Immigration Enforcement and Origin Country Labor Markets

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This version: February 14, 2025
First version: June 15, 2020

Abstract

This paper studies how increased immigration enforcement in the destination country impacts labor markets in the origin country. Using variation in Mexican migrant networks and Secure Communities (SC), a policy which expanded local immigration enforcement, I show that the deportation of Mexican migrants from the U.S. increases return migration to Mexico, which increases labor market competition and decreases monthly earnings for workers in Mexico. I show that the relatively large effect on earnings is due, in part, to return migrants being more productive than locals and competing for similar jobs.

Keywords: Return migration, deportations, labor markets
JEL Codes: F22, O15, J40, R23

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I am grateful to my advisors Sam Bazzi, Daniele Paserman, and David Lagakos for their support and guidance throughout this project. I would like to thank Dilip Mookherjee, Kevin Lang, Hanna Schwank, Cem Özgüzel, Brian Cadena, David McKenzie, Steven Stillman, Eduardo Medina-Cortina, and Daniel Osuna-Gomez for their helpful comments and suggestions. I benefited from discussions with seminar participants at California State University, Fullerton, Cornell University, Syracuse University, and the University of California Davis Global Migration Center and conference participants at the Western Economic Association International Virtual (WEAI) Conference, the IZA Annual Migration Meeting, the Development and Political Economics SF Bay Area PhD Student Conference (DEVPEC), the Young Economists Symposium (YES), the RIDGE Virtual Workshop on Labor, the 15th Migration and Development Conference hosted by NOVAFRICA, and the Southern Economic Association (SEA) Conference. I am grateful to Eduardo Medina-Cortina for sharing bilateral migration data and to Sue Long at the Transactional Records Access Clearinghouse (TRAC) for assistance with immigration enforcement data, which I obtained from Syracuse University as a TRAC Fellow. Any errors are my own.

1 Introduction

International migration has surged in the recent wave of globalization, driven by persistent wage disparities across countries that have fueled labor mobility. This phenomenon has played a pivotal role in bringing economies closer together, fostering integration, and benefiting both sending and receiving countries. However, the rise in the number of immigrants in rich host countries has led to their voters and politicians advocating for stricter immigration enforcement policies. This has introduced new challenges and risks for developing countries where migration offers a path to development by decreasing unemployment, providing migrants with higher-paying job opportunities abroad, and increasing income through remittances.

In this paper, I explore how stricter immigration enforcement policies impact labor markets at origin in the world's largest migration corridor, between Mexico and the United States. To do this, I exploit rich sub-national bilateral migration data and the variation across the U.S. in the application of Secure Communities (SC), a policy that led to the largest expansion of local immigration enforcement in U.S. history (Kalhan 2013). I provide evidence that the forced removal and return of migrants living in the destination country can negatively impact workers in the origin country. Specifically, I show that deportations through SC increase return migration from the U.S. to Mexico, which increases labor market competition and decreases earnings for workers in Mexico. The negative effects are relatively large in this context because these migrants can more seamlessly reinsert themselves into the same labor markets as locals, competing for the same types of jobs.

To identify the effects of deportations, I use data on SC deportations and on Mexican migrant networks and construct a shift-share instrument which predicts the number of deportees for over 2,300 Mexican municipalities. The instrument combines variation in migrants' destinations in the U.S. and the variation across U.S. counties in the number of interior SC deportations.[1] I divide the predicted number of deportees by the size of the municipality's working-age population, providing an intuitive measure, the predicted deportee population share. I then use the cross-sectional variation across Mexican municipalities in the predicted deportee population share to study how exposure to SC deportations impacted labor market outcomes between 2010 and 2015.

The identifying assumption is that the predicted deportee population share is uncorrelated with changes in unobserved economic conditions, conditional on Mexican state fixed effects and controls for initial municipality characteristics. This measure is based on migration decisions made before the policy was implemented and the number of deportations at the U.S. county level, which is plausibly exogenous to conditions in Mexican municipalities. I also show that exposure to deportations is uncorrelated with trends in average earnings, wages, employment, and homicide

[1]Border enforcement is carried out by the U.S. Border Patrol and is primarily concerned with detecting and deterring illegal entry and preventing the trafficking of people and contraband. Interior enforcement is carried out by Immigration and Customs Enforcement (ICE) and generally consists of immigration enforcement outside 100 miles of the U.S. border. ICE is tasked with the apprehension and removal of deportable noncitizens that have already successfully entered the U.S.

rates across municipalities before SC was introduced. Additionally, U.S. counties with high levels of SC deportation intensity were similar to those with lower levels across several key characteristics, providing further evidence that these shocks are exogenous.

To reinforce a causal interpretation of my findings, I also present results which identify the effects of exposure to SC using only the variation in the timing of the policy's implementation across counties. Several papers highlight the quasi-random nature of SC's rollout and use it to identify its effects on outcomes in the U.S. such as the take-up of safety net programs and U-visas (Amuedo-Dorantes and Arenas-Arroyo 2019; Watson 2014; Alsan and Yang 2022), crime (Miles and Cox 2014; Hines and Peri 2019), and U.S. labor markets (East et al. 2023). Results from this approach are qualitatively similar to those from the baseline approach, which uses the deportation intensity, further confirming the consistency and robustness of my identification strategy.

Using Mexican microdata, I show that SC deportations from 2010-2015 increased overall labor supply, primarily through return migration as opposed to decreased emigration, and decreased monthly earnings for local workers in Mexico during the same period. I estimate that one additional predicted deportee for every 100 working-age individuals over a five-year period decreases locals' monthly earnings by between 0.7 and 1.0 percent. The negative effects on earnings are largest for locals who return migrants would presumably be substitutable for, namely those most similar to the return migrants in terms of their demographics and their occupations and employment types. Together, these results point to return migration as a key channel through which deportations decrease local workers' earnings.

One possible explanation is that deportations decrease remittance income to Mexican municipalities, which, in turn, reduces labor demand and contributes to the decline in wages and earnings. This could occur because deportations decrease the stock of migrants living in the U.S. thereby limiting the number of potential remittance senders. For the migrants who remain abroad, SC deportations could negatively impact their employment outcomes, as shown in East et al. (2023), which could limit how much money they send back to Mexico.

However, I show that decreased remittance income does not appear to be a key driver of the the negative effects of deportations on earnings. Instead, both existing and new emigrants appear to respond to worsening economic conditions in Mexico by sending more remittances, as I find that deportations increase the share of households receiving remittances. I also show that exposure to SC deportations from 2010-2015 is positively associated with remittance income levels in 2015, providing suggestive evidence that remittances respond similarly on the intensive margin. These results seem to rule out the possibility that the drop in workers' earnings is driven by decreased remittance income.

After establishing return migration as the key channel through which SC deportations decrease workers' earnings in Mexico, I estimate the implied elasticity of earnings with respect to return migration. To do this, I use exposure to SC enforcement and implementation as instruments for changes in labor supply. I find that a one percent migrant-induced increase in the number of

workers decreases monthly earnings by between 1.0 and 1.3 percent. These estimates are similar to others found in the literature on the labor market effects of migration, particularly those found in similar settings (e.g., Morales 2018; Lebow 2022). However, they contrast with those found in East et al. (2023), who study the labor market effects of the same policy and find that the decline in foreign-born workers in the U.S. led to lower wages for native workers. This raises an important question: Why does the outflow of immigrant workers from the U.S. as a result of this policy – the majority of whom are Mexican – lower wages for native workers in the U.S., while their resulting inflow into Mexico also reduce wages for local workers in Mexico?

I do not find evidence that the difference between the two settings can be explained by Mexico's level of economic development compared to the U.S. To test this mechanism, I explore the heterogeneity by municipality-level measures of development and find that the impact of deportations on earnings is fairly uniform across rich and poor municipalities. In fact, there is some evidence suggesting that deportations may even increase earnings in the poorest municipalities. This finding is consistent with other research that highlights the potential positive effects of Mexican return migration. For example, Bucheli et al. (2019) show that return migrants may reduce violence, while Bucheli and Fontenla (2022) find that return migration leads to higher levels of social development, including education, health care access, and living conditions.

The relatively large negative effect on earnings is likely driven by return migrants' relatively high degree of substitutability with local workers given their similarities, especially compared to other international migration contexts which often explore the effects of foreigners on natives. To highlight this mechanism, I show that deportations have larger negative effects on earnings in municipalities where there is less occupational segregation and more demographic similarity between return migrants and locals. In these municipalities, return migrants and locals tend to work in the same occupations, leading to more labor market competition from deportation-induced migrant inflows. I also show that return migrants are more productive, as measured by their earnings, than comparable locals from the same municipality. Together, these mechanisms allow return migrants to more effectively crowd out local workers, leading to large drops in locals' earnings. Collectively, these findings suggest a potential lose-lose scenario in terms of the labor market effects of the SC policy in the short term, where the forced return of Mexican migrants disrupts the complementarities between Mexican migrants and U.S.-born workers while increasing labor market competition between non-migrants and return migrants in Mexico.

My findings shed light on the potential risks developing migrant-sending countries face as they become more closely linked to rich country destinations. Recent work shows how income and employment shocks in destinations during the Great Recession impacted various migrant-sending countries, including Vietnam (Gröger 2021), Colombia (Cuadros-Menaca and Gaduh 2020), and Mexico (Caballero et al. 2021; Conover et al. 2021).[2] Several papers use these and other shocks to

[2]Yang (2008) and Khanna et al. (2022) highlight the potential positive effects of economic shocks at destination, using the 1997 Asian Financial Crisis as a natural experiment. The crisis caused large fluctuations in exchange

study the effects of the recent surge in return migration from the U.S. to Mexico on other outcomes in Mexico such as economic development and violence (Bucheli and Fontenla 2022; Bucheli et al. 2019). This paper provides evidence of how immigration enforcement shocks affect labor markets in origin countries by studying a policy that forcibly returned hundreds of thousands of immigrants living in the U.S. Understanding the consequences of these types of policies for migrant-sending countries is especially important given the rise in anti-immigrant attitudes among politicians and constituents in the U.S. and throughout the world.[3]

The findings in this paper are especially relevant given the ongoing debates surrounding U.S. interior immigration enforcement, as they offer insights into the consequences of such policies on migrant-sending countries like Mexico.[4] Prior work studying the effects of deportations on migrant-sending countries has mostly focused on the spread of violence and gangs in El Salvador (Sviatschi 2019; Ambrosius 2018; Ambrosius and Leblang 2019; Kalsi 2018).[5] In a recent paper, Bandiera et al. (2023) leverage pre-existing exposure to deportations, measured by past repatriation flows, along with annual national variation in repatriation flows to El Salvador, and finds that these inflows decrease formal sector wages due to increased competition in the informal sector. My paper contributes to this literature by studying the labor market impacts of deportations in Mexico – the largest source of immigrants in U.S. history (Passel 2012) and the origin country of the majority of immigrant apprehensions and deportations in the U.S. (US Immigration and Customs Enforcement 2019). Moreover, it focuses on the effects of a particular large-scale policy that led to the most significant expansion of interior immigration enforcement in U.S. history. This policy provides exogenous variation in exposure to immigration enforcement, allowing me to provide credible estimates of the causal effects of immigration enforcement on labor market outcomes in the origin country and identify key mechanisms.

By focusing on Mexico, this paper adds to the growing literature on the labor market effects of international migration in developing countries. Understanding these effects is important as nearly half of the world's international migrants live in developing countries (United Nations 2020). Yet, much of the existing literature on the labor market effects of international migration has focused on developed countries and has generally found small effects of immigration on natives' labor market outcomes.[6] Some exceptions include recent work on Syrian refugees in Turkey (Tumen

rates across destination countries, leading to variation in migrant income. The authors use this variation to study the impacts of migrant income on remittances, education, and broader economic development at origin in the Philippines.

[3]See Card et al. (2022) and Dennison and Geddes (2019) for evidence of this phenomenon in the U.S. and Europe respectively.

[4]Several papers study how interior enforcement affects outcomes in the U.S. such as the take-up of safety net programs and U-visas (Amuedo-Dorantes and Arenas-Arroyo 2019; Watson 2014; Alsan and Yang 2022), crime (Miles and Cox 2014; Hines and Peri 2019), and U.S. labor markets (East et al. 2023).

[5]One exception is Rozo et al. (2021) who study the effects of repatriations on crime and victimization in repatriation areas in Mexico, most of which are along the Mexico-U.S. border. They identify these effects using annual variation in the total number of deportees.

