344	-0.868***	-0.820***	-0.516**
	(0.251)	(0.312)	(0.305)	(0.145)	(0.236)
Constant	-8.250	-56.052***	-11.100	-10.228	-16.729
	(10.539)	(15.780)	(9.668)	(9.019)	(13.132)
Mean Untransformed Outcome	1.371	1.382	1.568	1.554	1.570
Number of Observations	34198	39297	63406	137353	112602
Clusters	85	92	91	114	101
Municipal & Year FE	X	X	X	X	X
Municipality Time Trends	X	X	X	X	X
Weights	X	X	X	X	X
287(g)	X	X	X	X	X
Unemployment US	X	X	X	X	X
Individual controls	X	X	X	X	X

Notes: Individual-level data from El Salvador's Multiple Purpose Household Survey (EHPM) for people 18–75 years old from 2009–2018. The dependent variable is the natural logarithm of the income per hour among wage workers. The mean values before transformation are reported. The sample is divided based on municipal characteristics using median splits. Panel A reports results for municipalities below the median value; Panel B reports results for municipalities above the median value. The municipal characteristics employed to divide the sample in columns 1 to 3 are from the 2005 Poverty Map of El Salvador Briones et al. (2005), being poverty rate (column 1), agricultural employment share (column 2), and youth unemployment rate (column 3). In column 4, the municipal characteristic employed is the number of homicides per capita for the year 2008. In column 5, the municipal characteristic is the presence of gangs, measured as the drop in homicides during a truce between rivaling gangs in 2012 compared to the longer-term average as an indicator for the presence of gangs, drawn from (Ambrosius, 2021). The explanatory variable of interest is a shift share instrument between 0 and 1 that accounts for the exposure to Secure Community policies in the previous year. All regressions are estimated via OLS. Fixed effects, municipality time trends, municipality controls, and analytical weights are the same as in Table 1. Individual controls are age, gender, and years of education. Standard errors are clustered by municipality and year. *p<0.1; **p<0.05; ***p<0.01

Table A14: Effect of Secure Communities on Household Remittances and Food Expenditure – Heterogeneity by Employment of the Head of the Household

	(1) Unemployed	(2) Employer	(3) Self-employed	(4) Wage worker
Panel A: Likelihood of receiving remittances (=1)				
Secure Communities (t-1)	-0.142	0.498**	-0.150**	-0.162**
	(0.097)	(0.239)	(0.068)	(0.079)
Constant	-1.092	8.138	6.296*	2.580
	(3.374)	(7.554)	(3.409)	(4.111)
Mean Outcome	0.404	0.310	0.248	0.125
Number of Observations	57949	9377	58828	85089
Clusters	201	199	201	201
Panel B: Ln(Remittances)				
Secure Communities (t-1)	-0.016	-0.751	0.087	-0.189
	(0.184)	(0.881)	(0.413)	(0.526)
Constant	-12.862	-36.501**	-27.329	-24.375
	(10.327)	(16.066)	(20.434)	(21.954)
Mean Untransformed Outcome	2548.089	2269.412	1883.570	1573.263
Number of Observations	22848	2719	13874	9757
Clusters	200	168	200	199
Panel C: Ln(Food Expenditure per Capita)				
Secure Communities (t-1)	-0.146	-0.128	-0.210*	-0.128
	(0.193)	(0.199)	(0.108)	(0.170)
Constant	-0.489	25.122***	11.828**	13.920**
	(5.778)	(8.712)	(5.225)	(6.176)
Mean Untransformed Outcome	45.196	51.363	41.218	41.758
Number of Observations	57949	9377	58828	85089
Clusters	201	199	201	201
Municipal & Year FE	X	X	X	X
Municipality Time Trends	X	X	X	X
Weights	X	X	X	X
287(g)	X	X	X	X
Unemployment US	X	X	X	X

Notes: Household-level data from El Salvador's Multiple Purpose Household Survey (EHPM) from 2009–2018. In panel A, the dependent variable is a dummy indicating whether the household received remittances; in panel B, it is the natural logarithm of the annual amount of remittances received; in panel C, it is the natural logarithm of the annual food expenditure per capita. Estimates are presented by household head employment status: unemployed (column 1), employer (column 2), self-employed (column 3), and wage worker (column 4). The variable of interest is a shift-share instrument between 0 and 1 that accounts for exposure to Secure Community policies in the previous year. Fixed effects and controls are the same as in Table 1. Standard errors are clustered by municipality and year. *p<0.1; **p<0.05; ***p<0.01