[6]In Dustmann et al. (2016), the authors highlight 26 papers on the labor market effects of immigration. 25 out of

2016; Esen and Oğuş Binatlı 2017) and Venezuelan refugees in Colombia (Caruso et al. 2021; Delgado-Prieto 2020; Santamaria 2020; Lebow 2023).[7] Several of these studies have found larger negative effects of immigration flows on natives' labor market outcomes relative to those found in rich country destinations. Aracı et al. (2022) provide evidence in the Turkish context which suggests that certain factors may protect local workers from the negative labor market effects of forced migration in developed regions and countries. I also find larger impacts of forced migration on non-migrants in the Mexican context relative to the rich country destinations that have been the focus of prior work. However, unlike Aracı et al. (2022), my results do not suggest that underdevelopment is the reason for the larger negative effects I find in this context.

This paper also contributes to the international migration literature by studying return migrants, whose labor market effects are less studied and may differ from those of foreigners. Prior research has shown instances of return migrants crowding out labor market opportunities for locals. Examples include the inflow of French-Algerians returning to France after Algerian independence (Hunt 1992; Edo 2017) and the arrival of *retornados* to Portugal from Angola and Mozambique (Carrington and De Lima 1996). Building on this literature, this paper shows that forced return migration due to immigration policies in destination countries can similarly harm labor market outcomes for locals in origin countries.

This paper proceeds as follows. Section 2 provides background on Mexican migration to the U.S., U.S. immigration enforcement, and Secure Communities, the particular immigration policy I focus on in this paper. Section 3 describes my empirical approach and data. Section 4 presents the main results and discusses their robustness. Section 5 explores the key channel driving the labor market effects. Section 6 discusses the contextual factors which shape the effect of immigration enforcement on earnings at origin. Section 7 concludes.

2 Background

2.1 Mexican Immigration and U.S. Immigration Enforcement

Mexican immigration to the U.S. was relatively slow for nearly a century after the present-day border between the U.S. and Mexico was officially established in 1848. Apart from a wave of migrants in the early 20th century[8], it was not until the 1960s/1970s when the flow of Mexican migrants accelerated dramatically and continued increasing the next few decades. This upswing is depicted in Appendix Figure A2 which shows that in 1970, only 2 percent of the Mexican-born

these 26 papers focus on developed countries per the United Nations classifications. In Jaeger et al. (2018), the authors provide a list of 70 papers that use some version of the shift-share design to study the impacts of migration flows. 65 out of 70 of these papers focus on developed countries.

[7] See Lebow 2022 for a review of the research on the labor market effects of Venezuelan migration to Colombia.

[8] This early wave was due to several factors including: violence in Mexico from Mexican Revolution (Escamilla-Guerrero et al. 2023) and the Cristero War (Young 2015), the expansion of the railway system, and restrictions on immigration from Europe in the 1920s which increased demand for Mexican laborers (Abramitzky et al. 2023).

population lived in the U.S. However, by 2000 this number had increased to close to 10 percent.

This surge in Mexican immigration was due to several reasons including economic crises in Mexico (the debt crisis in 1983 and the Mexican peso crisis in 1995) and population growth in Mexico[9]. As Mexican immigrants increasingly settled in locations across the U.S., they played a pivotal role in facilitating further migration for fellow compatriots (Massey and España 1987) and providing support to newcomers once they arrived (Munshi 2003; Orrenius and Zavodny 2005).

Like migrant networks in other contexts, comparably similar Mexican origin communities often have networks in different destinations in the U.S.[10] Using data from the Mexican Migration Project (MMP), Munshi (2003) shows that the education levels and occupational profiles of migrants hailing from the Mexican states of Jalisco, Guanajuato, San Luis Potosí, Michoacán, Zacatecas, Nayarit, and Colima bear significant similarities. However, he also higlights the substantial divergence among these Mexican states in terms of destination choices, offering illustrative examples which highlight this point.[11] Caballero et al. (2018) corroborate these patterns drawing on data from the Matrícula Consular de Alta Seguridad (MCAS) program and provide an example of starkly differing destination distributions between two municipalities in the same Mexican state.[12] These origin-destination patterns are consistent with historical accident playing a role in establishing migrant networks.

The 1965 Immigration and Nationality Act imposed caps on immigration from Mexico to the U.S. for the first time. The combination of these caps and the persistent demand for migrant labor and the influence of migrant networks gave rise to unprecedented levels of unauthorized immigration, a phenomenon not seen before at that time (Massey and Pren 2012). The issue of illegal immigration led to the passage of policies in the 1980s and 1990s which allocated greater budgetary and resource commitments to immigration enforcement. Among these policies were the Immigration Reform and Control Act (IRCA) in 1986 and the Illegal Immigration Reform and Immigrant Responsibility Act (IIRIRA) in 1996. IIRIRA, in particular, broadened the criteria for deportation, resulting in the detention and deportation of a considerably larger number of immigrants (Fragomen Jr 1997).

The September 11, 2001 terrorist attacks prompted the creation of the Department of Homeland Security (DHS) and a restructuring of the responsibilities previously held by the Immigration and

[9]Hanson and McIntosh (2010) estimate that 40 percent of emigration from 1977 to 1997 can be explained by population growth in Mexico.

[10]Carrington et al. (1996) document similar patterns for Southern Black migrant networks during the Great Migration.

[11]21 percent of migrants from Michoacán live in San Francisco, CA while only a small fraction of migrants from the other states locate there. 27 percent of migrants from Jalisco settle in San Diego, CA while only 1 percent of migrants from San Luis Potosí end up there. Instead, migrants from San Luis Potosí are much more likely to move to Houston, TX. 16 percent of migrants from San Luis Potosi locate in Houston while only 1 percent of migrants from Jaliso choose Houston as a destination.

[12]Migrants from Ciudad Hidalgo in the state of Michoacán mostly settle in Illinois while the majority of migrants from Tiquicheo, which is also in Michoacán and is only a three-hour drive from Ciudad Hidalgo, instead reside in Texas.

Naturalization Service (INS). This reorganization resulted in the creation of three distinct agencies: U.S. Citizenship and Immigration Services (USCIS), U.S. Immigration and Customs Enforcement (ICE), and U.S. Customs and Border Protection (CBP). Similar to the earlier impacts of IRCA and IIRIRA, this period also witnessed substantial increases in funding allocated to immigration enforcement, with the enforcement budget tripling between 2000 and 2006 (Bolter and Chishti 2021). Alongside the infusion of resources, ICE faced mounting pressure to enhance immigrant detentions, a challenge the agency met head-on.

Another significant development during this era was the collection and sharing of data on immigrants residing in the U.S., along with increased coordination across various government agencies. This shift resulted in a massive expansion of arrests in the interior as state and local law enforcement officers became increasingly tasked with carrying out immigration enforcement duties. Initially, this was done through programs like 287(g), which deputized state and local law enforcement officers to identify non-citizens in violation of immigration laws. Towards the end of the Bush administration in 2008, this program was replaced by a more comprehensive initiative called Secure Communities, which I explain in detail in the next subsection as it is the focus of this paper.

The increased number of apprehensions, detentions, and deportations of Mexican migrants living in the U.S. has resulted in Mexico receiving large inflows of deportees as shown by Rozo et al. (2021). These stricter enforcement measures also likely prompted the return of other migrants to Mexico. Figure 1 provides striking, though suggestive, evidence of the relationship between heightened immigration enforcement and increased return migration from the U.S. to Mexico.

However, it is not obvious how the removal and return of these migrants would impact Mexico, particularly the migrant-sending regions exposed to these enforcement activities through their migrant networks. On the one hand, migrants' initial out-migration increased wages for workers in Mexico (Mishra 2007) which may suggest that their sudden and forced return would reverse such effects. On the other hand, the forced return migrants could leverage the skills and experience they acquired while living in the U.S. in ways that complement the local workforce. Recognizing both the potential challenges and the potential opportunities posed by the arrival of returnees, the Mexican government launched Somos Mexicanos. This program is designed to assist deportees and other returning migrants in their integration into Mexico, aiming to harness their skills and experiences for the benefit of both the individuals and their home country.

2.2 Secure Communities

Before 2008, federal officers under the Criminal Alien Program (CAP) or local officers under agreements with the Department of Homeland Security (DHS) interviewed arrested individuals to determine whether they were in violation of immigration law. The introduction of Secure Communities in 2008 marked a significant shift in procedure. Under this new system, the fingerprints of

arrested individuals are sent automatically sent directly to DHS. Upon receiving these fingerprints, DHS cross-references them with their Automated Biometric Identification System (IDENT) to determine whether the arrested individual is deportable.[13] Immigration and Customs Enforcement (ICE), the principal interior enforcement agency under DHS, can then issue a detainer request to the local law enforcement agency. These are requests to hold the individual in the agency's custody for 48 additional hours, during which time they can be taken into federal custody to start their removal proceedings. This can happen regardless of whether the individual has been convicted of the crime for which they were arrested. This technology is much more efficient at identifying and removing individuals and thus significantly increased the probability of being deported conditional on being arrested (Alsan and Yang 2022). From 2009-2019, 700,000 individuals were removed through SC, the large majority of whom were Mexican nationals.

Starting in October of 2008, SC was gradually rolled out county-by-county. By January 22, 2013, it had been activated across all counties in the U.S. Miles and Cox (2014) study the determinants of activation timing across U.S. counties. They find that the best predictors for earlier activation included proximity to the border, the county's Hispanic population share, and the presence of 287(g) agreements. Cox and Miles also show that activation timing was not associated with county crime or unemployment rates.[14] East et al. (2023) also show that trends in labor market outcomes were similar for earlier and late adopters.

In the spring of 2011, governors of Massachusetts, New York, and Illinois sought to effectively opt out of SC by ending their agreements with DHS. After several jurisdictions continued to disobey detainer requests, SC was temporarily suspended in November of 2014 and replaced with the Priority Enforcement Program (PEP). Under PEP, ICE was to transfer only individuals convicted of serious offenses. In this paper, I focus on the number of removals in the fiscal years 2009-2015 from fingerprint matching and thus will include some removals under PEP. Throughout this paper, I refer to all removals stemming from the fingerprint matching program as SC for simplicity.

My empirical strategy detailed in Section 3 leverages the variation across counties in the number of SC deportations of Mexican migrants from 2010-2015. This variation is driven by differences across counties in the timing of SC's activation, the composition of the pool of arrestees, and the level of cooperation between local law enforcement agencies and federal immigration enforcement (Ciancio and García-Jimeno 2022). I also present results identifying the effects of immigration enforcement using only the timing of SC's staggered rollout across counties as a source of quasi-random variation.

[13] Deportable noncitizens are those that have violated immigration law (e.g., entered illegally, previously deported, overstayed visa) or who are lawfully in the U.S. but are convicted of serious crimes.

[14] Appendix A.2 confirms these results, showing that counties adopting SC earlier were similar to those adopting it later across various population and economic indicators, as well as industry shares.

3 Empirical Framework

This section describes the empirical strategy and data I use to identify the effects of deportations on labor market outcomes in Mexico. First, I describe how I measure exposure to U.S. deportations and identify their effects. Second, I present the baseline regression specifications to estimate the effects of deportations on key labor market outcomes in the Mexican microdata, namely employment and monthly earnings, as well as potential channels through which deportations could impact these outcomes. Third, I describe the primary data sources and the construction of the outcomes of interest. Fourth, I discuss identification, provide supportive evidence of the identifying assumption, and present an example which illustrates the identifying variation.

3.1 SC Deportation Shock

To identify the effects of deportations, I leverage the spatial variation across the U.S. in the application of SC during the period from 2010-2015. My analysis focuses on changes in outcomes during this period due to the availability of Mexican microdata, which I describe later in this section. Specifically, I predict the number of deportees through SC from each Mexican municipality using a variant of the shift-share instrument. The predicted number of deportees from municipality m deported from U.S. county j is determined by the municipality's relative network strength in j and the number of Mexicans removed from j through SC during this period. The total predicted number of deportees from municipality m is then the sum of the predicted deportees from all the U.S. counties:

$$d_m = \sum_{j \in J} Share_{mj} \times Deportations_j \tag{1}$$

where $Deportations_j$ is the number of interior removals of Mexican nationals from county j within the five-year period from 2011 to 2015 and $Share_{mj}$ is the share of Mexican undocumented migrants living in j from m.[15] To illustrate how this instrument works, consider the following scenario: suppose 10 percent of the Mexicans living in Harris County, Texas are from municipality m and Harris County deports 10,000 Mexicans through SC during the period.[16] In this case, the instrument would predict that 1,000 of those deportees are from municipality m. It would then predict the number of deportees from other counties in the U.S. that are from municipality m and take the sum, providing the total number of predicted deportees from municipality m.

I use restricted-access data on Mexicans removed through SC from the Transactional Records Access Clearinghouse (TRAC), spanning the years from 2008 to 2015. Subnational origin-destination

[15] I do not include removals of deportees apprehended by Customs and Border Patrol (CBP) as these individuals were more likely to be en-route to the U.S. The focus of this paper is instead on the effects of interior enforcement which is tasked with the removal of deportable migrants living in the U.S.

[16] According to data from the Transnational Records Access Clearinghouse (TRAC), there were 11,182 interior removals of Mexican nationals through SC during this period from Harris County, Texas.

migration data come from Mexico's Matrícula Consular de Alta Seguridad (MCAS) identification card program. As detailed in both Caballero et al. (2018) and Massey et al. (2010), these identification cards (IDs) serve various purposes such as facilitating the opening of a bank account and, in some states, acquiring a driver's license. Massey et al. (2010) explain that it is reasonable to assume that all of the MCAS IDs are issued to undocumented immigrants as legal immigrants do not have a need for such documentation. This implies that the MCAS data cover 75 to 80 percent of the undocumented Mexican immigrants living in the U.S. (Caballero et al. 2018). Mexico's Instituto de los Mexicanos en el Exterior (IME) has published annual counts of matrículas issued by migrants' U.S. state of residence and Mexican origin municipality. IME has also provided researchers with versions which provide these annual counts at the U.S. county-level. In this paper, I use MCAS data which provides migrants' municipality of birth and U.S. county of residence from Medina-Cortina (2023). Caballero et al. (2018) verify the quality and representativeness of this data from the MCAS program to study migration flows between sub-national locations.

I use the number of IDs issued to migrants from municipality m living in state/county j through the MCAS program to measure $Share_{mj}$. Given SC activation started in the earliest counties in late 2008 and removals through SC did not begin in earnest until 2009,[17] I use the matrículas issued between 2006 to 2008 to identify these initial networks. Specifically, the network strength of municipality m in U.S. county/state j is:

$$Share_{mj} = \frac{matriculas_{mj}}{matriculas_{j}} \tag{2}$$

where $matriculas_{mj}$ represents the number of matrículas issued to Mexican immigrants with residence in U.S. county j from Mexican municipality m from 2006-08 and $matriculas_{j}$ denotes the total number of matrículas issued to Mexican immigrants with residence in U.S. county j from 2006-2008. I scale the deportee shock by the initial size of the municipality's working-age population, providing an intuitive measure, the deportee population share. I will use the term "SC Deportee Share" to describe this measure of exposure throughout the paper.

The average value of the deportee share across the baseline sample of municipalities from 2010-2015 was 0.94. In other words, there was nearly one predicted deportee for every 100 residents in the average municipality.[18] Figure 3 shows there is considerable variation in exposure to the deportee share across Mexican municipalities, even within Mexican states which are outlined in black. This identifying variation is crucial as my identification strategy will absorb common shocks within Mexican states by including Mexican state fixed effects.

[17] Only five counties deported Mexican migrants in 2008 through SC.

[18] The total number of predicted SC removals as a percentage of the national working-age population was equal to 0.44 percent.

3.2 Effects of SC Deportations on Labor Market Outcomes

With the deportation shock in hand, I first provide causal estimates of the labor market effects of deportations. For most outcomes, I estimate the following specification:

$$\Delta y_m = \beta \cdot d_m + \alpha_s + X_m^{'}\gamma + \varepsilon_m \tag{3}$$

where Δy_m is the change in outcome y in municipality m from 2010-2015, d_m is the SC deportee share using deportations through SC from 2011-2015, α_s are Mexican state fixed effects, and X_m is a set of municipality controls. The baseline controls include the log of the municipality's square area and the following initial municipality characteristics all of which are measured in 2010: the log of the size of the working-age population, the gender ratio, the shares of the working-age population in various age groups (18-29, 30-39, 40-49, 50-59, and 60 and older), and the shares of the working-age population with different levels of attainment (no schooling, primary, high school, or university). Additional controls include the emigrant population share in 2010 (international migrants as a share of the population), the return migrant share in 2010 (migrants who returned from the U.S. since 2005 as a share of the population), and the percentage change in the size of the working-age population from 2005 to 2010. I cluster standard errors at the Mexican state level. The coefficient of interest β provides an estimate of the effect of a 1 percentage point (p.p.) increase in the predicted SC deportee share.

3.3 Data and Outcomes of Interest

The primary data sources are the 2015 Mexican Intercensal Survey (9.5 percent subsample), the 2000 and 2010 Mexican Population and Housing Censuses (10.6 and 10 percent subsamples, respectively), and the 2005 Mexican Count of Population and Dwellings (10 percent subsample) microdata from IPUMS International. Following Jaeger et al. (2018), I restrict my sample to those who are 18 to 64 and not in school in each of these surveys. I conduct my analysis on the sample of Mexican municipalities using IPUMS 1960-2015 consistent boundaries observed in the Mexican microdata in 2000, 2005, 2010, and 2015, of which there are 2,318 across 30 Mexican states.[19]

The Mexican microdata provides respondents' previous municipality or country of residence five years prior to the survey. I classify an individual as a U.S. return migrant if they were in the U.S. five years prior to the survey. I classify an individual as a local (i.e., a non-migrant) if they were not in the U.S. five years prior. In this paper, I focus on the effects of deportations on locals' labor market outcomes, following the literature on the labor market effects of international migration, which typically studies how migration flows from foreign countries impact non-migrants

[19]The sample does not include the municipality of Matías Romero Avendaño in the state of Oaxaca, as there were only four working-age respondents surveyed in 2015. In the states of Baja California Sur and Quintana Roo, there is only one IPUMS 1960-2015 consistent boundary municipality. The Mexican state fixed effects included in the baseline specifications absorb all of the variation for these two municipalities, so I drop them from the sample.

in the destination country.

The key labor market outcomes I use when estimating equation 3 include the change from 2010 to 2015 in locals' mean log monthly earnings and employment rate. Following Jaeger et al. (2018), I drop workers in the bottom and top percentile of earnings in each survey year to minimize the influence of outliers

The 2010 Census and 2015 Intercensal Survey ask whether a respondent or someone in their household receives income from abroad but do not collect information on the amount received. I thus estimate the effect of deportations on the share of households receiving remittances using equation 3, capturing effects on the extensive margin. To supplement this analysis, I use data on the total amount of remittances received in 2015 at the municipality level from the Banco de México, Mexico's central bank.

Using the information on respondents' prior municipality or country of residence, I generate municipality-level counts of the number of return migrants in each period, providing a measure of the five-year return migrant inflow. I normalize the return migrant flow in each period by the initial working-age population, which provides the municipality's return migrant population share and a measure of the change in labor supply due to return migration.[20] To measure the net change in labor supply in each period, I calculate the percentage change in the size of the working-age population. I estimate the effect of deportations on the return migrant share and labor supply growth in the 2010-2015 period using equation 3.

All initial municipality characteristics I use as controls are measured in 2010 using the Mexican microdata except for the municipality's square area which comes from the municipality map provided by IPUMS International.

To examine pre-trends, I use data from the 2004 and 2009 Mexican Economic Censuses and homicide rates from 2003 to 2008, sourced from Mexico's National Institute of Statistics and Geography (INEGI). The Economic Censuses provide information on all economic activity for firms with establishments in the previous year. The 2005 Count of Population and Dwellings does not provide information on labor market outcomes, so I rely on the Economic Censuses to test for pre-trends in key labor market outcomes in the next subsection. These include earnings per worker (i.e., average earnings), wages per hour worked (i.e., hourly wages), and the employment rate. I define the employment rate in 2003 and 2008 as the number of employed personnel reported in the respective period's Economic Census divided by the size of the working-age population in 2000 and 2005 respectively, which I calculate using the Mexican microdata described above.

[20] Throughout this paper, migration flow rates/shares are multiplied by 100 so that coefficients can be interpreted as percentage points.

3.4 Identification

Identifying Assumptions and Supportive Evidence. The identification assumption is that deportation shocks are uncorrelated with changes in unobserved labor market conditions conditional on the controls. That is, trends in the outcome variables for less-exposed municipalities (i.e., those that will experience a smaller deportation shock) serve as an accurate counterfactual for more-exposed ones (i.e., those that will experience a larger deportation shock). I first discuss how the construction of the shock alleviates concerns about this assumption, then present validity checks supporting it.

One advantage of this shock is that unlike the standard Bartik/shift-share enclave instruments used in the immigration literature, the "share" in this case is measured *outside* of the municipality. The classic shift-share enclave instrument used in Altonji and Card (1991) uses the historic fraction of migrants from an origin (e.g., country) residing *inside* a particular destination geography (e.g., a metropolitan-statistical area or county). A well-known concern is that these shares could depend on prior economic shocks which acted as pull factors for migrants. If these economic shocks are sufficiently serially correlated, both reduced-form and instrumental variables estimates could conflate persistent immigration flows with positive economic shocks resulting in biased estimates of the effects of immigration. In contrast, I study labor market outcomes at the municipality level, but the exposure shares are based on the number of Mexican residents in U.S. counties, mitigating this concern.

A potential issue is that municipalities hit with negative labor market shocks may send more migrants to the U.S., mechanically increasing their measured exposure to deportations. If these labor market shocks are serially correlated, the deportation shock could be spuriously correlated with labor market outcomes. To address this, I control for the number of international migrants – the overwhelming majority of whom are living in the U.S. – as a share of the population in 2010. The shock will instead exploit the fact that different municipalities have networks in distinct U.S. counties which generates quasi-random exposure to SC deportations.

This assumption could still be violated if migrants sort to particular U.S. destinations with higher rates of SC enforcement based on underlying municipality characteristics. For example, suppose poor labor market outcomes or crime and violence at origin lead migrants to gravitate towards locations that will eventually have higher SC enforcement rates. This could happen if such migrants are more prone to compete with the U.S.-born workers for jobs or be involved in criminal activity, which could lead to more deportations and thus more measured exposure to SC. If conditions continue to deteriorate in these municipalities, then I would mistakenly infer that deportations negatively affect labor market outcomes at origin.

However, pre-trends tests reveal no such evidence of migrant sorting. I show that eventual exposure to SC deportations, measured by the SC deportee share (d_m), is uncorrelated with pre-existing trends in average earnings, average hourly wages, employment, and homicide rates. Appendix Ta-

ble A1 reports these results, supporting the identifying assumption.

Recent developments in the econometrics literature have brought new insights into shift-share designs. Goldsmith-Pinkham et al. (2020) show that with certain designs, exogeneity should be interpreted in terms of the shares. In this context, the identifying assumption is that municipalities with migrant networks in different destinations would have similar counterfactual trends in labor market outcomes absent the implementation and enforcement of SC. As mentioned in Section 2.1, in the seminal work on Mexican migrant networks, Munshi (2003) provides evidence consistent with this assumption. He shows that similar municipalities often sent their migrants to different destinations in the U.S., suggesting some randomness in the the formation and persistence of migrant networks.

Despite this evidence, the shares could still be endogenous if municipalities with networks in certain U.S. destinations systematically differ in unobservables. For example, municipalities with networks in manufacturing-heavy U.S. counties may be differentially affected by industry-specific demand shocks. However, Borusyak et al. (2022) show that even if shares are endogenous, one can still obtain consistent estimates by leveraging the exogeneity of the shifts (shocks). This shift-based approach is only consistent if there are many shifting units, which is satisfied here given the over 2,500 U.S. counties used to construct the deportation shock.

In this context, the shift-based approach to identification relies on exogenous variation in enforcement intensity across U.S. counties. Several studies have exploited this variation to explore the effects of SC on outcomes in the U.S. For example, Miles and Cox (2014) and Hines and Peri (2019) use the variation in the timing and intensity of SC to explore its impacts on crime and police effectiveness. Following the diagnostics recommended by Borusyak et al. (2022), I conduct a balance test across U.S. counties to check whether the variation in enforcement intensity (measured by the number of SC deportations per matrículas issued) is correlated with county-level confounders. Appendix A.3 describes results showing that counties with higher SC enforcement intensity do not differ significantly from lower-intensity counties across several demographic and economic indicators or industry shares, nor are they more or less likely to have had prior agreements between local law enforcement and ICE. This balance test provides reassuring evidence that differences in enforcement intensity across counties generate a set of quasi-random shifts.

Although enforcement intensity appears as-good-as-random, unobservables correlated with enforcement could still influence Mexican municipalities via migrant networks. As discussed in Section 2.2, variation in SC deportations stems partly from the staggered rollout of SC across U.S. counties. Counties that implemented SC earlier removed more individuals on average than those that adopted SC later. Several studies, including Alsan and Yang (2022) and East et al. (2023), use this staggered implementation to study SC's effects on outcomes such as safety net participation, wages, and employment.