Table A15: Effect of Secure Communities on Main Outcomes – Robustness to Different Samples and Specifications

	(1) Balanced Sample	(2) All	(3) Excluding San Salvador	(4) Self-represented Municipalities	(5) Excluding early adopters	(6) Control: Crime	(7) Control: Temperature	(8) Weight: Migrants 2007
Panel A: Migration at the Household level								
Secure Communities (t-1)	-0.119*	-0.119*	-0.164***	-0.178*	-0.132*	-0.118*	-0.123*	-0.136***
	(0.069)	(0.069)	(0.058)	(0.093)	(0.067)	(0.068)	(0.070)	(0.052)
Constant	2.468	2.456	2.701	4.063	3.187	1.095	1.204	2.843
	(4.008)	(4.003)	(4.054)	(4.273)	(4.019)	(3.668)	(3.743)	(3.707)
Mean Outcome	0.163	0.163	0.163	0.136	0.160	0.173	0.173	0.163
Number of Observations	211245	214408	209499	135065	202172	191529	191529	211093
Clusters	201	234	200	49	191	201	201	200
Panel B: Probability of employment								
Secure Communities (t-1)	-0.092**	-0.093**	-0.078*	-0.127***	-0.102**	-0.087**	-0.092**	-0.112***
	(0.042)	(0.042)	(0.041)	(0.045)	(0.042)	(0.041)	(0.042)	(0.037)
Constant	2.678*	2.685*	2.656	3.322*	2.983*	3.413	3.594	3.375**
	(1.576)	(1.574)	(1.610)	(1.809)	(1.627)	(2.236)	(2.352)	(1.483)
Mean Outcome	0.619	0.619	0.619	0.633	0.621	0.620	0.620	0.619
Number of Observations	497144	504471	493028	320445	476471	452168	452168	496781
Clusters	201	234	200	49	191	201	201	200
Panel C: Salary per hour among wage workers								
Secure Communities (t-1)	-0.640***	-0.637***	-0.592***	-0.538**	-0.506***	-0.673***	-0.609***	-0.650***
	(0.197)	(0.197)	(0.189)	(0.228)	(0.186)	(0.194)	(0.201)	(0.199)
Constant	-9.931	-9.849	-10.284	-5.136	-6.449	-6.342	-8.449	-12.582
	(8.527)	(8.518)	(8.418)	(8.309)	(7.469)	(11.904)	(11.546)	(8.980)
Mean Outcome	1.544	1.542	1.540	1.573	1.540	1.538	1.538	1.544
Number of Observations	176155	178309	174519	121098	169730	159801	159801	176050
Clusters	201	234	200	49	191	201	201	200
Municipal & Year FE	X	X	X	X	X	X	X	X
Municipality Time Trends	X	X	X	X	X	X	X	X
Weights	X	X	X	X	X	X	X	X
287(g)	X	X	X	X	X	X	X	X
Unemployment US	X	X	X	X	X	X	X	X
Individual controls*	X	X	X	X	X	X	X	X

Notes: Household and individual-level data from El Salvador's Multiple Purpose Household Survey (EHPM) from 2009–2018. The dependent variables are as follows: in panel A, a dummy indicating whether a household member migrated; in panel B, a dummy indicating whether the individual is employed (i.e. has a job), and in panel C, the natural logarithm of income per hour of wage workers. In panel C, the mean values before transformation are reported. In column 1, we present the results for all observations. From column 2 onward, we focus on a balanced sample of municipalities across all years. Column 3 excludes the municipality of San Salvador. Column 4 includes only 50 self-represented municipalities, which are defined as provincial capitals and other municipalities that are particularly relevant and distinct due to their sociodemographic characteristics. Column 5 excludes the top 5% of municipalities with the highest exposure to Secure Communities in 2008 and 2009. Column 6 adds homicides per capita from the previous year as a control variable. Column 7 includes a variable for high temperature, measured as the number of weeks in the year during in which the average temperature was 2 standard deviations higher than the historical average, drawn from Ibáñez et al. (2022). Column 8 employs the number of migrants per capita at the municipal level from the 2007 National Census General Directorate of Statistics and Census (DIGESTYC) to compute analytical weights, rather than relying on consular data before 2008. The variable of interest is a shift-share instrument between 0 and 1 that accounts for exposure to Secure Community policies in the previous year. Fixed effects, municipality time trends, municipality controls, and analytical weights are the same as in Table 1. Individual controls are age, gender, and years of education. Standard errors are clustered by municipality and year. *p<0.1; **p<0.05; ***p<0.01