To further strengthen identification, I also provide results which instrument for enforcement intensity using variation driven solely by SC's staggered rollout. Specifically, I replace the number

of deportations during the 2010-2015 period ($Deportations_j$) in equation 1 with the number of days SC was active in the county in the period multiplied by the number of Mexican non-citizens in the county in 2005.[21] I then similarly scale this shock by the initial size of the working-age population in the municipality. I provide results for all baseline outcomes using this activation timing shock ($\tilde{d}_m$) as an instrument for the SC deportee share (d_m).

Appendix A.3 additionally includes results from an analogous shift-level balance test using this alternative shock, and similarly suggests few differences between counties with high and low activation intensity. Counties with higher activation intensity had larger Hispanic population shares, consistent with Miles and Cox (2014), who show that a county's Hispanic population share strongly predicted earlier activation. Counties in border states tended to have lower activation intensity. To alleviate concerns that these or other characteristics correlated with enforcement intensity drive the baseline results, I also present results that control for exposure to salient potential confounders in Section 4.3.

Identification Example: Santiago Maravatío and Tarimoro. To illustrate the identifying variation, I provide an example using the municipalities of Santiago Maravatío and Tarimoro in Guanajuato, a state that has been the source of many Mexican migrants to the U.S. From 2006-2008, there were nearly 20 *matrículas* issued to Mexican migrants from both these municipalities living in the U.S. for every 100 working-age individuals in the municipalities. This suggests that emigration to the U.S. was of similar magnitude for the two municipalities in the years before SC. If migrants from the two municipalities had the exact same destination patterns, then the construction described in Section 3.1 would ensure that the deportation shocks would be almost the same for the two municipalities in both periods. Both Santiago Maravatío and Tarimoro did have fairly similar destination patterns as shown in Figure 2. The only difference was that a larger fraction of migrants from Tarimoro lived in California, whereas migrants from Santiago Maravatío were more likely to choose Michigan, Pennsylvania, Virginia, and Texas as their destinations.

These slight differences in the destination patterns between the two municipalities translated to varying levels of exposure to SC from 2010-2015 due to the variation across the U.S. in the policy's application. The map in Figure 2 shows the variation across U.S. states in the program's intensity as measured by the number of Mexicans deported for every 100 matrículas issued from 2006-2008.[22] Municipalities with strong networks in states with higher deportation intensities in a given period experienced more exposure as measured by the deportation shock. For example, in California, the deportation intensity was around 12, which meant 12 additional predicted deportees for a given municipality for every 100 migrants sent from that municipality to California. Deportation intensities in Michigan (22), Pennsylvania (13), and Texas (22) were even higher. Since a larger

[21] I measure the size of the Mexican non-citizen population using the Integrated Public Use Microdata (IPUMS) American Community Survey (ACS) (Ruggles et al. 2024) from 2005 and the crosswalks provided by Autor and Dorn (2013).

[22] As detailed in Section 3.1, the baseline SC Deportee Share uses variation in SC's intensity across U.S. counties.

fraction of migrants from Santiago Maravatío was located in these high-intensity states, its SC deportee share was higher in this period than that of Tarimoro. This may also explain why Santiago Maravatío had a higher return migrant share in that period as well (2.48 vs. 1.79). The figure also shows that the average log monthly earnings for men dropped more in Santiago Maravatío, which had greater exposure to SC deportations and return migration, than in Tarimoro (-5.0 percent vs. -2.6 percent).

Like this example, my identification strategy will use variation in exposure to SC deportations within Mexican states. This variation stems from differences in the pre-existing settlement patterns across Mexican municipalities and the differences across the U.S. in the enforcement and implementation of SC. I will use this quasi-random deportation exposure to identify the effects of deportations on earnings and employment, while also delving into the potential channels driving these effects, such as return migration.

4 Results

In this section, I will estimate the effects of deportations on locals' earnings and employment, investigate potential channels driving these effects, and discuss the robustness of the main results.

4.1 Effects of Deportations on Earnings and Employment

Table 1 reports estimates of the effect of deportations on log monthly earnings (columns 1-2) and the employment rate (columns 3-4) using both ordinary least squares (OLS) in the odd-numbered columns and instrumental variables (IV) in the even-numbered columns with the SC activation intensity instrument. The estimates suggest that a 1 percentage point (p.p.) increase in the deportee population share decreases locals' earnings by between 0.7 and 1.0 percent and increases their likelihood of being employed by 0.2 p.p., though the estimates on employment are insignificant. The OLS and IV results are similar, which suggests that all the sources of variation in SC deportation intensity have similar effects on labor market outcomes in Mexico.

The effects on the overall employment rates and earnings mask underlying heterogeneity, which I provide evidence of and discuss further in Section 5.1. In terms of magnitudes, the employment rate effect is small, as 63 percent of working-age individuals were employed in 2010, implying a less than 0.3 percent increase relative to the mean. The modest employment response is consistent with the findings on the labor market effects of migration flows in other developing countries which find little to no effects of migration on employment (see, e.g., Kleemans and Magruder 2018; Lebow 2022).

On the other hand, the estimated effect of deportations on earnings is meaningfully large. In the next subsection, I discuss three of the primary potential channels that could be be driving the negative effect of deportations on earnings.

Potential Channels Driving Negative Effect of Deportations on Earnings. Perhaps the most salient channel through which deportations can impact local Mexicans' earnings is through return migration. SC forcibly returned over 300,000 Mexican deportees to Mexico by 2015.[23] When friends or family are deported, it may push other Mexicans living in the U.S. to return to Mexico as well. This influx of return migrants provides a labor supply shock to Mexican municipalities which could put downward pressure on wages and thus reduce earnings. Though the relationship between deportations and return migration may seem mechanical, it is important to note the possibility that exposure to deportations as measured by equation 1 may not translate into return migration to origin municipalities. Deportees could opt re-emigrate to the U.S. or relocate to different municipalities.

Second, reduced emigration may also play a role. Exposure to the challenges faced by deported individuals might deter potential migrants from leaving. This reduction in emigration could also result in surplus labor, further decreasing wages.

Third, deportations could decrease remittances by decreasing the stock of potential remittance senders as migrants return to Mexico. Additionally, for those migrants still living in the U.S., increased immigration enforcement could force them to "work in the shadows" in less formal and secure jobs. The employment effects for those abroad could decrease the frequency and the amount they remit to family members and friends in Mexico. Given their economic importance in Mexico, a decrease in remittance income could decrease labor demand and result in lower wages and earnings.[24]

While the channels mentioned above are significant and pertinent in this context, it is important to note that this list is not exhaustive when it comes to potential factors driving the negative impact of deportations on the earnings of local workers in Mexico. Nevertheless, these channels stand out as prominent and can be studied using the available Mexican microdata. In the next subsection, I estimate the causal effects of deportations on return migration, the size of the working-age population, and remittances to determine the primary channel(s) through which deportations decrease earnings.

4.2 Evidence on Potential Channels Driving Negative Effects of Deportations on Earnings

Return Migration and the Size of Working-Age Population. Table 2 reports OLS and IV estimates of the effects of deportations on return migration (columns 1-2) and the change in the working-age population (columns 3-4). The results suggest deportations increased return

[23] According to data from TRAC, there were 320,680 interior removals of Mexican citizens through SC by 2015. This does not include removals from U.S. territories such as Puerto Rico or Guam.

[24] Remittances have accounted for around 2-3 percent of total GDP in Mexico since the early 2000s (The World Bank) and Woodruff and Zenteno (2001) find that remittances account for almost 20 percent of capital invested in microenterprises in Mexico.

migration, contributing to an overall increase in labor supply. A 1 p.p. increase in the deportee population share resulted in between a 0.15 and 0.24 p.p. increase in the return migrant population share, implying a 14-21 percent increase in return migration relative to the mean return migrant population share. This suggests that for every 100 predicted deportees from a given municipality, 15 to 24 additional migrants, who had previously lived in the U.S. in 2010, returned to Mexico and resettled in that same municipality. Some of these returnees may have been deportees themselves, while others could have been migrants who were not deported but were induced to return due to the SC program. The Mexican microdata through 2015 does not distinguish deportees from other return migrants.

As noted earlier, my measure of return migration includes only individuals who were in the U.S. five years prior and now reside in the municipality. This excludes those who emigrated and returned to the U.S. within the same five-year period. As a result, the coefficients in columns 1 and 2 likely underestimate the effect of deportations on return migration, as deportations may have prompted some migrants to return to Mexico within that timeframe.

I use the MCAS data on matrículas issued to new migrants and average return probabilities calculated using data from the Mexican Migration Project (MMP) to estimate the extent to which the return migrant share underestimates total return migration.[25] I find that the total predicted return migrant share – which includes both observed return migration and the predicted number of emigrants who returned within the five-year period – was larger than the observed return migrant population share by a factor of 3.7 for the average municipality in the 2010-2015 period. Rescaling the OLS and IV estimates of the effect of deportations on observed return migration by this factor suggests an increase in total return migration from the U.S. to Mexico of between 0.56 and 0.89 p.p.

These estimates align closely with the estimated effect on the size of the working-age population in columns 3 and 4, indicating that increased return migration likely fully accounts for the observed increase in labor supply. A 1 percentage point increase in the predicted deportee population share raised the working-age population by 0.60 to 0.90 percent, meaning that for every 100 predicted deportees, there were 60 to 90 additional working-age individuals in the municipality. Together, these results suggest that deportations increase labor supply primarily through return migration rather than reduced emigration.[26]

Remittances. To test whether decreased remittance income is a potential driver of the decrease in local Mexicans' earnings, I estimate the effect of deportations on the share of households receiving remittances using Equation 3. Columns 1 and 2 of Table 3 report these estimates which suggest that a 1 p.p. increase in the deportee population share led to a 0.53 to 0.61 p.p. increase in the

[25] See Appendix A.1 for details.

[26] The Mexican microdata in 2015 does not provide information on the number of international emigrants. Using the issuance of matrículas issued to new migrants as a proxy for emigration suggests that exposure to SC deportations did not decrease emigration, supporting the calculations here.

share of households receiving remittances. This represents a 5.0 to 5.7 percent increase in the average municipality, where 10.62 percent of households received remittances in 2010.

The results in columns 1 and 2 suggest that deportations increased remittances on the extensive margin. Additionally, columns 3 and 4 provide suggestive evidence that remittances also increased on the intensive margin, as I estimate the effect of deportations on remittances per capita (measured in U.S. dollars) using a modified version of Equation 3, where the dependent variable is the 2015 level of remittances (y_m) rather than the change from 2010-2015 (Δy_m). Although these regressions do not control for time-invariant municipality factors, the cross-sectional relationship between remittance amounts and deportations is consistent with the results in columns 1 and 2, suggesting that deportations increased remittances on both margins. These findings are consistent with Amuedo-Dorantes and Puttitanun (2014), who also find that SC deportations increased remittances using data from the Mexican Migration Project (MMP), which offers rich information on Mexican migration but is not nationally representative.

Summarizing Evidence on Potential Channels. Collectively, the results in Section 4.2 point to return migration as a key channel through which deportations may decrease local Mexican workers' earnings. Deportations significantly increase return flows from the U.S. to Mexico which could increase labor market competition for local Mexican workers and thus offer a plausible explanation for their observed drop in earnings. In Section 5, I delve further into this mechanism, exploring the role of returnee-induced labor market competition.

Moreover, the findings challenge the idea that reduced remittance income is responsible for the decline in local Mexican workers' earnings. In fact, remittances *increase* as a result of deportations, which is consistent with migrants abroad responding to local labor market conditions back home in Mexico. As earnings decline for workers in Mexico, an increasing fraction likely reach out to their friends and family abroad for financial assistance to maintain consumption levels. This dynamic leads to an increase in the share of households receiving remittances. Another possibility is that return migrants leverage the contacts they established in the U.S.to access resources, contributing to an increase in remittances.

These findings offer another perspective into the underlying mechanisms in Caballero (2020), who finds that SC increased dropout rates of Mexican children. She explains that children leaving school for work can be attributed to household budget constraints resulting from a decline in remittance income. The findings presented here align with Caballero's proposed channel, namely that SC increased dropout rates by decreasing household income. However, my results suggest that the decline in household income may have stemmed from reduced earnings among other household members due to heightened labor market competition resulting from SC-induced return migrant flows as opposed to falls in remittance income.

4.3 Robustness of Main Results

Before exploring the return migration channel in further detail, I conduct several robustness checks to ensure a causal interpretation of the main results. I report several important checks in Tables 4 and 5 with further results in the Appendix.

SC Activation Correlates. As mentioned in Section 2.2, the timing of SC's rollout was fairly random; however, there were some characteristics which predicted early activation, including proximity to the border, the county's Hispanic population share, and the presence of 287(g) agreements (Cox and Miles 2013; Alsan and Yang 2022). Appendix A.2 confirms these patterns. If differences among these characteristics is associated with other shocks that may impact Mexican municipalities through migrant networks, then this would result in biased estimates of SC's impacts on labor market outcomes in Mexico. Borusyak et al. (2022) show that when there are shift-level correlates that could impact the outcome of interest, the solution is to control for these confounders using the same exposure shares in the instrument. Specifically, this would entail replacing $Deportations_j$ in equation 1 with these correlates.

In panel (a) of Tables 4 and 5 I provide OLS and IV results which include share-weighted controls for a set of county predictors for early activation. These include the county's Hispanic population share in 2005, indicators for whether there were state or local 287(g) agreements, and an indicator for being in a border state. Inclusion of these controls does not meaningfully change the results. The estimated effect on earnings, return migration, and working-age population growth remain positive and significant. The estimates of SC's effect on the share of households receiving remittances are still positive and similar in magnitude but becomes slightly noisier compared to the baseline estimates in Table 3.

Early Adopters and Sanctuary Cities. In panel (b) of Tables 4 and 5, I report results which similarly control for exposure to early adoption counties, which I define as counties that implemented SC in the first year since its launch, and counties that adopted sanctuary city policies at any point through 2017.[27] The results are robust to the inclusion of these controls, suggesting that the differences across counties that adopted SC very early and those that limited or refused cooperating with federal immigration enforcement are not driving the baseline results.

Great Recession. Caballero et al. (2021) show that U.S. labor demand shocks during the Great Recession increased return migration and decreased emigration. Conover et al. (2021) use these labor demand shocks to show that the resulting decline in Mexican net migration had negative employment affects for workers in Mexico.

To ensure the Great Recession is not driving the estimated effects of SC on labor market

[27]Data on the sanctuary city policies comes from the replication package of Alsan and Yang (2022)

outcomes, I construct measures that capture each munipality's exposure to the Great Recession, which I measure across counties as the percentage change in employment from 2006 to 2010 using county employment data from the U.S. Census Bureau's County Business Patterns. Reassuringly, the results in panel (c) of Tables 4 and 5 are robust to the inclusion of these controls.

Sensitivity of Baseline Results. In Figure A4, I exclude municipalities from individual Mexican states from the baseline sample. These results suggest that the results are are not sensitive to the definition of the baseline sample and that the effects are relatively uniform.

Alternative Methods for Conducting Inference. Adao et al. (2019) show that conventional standard errors, including those clustered at some geographic level, tend to result in severe overrejection in shift-share regression designs. This is due to the fact that regression residuals are correlated across regions with similar shares. To ensure that my standard errors do not suffer from this same problem, I use the package provided by Adão et al. (2020) to calculate adjusted standard errors using the Mexican municipality-U.S. state share matrix.

Clustering by Mexican state allows for the errors across municipalities within the same Mexican state to be correlated. However, there could also be correlation in the error terms across nearby municipalities in different states. To account for this, I allow for arbitrary dependence of the errors across municipalities within 75 and 100 kilometer distances using the methods of Conley (1999) as well as a version with distance decay based on the Bartlett kernel provided by Colella et al. (2019).

Another concern when clustering standard errors is the issue of "few" clusters which can lead to overrejection. In the baseline analysis, I cluster standard errors by Mexican state, of which there are 30 in my sample. Cameron and Miller (2015) mention that there is no general consensus on the definition of "few" as it can range from 20 to 50 clusters when the number of observations across clusters are fairly balanced, and even more when they are unbalanced. Cameron et al. (2008) provide simulations using the standard cluster robust variance estimator with the number of clusters being equal to 30 and the critical value of 1.96 which have rejection rates of 0.068. This provides some reassuring evidence that 30 clusters is enough in my case, especially since the number of observations per cluster is large and relatively balanced apart from the Mexican state of Oaxaca, which has over 500 municipalities. However, since there may still be concerns about overrejection, I conduct wild cluster bootstrap tests for the coefficient of interest in the main results using the package from Roodman et al. (2019) which corrects for the potential bias that results from a small number of clusters.

Appendix Table A2 shows the main results with the corresponding p-values calculated using the baseline method in parentheses, along with p-values calculated using these alternative methods. These results suggest that the estimates are largely significant across an array of alternative inference procedures. The estimated effects on employment are insignificant at conventional levels

under most alternative methods, similar to the baseline. For the estimated effect on the working-age population, p-values are slightly larger with several alternative methods but remain marginally significant below the 10 percent significance level. The estimated effects on earnings, return migration, and remittances remain significant throughout, with p-values consistently at or below the 5 percent significance level.

5 Key Channel: Deportation-Induced Return Migration

The results in the previous section show that deportations decrease workers' earnings in Mexico, while also ruling out the possibility that this effect is driven by deterred emigration or decreased remittances. Instead, they suggest that return migration could be a potential channel. The inflow of return migrants may increase labor market competition, reducing local workers' wages and, consequently, their earnings. In this section, I first provide evidence supporting this mechanism, followed by estimates of the elasticity of earnings with respect to the number of workers, using exposure to SC deportations as an instrument for the size of the working-age population. I then benchmark these estimates to the literature and conclude by discussing several contextual factors that could explain the relatively large negative effect of deportations on earnings in the origin country.

5.1 Mechanism: Labor Market Competition with Return Migrants

Heterogeneity by Gender and Education. To highlight evidence of migrant-induced labor market competition, I examine the relationship between the composition of return migrants and the local workers most affected. I start by showing the effects of deportations on the quantity of return migrants by gender and education, then compare those results with the effects of deportations on locals' earnings across the same categories. I divide local workers into two education levels: those without a high school degree and those with a high school degree or more.

Panel (a) of Table 6 reports the results from estimating equation 3 on the return migrant share overall (column 1) and broken down by gender and education (columns 2-5). The results show that the over half the increase in return migration is driven by inflows of return migrant men without a high school degree.[28] To further validate the composition of the increased labor supply, I estimate equation 3 on the percentage change in the size of the working-age population (column 1), along with this change broken out by gender and education category (columns 2-5). The results in panel (b) show that men without a high school degree similarly account for over half of the increase in workers, while men with a high school degree or more and women without a high school degree each contribute around 20 percent.

[28] Table A4 shows that even when comparing return migrants to locals from the same municipality, return migrants are more likely to be men and less likely to have a high school degree.

Panels (c) and (d) of Table 6 report results from estimating Equation 3 on the change in mean log monthly earnings and the employment rate for all working-age locals (column 1) and separately by gender and education group (columns 2-5). The earnings decline is concentrated almost entirely among local men, with a small and statistically insignificant effect for women. Notably, more educated men also experience earnings declines, which may reflect a relatively high elasticity of substitution between more-educated men and less-educated return migrants. Another possibility is that while the number of men with a high school degree increased far less in absolute terms than the number of men without one, proportionally their increase was substantial since they make up a smaller share of the workforce. In the average municipality in 2010, men with a high school degree or more made up 7.6 percent of the workforce, compared to 39.9 percent for men without a high school degree. Together, the results in panels (a) and (b) suggest that return migrants intensify labor market competition primarily for demographically similar local workers, who are presumably the closest substitutes.

In contrast, women drive the positive effect of deportations on the overall employment rate, while employment rates for men are unaffected. One potential explanation is that the decline in earnings among other male household members may compel women – who generally have low employment rates[29]–to seek work outside the household to help supplement household income.

Heterogeneity by Types of Workers. Similarly, the negative effects on earnings are likely concentrated in the types of jobs which return migrants tend to sort into. Panels (a) and (b) of Figure 4 show the shares of return migrants and locals across various occupation categories and employment types.[30] Overall, return migrants and locals have fairly similar distributions across occupation categories. The most common occupation categories for return migrants include agricultural, crafts and trades, elementary, operation/assembly, and service/sales occupations. Return migrants are approximately twice as likely to be working in agricultural occupations and over 30 percent more likely to be working in crafts and trades occupations. Return migrants are also more likely to be self-employed and thus less likely to be wage/salary workers.[31]

Panels (a) and (b) of Figure 5 report estimates of the effects of deportations on locals' earnings by occupation category[32] and employment type. Agriculture, crafts and trades, operation/assembly, and service/sales occupations drive the overall negative effect on earnings. In terms of employment type, self-employed locals, particularly those working on their account, ex-

[29] In 2015, the employment rate was 33.2 percent for women without a high school degree, 58.0 percent for women with at least a high school degree, 82.2 percent for men without a high school degree, and 88.0 percent for men with a high school degree or more.

[30] Occupations are classified into these categories in accordance with the International Standard Classification of Occupations (ISCO) scheme.

[31] These differences across occupation categories and employment types generally hold when comparing return migrants and locals from the same municipality as shown in Appendix Tables A5 and A6.

[32] I combine the following related occupation categories to improve precision: "Legislators, officials, and managers," "Professionals," and "Technicians and associate professionals."

perience the largest drop in earnings. These results are consistent with forced return migration decreasing wages and earnings for locals in jobs where labor market competition increased the most. The concentrated effect for self-employed workers align with other studies that have found larger wage effects for workers in the informal sector, where there is no minimum wage and greater wage flexibility.[33]

5.2 Elasticity of Earnings With Respect to the Number of Workers

I now turn to providing causal estimates of the effect of migrant-induced increases in labor supply on workers' earnings using exposure to SC deportation intensity and activation timing as instruments. The exclusion restriction in this context is that the SC deportation policy implementation and enforcement impact earnings for workers in Mexico only through increased labor market competition. The evidence presented so far supports the validity of this assumption.

Dividing the baseline OLS estimate of the effect of the SC deportee share on earnings in column 1 of Table 1 (-0.007) by the estimated effect on return migration in column 2 of Table 2 (0.149) suggests that a 1.0 percent increase in labor supply through return migration decreases earnings by 4.7 percent. The corresponding calculation using the IV estimates in column 2 of these two tables is -4.2 percent (-0.010/0.238). As mentioned in Section 4.2, the observed level of return migration underestimated the total predicted return migrant population share by a factor of 3.7 for the average municipality in 2015. Rescaling these estimates by this factor gives an elasticity of -1.1 to -1.3. Calculating the elasticity using the estimates of the effect of deportations on the increase in labor supply in columns 3 and 4 of Table 2 (0.602 and 0.901) provides a similar range between -1.1 and -1.2. In other words, these calculations suggest that a 1.0 percent migrant-induced increase in labor supply decreases earnings for workers in Mexico by between 1.1 and 1.3 percent.

In addition to these Wald estimates, I also estimate the following specification:

$$\Delta log(earnings)_m = \Theta \cdot \Delta\%LS_m + \alpha_s + Z_m^{'}\beta + \varepsilon_{m\tau} \tag{4}$$

where $\Delta log(earnings)_m$ is the change in mean log monthly earnings of local Mexicans from 2010-2015 and $\Delta\%LS_m$ is a measure of the percentage change in labor supply due to migration during the same period. I estimate equation 4 using both the observed return migrant share and the percentage change in the size of the working-age population as measures of the change in labor supply. As in the baseline analysis, equation 4 includes Mexican state fixed effects (α_s) and controls for initial municipality characteristics (Z_m), which include the baseline controls and controls for changes in labor supply in the 2000-2005 periods. The inclusion of controls for labor supply shocks in the prior decade is motivated by the theoretical and empirical findings in Jaeger et al. (2018),

[33]For example, Lebow (2022) finds that the negative wage effects of Venezuelan migration to Colombia are experienced mostly by workers in the informal sector. Similarly, Kleemans and Magruder (2018) find that internal migrants in Indonesia decrease income primarily in the informal sector.

who show that the shift-share migrant enclave instrument can be correlated with previous supply shocks and thus lead researchers to conflate the short-run effects of migration with responses to previous inflows. The coefficient of interest, Θ, provides the total effect of the migration labor supply shock on locals' earnings in Mexico. Estimates of this coefficient will provide the percentage change in earnings resulting from a 1.0 percent migrant-induced increase in labor supply. I provide causal estimates of Θ using the SC deportee share, d_m, and the activation timing instrument, $\tilde{d}_m$, as instruments for $\Delta\%LS_m$.

Table 7 presents ordinary least squares (OLS) and instrumental variables (IV) estimates of the effect of changes in labor supply on local Mexican workers' earnings. Columns 1-3 use the observed return migrant share as the endogenous variable, while columns 4-6 use the percentage change in the working-age population. Columns 2 and 5 use the SC deportee share as an instrument while columns 3 and 6 use the activation timing shock as an instrument.

For both return migration and the growth in the working-age population, the OLS estimates are biased upwards. This is likely due to the fact that migrants and workers often choose municipalities with favorable economic conditions where earnings are increasing. The IV estimates are significant at conventional levels, with evidence of a strong first stage and robustness to potential instrument weakness.[34] The IV point estimates in columns 2 and 3 suggest that a 1 p.p. increase in the return migrant share (i.e., one additional return migrant for every 100 working-age individuals) decreases the monthly earnings of local Mexicans by 3.8-4.1 log points. Adjusting this estimate by the average return migrant undercount factor provides an elasticity of earnings with respect to return migration of between 1.0 and 1.1. The IV estimates in columns 5 and 6 using the percent change in the number of workers provides a nearly identical range for the estimated elasticity.

Benchmarking Estimates to the Literature The Wald calculations and IV estimates suggest elasticity estimates ranging between -1.0 and -1.3. This range is in line with the literature on the labor market effects of migration, particularly those found in similar settings.[35] For instance, Morales (2018) finds that a 1 p.p. increase in population due to conflict-induced domestic migration in Colombia decreases wages by 1.4 percent. Lebow (2022) finds that a 1 p.p. increase in the migrant share as a result of the massive inflow of Venezuelans decreases wages by 1.1 percent. Both studies focus on developing countries in Latin America with migrant arrivals who share the same language, cultural background, and educational levels, similar to this setting.

[34] Appendix Table A3 reports results from the corresponding first stage regressions.

[35] Mishra (2007) examines the impact of Mexican emigration on labor markets in Mexico from 1970 to 2000. She finds that a 10 percent reduction in the number of workers in a skill group increases wages by 4 percent, implying an elasticity of -0.4. This elasticity is slightly lower than the range I estimate when analyzing SC's forced reversal of this migration. Mishra uses the skill-cell approach, which exploits variation across schooling and experience groups, rather than spatial variation in migrant inflows. While the skill-cell approach identifies relative wage effects of migration, the spatial approach assesses the total wage effect of migration on a group (Dustmann et al. 2016). The discrepancy in the estimated elasticities may stem from these differing methodologies or from the fact that Mishra's study primarily involves economic migrants leaving, whereas my analysis includes the effects of forced return migrants.

However, these results contrast with those of East et al. (2023), who study the labor market effects of the same immigration enforcement policy in the U.S. They find that SC decreased the number of foreign-born workers through deportations and increased unemployment among immigrants but did not increase wages for U.S.-born workers. Instead, their estimates suggest that a 1.0 percent decrease in the employment share of low-skilled foreign-born workers led to a 0.1 percent decrease in wages for U.S.-born workers. This suggests that the effects of immigration and immigration policy in destination countries like the U.S. might differ significantly from their consequences in origin countries like Mexico.

One possible explanation for these differences is that many migrants return to less developed regions of Mexico. Characteristics associated with underdevelopment may lead to different adjustments and more pronounced negative labor market effects of migration. This aligns with Verme and Schuettler (2021), who finds that the economic impacts of forced displacement are often largest in middle-income developing countries.

Another potential explanation is that return migrants in Mexico may more closely resemble the local workforce, making them more interchangeable compared to foreign immigrants in the U.S. Indeed, other work studying return migrants specifically has found relatively large negative effects on wages. For example, Edo (2017) studying the effects of French repatriates from Algeria finds wage elasticities ranging from -1.2 to -2.2. Additionally, return migrants to Mexico may bring new skills and resources acquired while living in the U.S., further increasing labor market competition for local workers and leading to larger declines in their earnings.

In the next section, I explore these potential explanations in detail.

6 Contextualizing the Labor Market Impact of U.S. Deportations in Mexico

6.1 Underdevelopment

In rich countries, more integrated factor markets and faster adjustments to migrant-induced labor supply shocks may mitigate the negative wage impacts. In contrast, developing economies with weaker infrastructure, less financial inclusion, a higher degree of informality in the labor market, and other factors characteristic of underdevelopment may experience more pronounced negative labor market effects from migration.

Here I explore whether underdevelopment exacerbates the negative effects of the labor supply shock caused by U.S. deportations on Mexican workers. To do this, I estimate a version of equation 3 which estimates the effect of deportations on earnings by varying levels of economic development. I include interactions between the deportee shock and indicators for whether the municipality falls within a given development quintile along with development quintile fixed effects. I use two mea-

sures of development: a.) GDP per capita and b.) a multidimensional poverty index. I construct a.) using data on total output in the 2009 economic census and municipality population counts from the 2005 Count of Population and Dwellings. I collect b.) from Mexico's National Council for the Evaluation of Social Development Policy (CONEVAL) which calculates the percentage of the municipality's population living in poverty according to their multidimensional index. For consistency with the other measure (GDP per capita), where higher values correspond with higher levels of development, I use the share of individuals *not* living in poverty.

The results in Figure 6 suggest that exposure to deportations does not lead to larger drops in earnings in poorer municipalities. In fact, deportations appear to have a positive effect for the municipalities in the poorest quintile. The point estimate in panel (a) suggests that a 1 p.p. increase in the deportee population share increases locals' earnings by close to 1.0 percent for municipalities in the bottom quintile of GDP per capita, with similar patterns in panel (b) when using the poverty measures from CONEVAL. This finding aligns with Bucheli and Fontenla (2022) who provide evidence suggesting that return migrants may positively impact economic development by bringing different skills, resources, and social norms. Meanwhile for municipalities in the second through fifth quintiles of economic development, the effect of deportations on earnings is fairly uniform and similar in magnitude to the baseline average effect.

These findings do not support underdevelopment as a potential explanation for the negative and relatively large earnings elasticity observed in this context. However, this does not necessarily rule out the possibility that certain features of underdevelopment could influence how municipalities absorb the inflow of migrants caused by deportations. It is possible that some features magnify the effects of migrant labor supply shocks while others mitigate them, which could explain the lack of heterogeneity observed in Figure 6. An ideal approach to address this question would leverage both exogenous variation in migration and exogenous variation in particular features of underdevelopment. Such an approach could provide credible evidence of whether, and how, the destination context influences the magnitude of migration's effects on locals' labor market outcomes.

6.2 Substitutability of Return Migrants

As mentioned in Section 5.2, several papers studying the labor market effects of migrant inflows have found sizeable negative effects for non-migrants in settings where the migrants closely resemble them in terms of their experience, culture, language, education, and/or occupational profile. Return migrants in Mexico are relatively similar to locals demographically, except for the fact that they are significantly more likely to be men. They are only slightly less likely to have a high school degree or more compared to locals in the same municipality and typically come from the middle rather than the bottom of the educational distribution, as shown in Appendix Table A4. Except for a few differences highlighted in Section 5.1, return migrants also tend to work in similar job

types as locals in terms of their occupations and employment arrangements.

In addition to these similarities, nearly all of these migrants are Mexican-born and speak the same language as the local population.[36] My identification strategy also leverages origin-municipality networks in the U.S., which means that the observed increase in return migration resulting from deportations is at least partially explained by migrants returning to their home municipalities where they can easily assimilate. Unlike other settings where migrants are concentrated into particular industries or occupations in which relatively few non-migrants work, these Mexican return migrants are likely reintegrating into the same labor markets as the local population, competing for similar job opportunities.

Here, I explore whether this feature of the migrant population is a potential mechanism driving the larger effects on earnings in this context. To do this, I take advantage of the spatial variation across Mexican municipalities the demographic similarity between return migrants and locals, as well as the similarity in their occupational distributions. I then test whether earnings fell by more in municipalities where return migrants were more similar demographically to locals and/or where return migrants tended to work in similar occupations as locals.

I measure demographic dissimilarity between return migrants and locals using the following equation:

$$skill\ dissimilarity_m = \frac{1}{2}\sum_{g \in G} |\pi_{rgm} - \pi_{lgm}| \tag{5}$$

where π_{rgm} is the share of return migrants in municipality m in skill group g and π_{lgm} is the share of locals in municipality m in the same skill group, both measured in 2010. The set G includes 110 skill groups based on five age ranges, two genders, and eleven education levels.[37] This measure of skill dissimilarity provides the share of return migrants that would need to be reallocated across various skill groups to match the demographic distribution of locals.

I similarly measure occupational segregation between return migrants and locals using the following equation:

$$occupational\ segregation_m = \frac{1}{2}\sum_{o \in O} |\pi_{rom} - \pi_{lom}| \tag{6}$$

where π_{rom} is share of return migrants in municipality m working in occupation o and π_{lom} is the share of locals in municipality m working in the same occupation, both measured in 2010. The set O includes 466 occupations.[38] Similar to the dissimilarity index above, this measure of occupational segregation provides the share of return migrants that would need to be reallocated

[36] 99 percent of the return migrants in 2010 and 2015 were born in Mexico.

[37] The five age categories are: 18-29, 30-39, 40-49, 50-59, and 60-64. The eleven education levels are: no schooling, some primary, primary, lower secondary general, lower secondary technical, secondary general, some college, secondary technical, post-secondary technical, university, and missing education.

[38] See the list of occupations on the IPUMS International website here.

across various occupations to match the occupational distribution of locals.

Appendix Figure A5 shows the distributions of skill dissimilarity and occupational segregation between return migrants and locals across Mexican municipalities. The average measures of skill dissimilarity and occupational segregation were 0.628 and 0.316 respectively for the 2,242 municipalities with non-missing measures in 2010.[39] This suggests that for the average municipality, approximately 63 percent of return migrants would have to be reallocated across skill groups and 32 percent across occupations to match the demographic and occupational distributions of the local workforce.

I estimate the heterogeneous effects of deportations on earnings by skill dissimilarity and occupational segregation using an identical approach as in Figure 6 and report these results in Figure 7. Panel (a) shows that in municipalities in the top quintile of demographic dissimilarity, where return migrants have much different demographic characteristics, the effects of deportations on earnings are positive and significant. However, for municipalities not in the top quintile of demographic dissimilarity, the effects of deportations are negative and similar in magnitude to the baseline average effect. Panel (b) shows a similar pattern. In municipalities in the top two quintiles of occupational segregation, deportations have a positive but insignificant effect, while in the bottom three quintiles, where return migrants work in more similar occupations to locals, the effects are negative, significant, and comparable to the baseline average effect.

Overall, these results suggest that the similarity between return migrants and the local workforce, and their ability to compete for the same jobs, contributes, at least in part, in driving the larger effects on earnings observed in this context. While return migrants may complement local workers in some municipalities, they appear relatively substitutable in most others.

Productivity of Return Migrants. Living and working in the U.S. provides migrants with opportunities to increase their human capital by developing new skills and gaining valuable experience. They may also acquire resources such as networks in the U.S. and boost their savings through higher wages. These factors could significantly increase the productivity of return migrants compared to locals in Mexico. If return migrants are more productive, they may compete more effectively for jobs, which could place additional pressure on local workers. As a result, the increased competition from highly productive return migrants might contribute to the significant negative impact of deportations on local workers' earnings.

Alternatively, if return migrants are negatively selected and less productive than locals, and if a sufficient number of return migrants were misclassified as locals due to returning within the five-year period, then the observed effects could be partially explained by changes in the composition of the working-age population. In this case, the observed drop in earnings might result from an influx of less productive individuals, leading to a decrease in average monthly earnings rather than an increase in labor supply and competition.

[39] These measures are missing for 76 municipalities in the baseline sample without return migrants in 2010.

To assess return migrants' productivity relative to locals, I compare the earnings of return migrants and locals in the same municipality. Table 8 reports results from regressions of each individual's log monthly earnings on an indicator for whether the individual is a return migrant, with varying control sets in the different columns. On average, return migrants earn approximately 9 percent more than locals in the same municipality. This difference holds after controlling for a rich set of controls which include a cubic in age and gender × educational attainment fixed effects and occupation, industry, and employment type fixed effects. These findings are consistent with return migrants being more productive than locals, which provides another potential explanation for the substantial drop in local workers' earnings as they face increased competition from these more productive return migrants.

7 Conclusion

This paper presents empirical evidence on the impact of U.S. deportations on Mexican labor markets. Using rich data on Mexican migration networks and a policy that expanded local immigration enforcement, I showed that deportations increase return migrant inflows, which increases labor market competition. Return migrants' similarities to the local workforce allow them to easily substitute for locals, and their higher productivity further intensifies competition, leading to significant drops in local workers' earnings

Beyond the immediate labor market effects, large-scale forced return migration could reshape the social fabric of receiving communities. First, the resulting changes in the gender ratio could impact marriage markets, household dynamics, and other gender-related outcomes. Second, return migrants may bring new political, religious, and social ideas, potentially influencing local norms. An important task for future research is to study these and other social consequences of forced return migration resulting from U.S. deportations. Studying the impacts on other migrant-sending countries is also warranted, particularly those that comprise the Northern Triangle (El Salvador, Honduras, and Guatemala) as these countries continue to account for a growing fraction of the U.S. immigrant population (Cohn 2017).

These findings are especially timely and relevant as the Trump administration has promised mass deportations (Hogan 2024) in the coming years. For Mexico, the results highlight the potential value of government programs like Somos Mexicanos and non-profits like New Comienzos, which help deportees and other return migrants reintegrate economically and socially. These initiatives not only help mitigate the immediate effects of forced return migrant inflows but also provide an opportunity to leverage migrants' skills to stimulate economic growth. For the U.S., the findings in this paper provide additional evidence of the potential unintended consequences of its immigration policy (Massey and Pren 2012). I show that removing undocumented Mexicans from the interior worsens economic conditions in their home communities. This suggests that restrictive immigration policies may not only disrupt local economies in the U.S. but also create economic pressures abroad

that could drive future migration.

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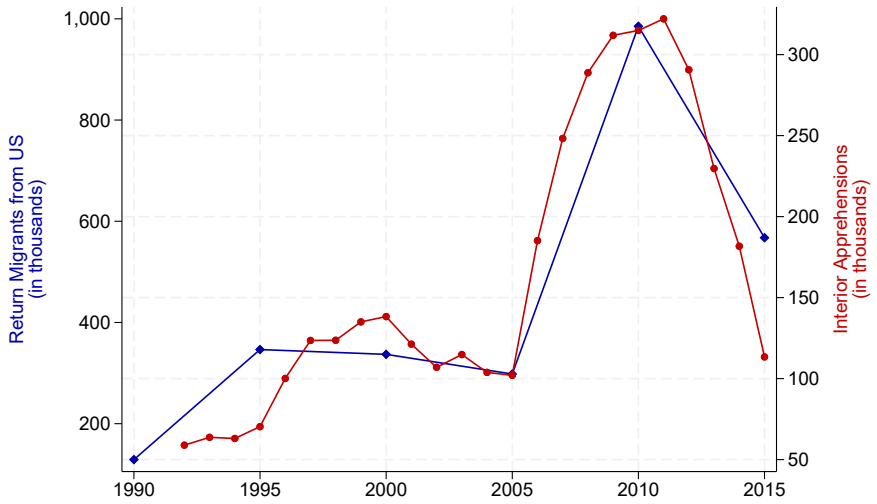
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Figures

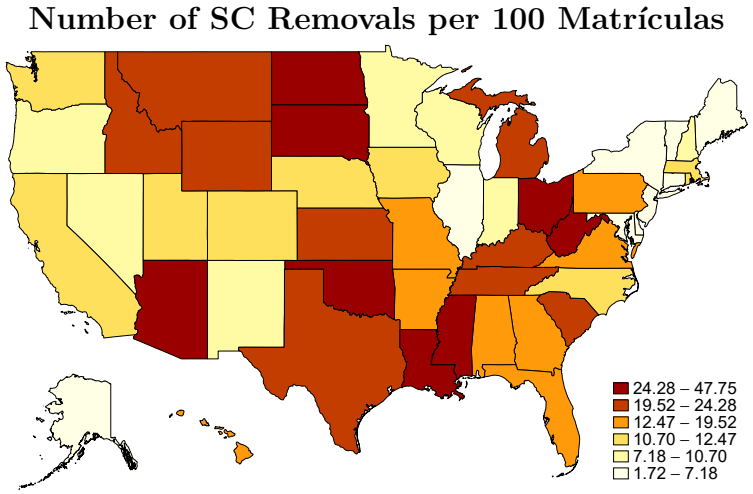
Figure (1) Interior Immigration Enforcement and U.S.-Mexico Return Migration



Notes: This figure shows the number of interior apprehensions by Immigration and Customs Enforcement (ICE) and its predecessor, the Immigration and Naturalization Service (INS), in each fiscal year from 1992-2015 and the five-year U.S.-Mexico return migrant flow in each five-year period from 1990 to 2015.

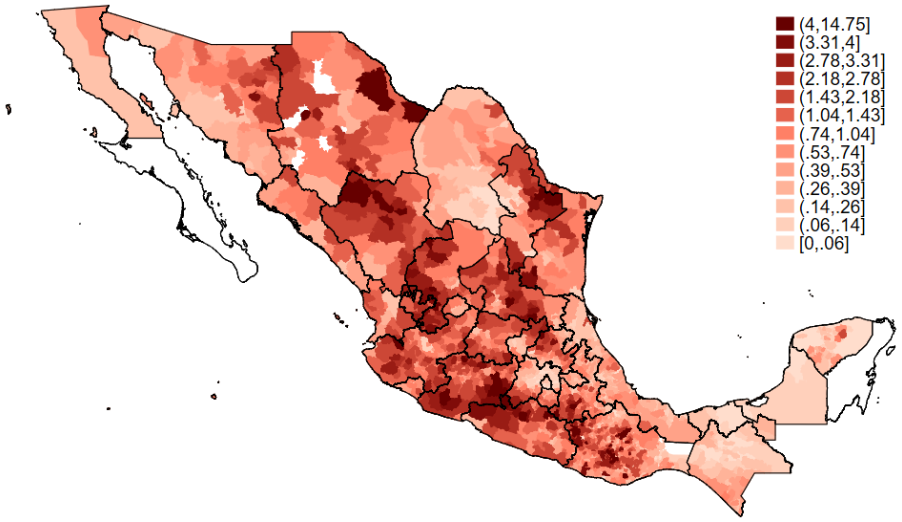
Figure (2) Identification Example: Santiago Maravatío and Tarimoro

	Santiago Maravatío	Tarimoro
California	7.9%	22.4%
Illinois	18.9%	17.7%
Michigan	5.8%	1.2%
Pennsylvania	4.2%	<0.1%
Texas	45.1%	38.3%
Virginia	4.0%	0.3%
SC Deportee Share (d_m)	2.59	1.98
Return Migrant Share	2.48	1.79
Δ Log Earnings (Men)	-0.050	-0.026



Notes: The top panel of the table to the left provides the percent of migrants from each of the two municipalities residing in California, Illinois, Michigan, Texas, and Virginia as measured by the number of matrículas issued from 2006-2008. The bottom panel of the table provides the SC deportee share, the return migrant share, and the change in mean log monthly earnings for men for the two municipalities. The map on the right shows the number of Mexicans removed through Secure Communities (SC) from 2011-2015 for every 100 matrícula consulares issued from 2006-2008 in each U.S. state.

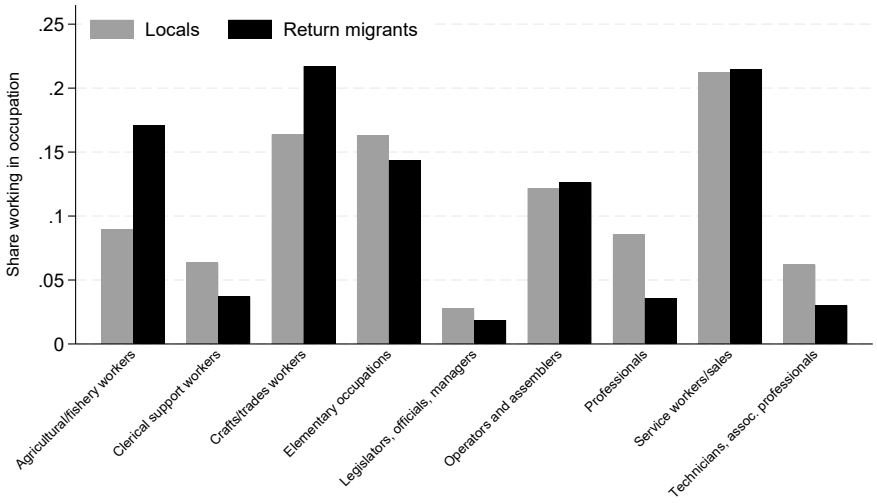
Figure (3) Variation in Exposure to SC Deportations Across Mexican Municipalities



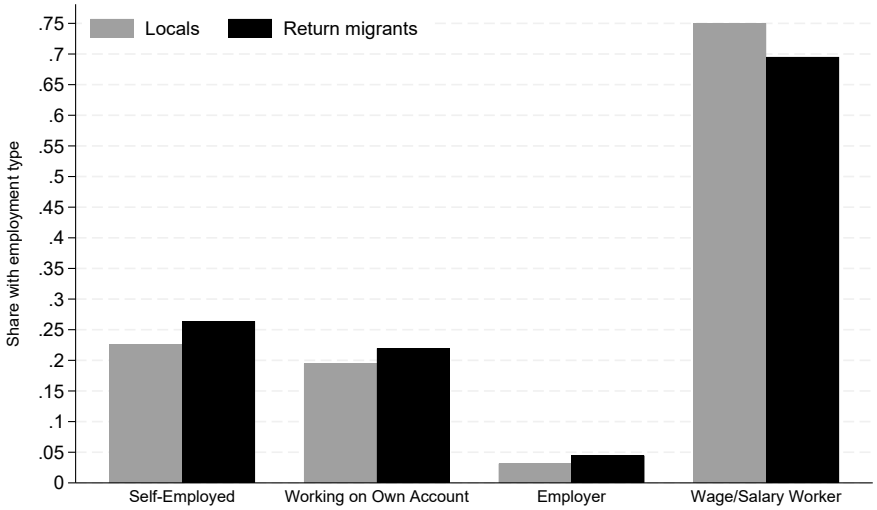
Notes: This map shows the SC deportee share (see Section 3.1 for details on the shock's construction) for the baseline sample of Mexican municipalities.

Figure (4) Occupations and Employment Types: Locals and Return Migrants

(a) Occupation

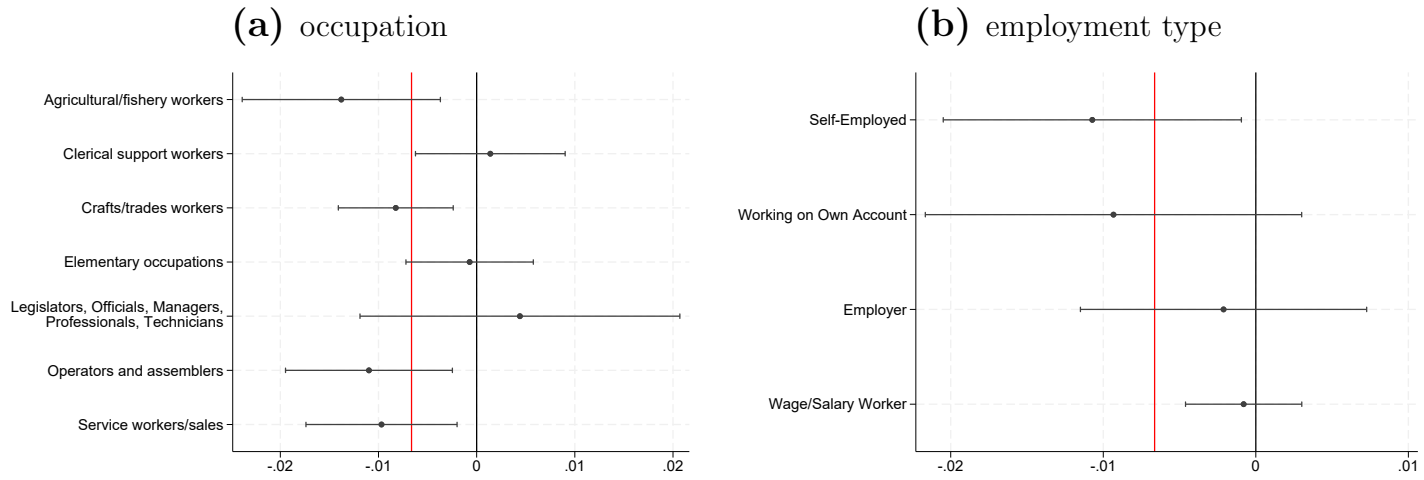


(b) Employment Type



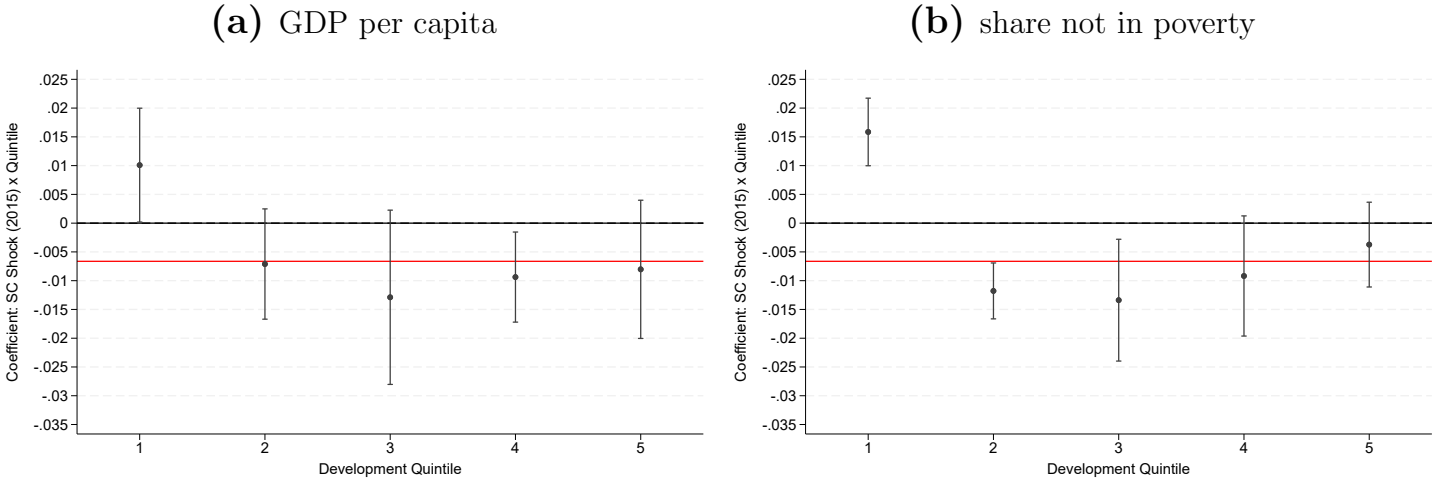
Notes: This figure shows the share of employed locals and return migrants working in different occupation categories (panel (a)) and employment types (panel (b)). The sample includes employed individuals in the baseline sample of municipalities between the ages of 18 and 64 that were not in school at the time of the survey in the 2015 Intercensal Survey.

Figure (5) Effect of Deportations on Earnings by Occupation Category and Employment Type



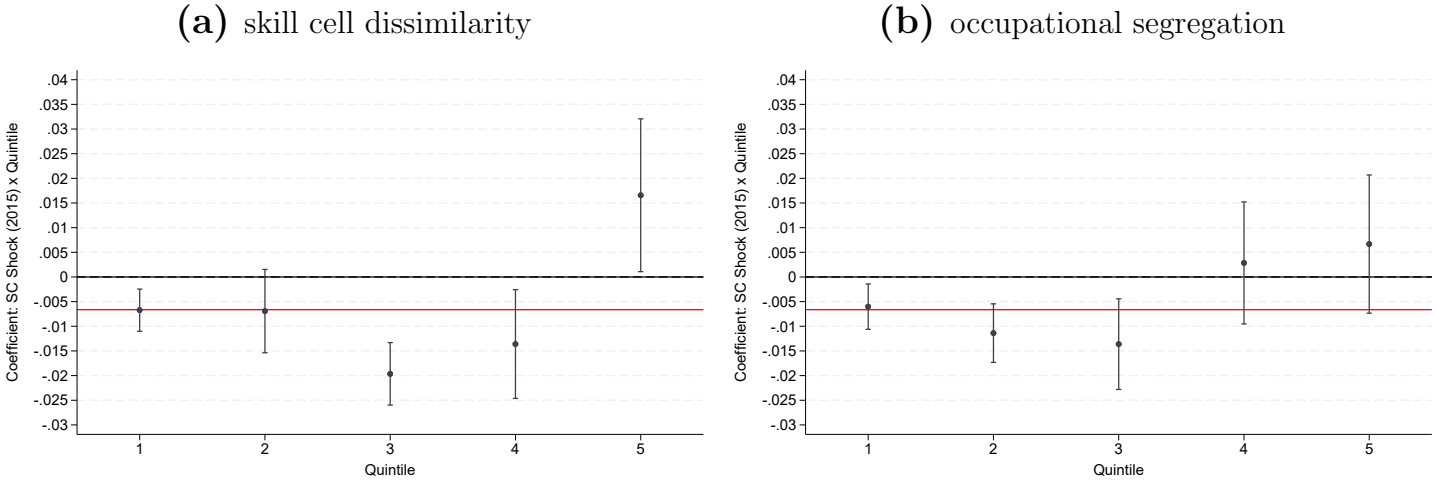
Notes: These figures show point estimates of the SC deportee share coefficient and 95 percent confidence intervals from regressions of equation 3. The dependent variable in each regression is the change in mean log monthly earnings for locals working in the given occupation category or employment type. The sample for each regression includes municipalities with non-missing changes in mean log monthly earnings for the corresponding occupation category or employment type. All regressions include the baseline controls detailed in the notes of Table 1 and Mexican state fixed effects. Standard errors are clustered at the Mexican state level. The red line demarcates the baseline estimate of the SC deportee share coefficient from column 1 of Table 1.

Figure (6) Effect of Deportations on Earnings by Economic Development



Notes: These figures show point estimates of coefficients and 95 percent confidence intervals from a version of equation 3 which separately estimates the SC deportee share coefficient by development quintile. The regression in panel (a) uses GDP per capita as the municipality's measure of economic development while the regression in panel (b) uses the multidimensional poverty index. The dependent variable in each regression is the change in mean log monthly earnings for the local working-age population. The sample for each regression includes the baseline sample of municipalities. All regressions include the baseline controls detailed in the notes of Table 1 and Mexican state fixed effects. Standard errors are clustered at the Mexican state level. The red line demarcates the baseline estimate of the SC deportee share coefficient from column 1 of Table 1.

Figure (7) Demographic Dissimilarity and Occupational Segregation



Notes: These figures show point estimates of coefficients and 95 percent confidence intervals from a version of equation 3 which separately estimates the SC deportee share coefficient by dissimilarity/segregation quintile. The regression in panel (a) uses the skill dissimilarity index while the regression in panel (b) uses occupational segregation. The dependent variable in each regression is the change in mean log monthly earnings for the local working-age population. The sample for each regression includes the 2,242 municipalities that had at least one return migrant in the 2010 Population and Housing Census. All regressions include the baseline controls detailed in the notes of Table 1 and Mexican state fixed effects. Standard errors are clustered at the Mexican state level. The red line demarcates the baseline estimate of the SC deportee share coefficient from column 1 of Table 1.

Tables

Table (1) Effects of SC Deportations on Log Monthly Earnings and Employment Rate

	Δ log earnings		Δ employment rate	
	(1)	(2)	(3)	(4)
SC Deportee Share	-0.007***	-0.010**	0.002	0.002
	(0.002)	(0.005)	(0.001)	(0.002)
Estimator	OLS	IV	OLS	IV
State FEs	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes
Dep. Var. Mean	-0.017	-0.017	-0.038	-0.038
Observations	2,318	2,318	2,318	2,318
KP Weak Ident., F-stat		144.9		144.9
AR Overident., p-val		0.017		0.155
KP Underident., p-val		0.060		0.060

Notes: This table shows results from estimating equation 3. The dependent variable in columns 1 and 2 is the change in mean log monthly earnings for the local working-age population. The dependent variable in columns 3 and 4 is the change in the employment rate for the local working-age population. Columns 2 and 4 instrument the SC deportee share using the activation timing shock ($\tilde{d}_m$) described in Section 3.4. The sample in each column includes the baseline sample of Mexican municipalities. All regressions include the baseline controls and Mexican state fixed effects. Baseline controls include the log of the municipality's square area, the percentage change in the size of the working-age population from 2005-2010, and the following initial municipality characteristics which are measured in 2010: the log of the size of the working-age population, the gender ratio, the shares of the working-age population in various age groups (18-29, 30-39, 40-49, 50-59, and 60 and older), the shares of the working-age population with different levels of attainment (no schooling, primary, high school, or university), and the emigrant and return migrant population shares. The Anderson-Rubin p-value corresponds to the null hypothesis that the coefficient on the endogenous regressor is zero and that the overidentifying restrictions are valid. The KP Underidentification test p-value corresponds to the Kleibergen-Paap Lagrange Multiplier (LM) test whose null hypothesis is that the equation is underidentified. Standard errors are clustered at the Mexican state level. Significance levels are denoted by $^*p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table (2) Effects of SC Deportations on Return Migration, Emigration, and Size of Working-Age Population

	return share		working age pop. growth	
	(1)	(2)	(3)	(4)
SC Deportee Share	0.149***	0.238***	0.602**	0.901**
	(0.025)	(0.076)	(0.285)	(0.388)
Estimator	OLS	IV	OLS	IV
State FEs	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes
Dep. Var. Mean	1.088	1.088	8.522	8.522
Observations	2,318	2,318	2,318	2,318
KP Weak Ident., F-stat		144.9		144.9
AR Overident., p-val		0.000		0.010
KP Underident., p-val		0.060		0.060

Notes: This table shows results from estimating equation 3. The dependent variables are the return migrant population share (columns 1 and 2) and the growth in the working-age population (columns 3 and 4). Columns 2 and 4 instrument the SC deportee share using the activation timing shock ($\tilde{d}_m$) described in Section 3.4. The sample in each column includes the baseline sample of Mexican municipalities. All regressions include the baseline controls detailed in the notes of Table 1 and Mexican state fixed effects. The Anderson-Rubin p-value corresponds to the null hypothesis that the coefficient on the endogenous regressor is zero and that the overidentifying restrictions are valid. The KP Underidentification test p-value corresponds to the Kleibergen-Paap Lagrange Multiplier (LM) test whose null hypothesis is that the equation is underidentified. Standard errors are clustered at the Mexican state level. Significance levels are denoted by $^*p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table (3) Effects of SC Deportations on Remittances

	share receive remittances		remittances per capita	
	(1)	(2)	(3)	(4)
SC Deportee Share	0.525**	0.614*	65.864***	74.968***
	(0.225)	(0.353)	(9.914)	(12.299)
Estimator	OLS	IV	OLS	IV
State FEs	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes
Dep. Var. Mean	0.322	0.322	201.765	201.765
Observations	2,318	2,318	2,318	2,318
KP Weak Ident., F-stat		144.9		144.9
AR Overident., p-val		0.086		0.000
KP Underident., p-val		0.060		0.060

Notes: This table shows results from estimating equation 3 in columns 1 and 2 and a version of equation 3 which uses the level of the outcome variable in 2015 (y_m) as the dependent variable as opposed to the change in levels (Δy_m) in columns 3 and 4. The dependent variables are the change in the share of households receiving remittances (columns 1 and 2) and remittances per capita (columns 3 and 4). Columns 2 and 4 instrument the SC deportee share using the activation timing shock ($\tilde{d}_m$) described in Section 3.4. The sample in each column includes the baseline sample of Mexican municipalities. All regressions include the baseline controls detailed in the notes of Table 1 and Mexican state fixed effects. The Anderson-Rubin p-value corresponds to the null hypothesis that the coefficient on the endogenous regressor is zero and that the overidentifying restrictions are valid. The KP Underidentification test p-value corresponds to the Kleibergen-Paap Lagrange Multiplier (LM) test whose null hypothesis is that the equation is underidentified. Standard errors are clustered at the Mexican state level. Significance levels are denoted by $^*p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table (4) Robustness Checks: OLS

Dependent Variable:	Δ log earnings (1)	Δ employment rate (2)	return share (3)	working age pop. growth (4)	Δ share receive remittances (5)
	(a) Controlling for Exposure to SC Activation Correlates				
SC Deportee Share	-0.009***	0.001	0.156***	0.541*	0.336
	(0.003)	(0.001)	(0.027)	(0.303)	(0.282)
	(b) Controlling for Exposure to Early Adopter and Sanctuary City Counties				
SC Deportee Share	-0.007**	0.002	0.145***	0.745**	0.558*
	(0.003)	(0.002)	(0.027)	(0.214)	(0.255)
	(c) Controlling for Exposure to Great Recession				
SC Deportee Share	-0.008***	0.003*	0.153***	0.622**	0.444
	(0.002)	(0.001)	(0.024)	(0.299)	(0.262)
Dep. Var. Mean	-0.017	-0.038	1.088	8.522	0.322
Observations	2,318	2,318	2,318	2,318	2,318
State FEs	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes

Notes: This table shows OLS results from estimating equation 3. The dependent variables are the change in mean log monthly earnings for the local working-age population (column 1), the change in the employment rate for the local working-age population (column 2), the return migrant share (column 3), the growth in the working-age population (column 4), and the change in the share of households receiving remittances (column 5). The sample in all panels and in each column includes the baseline sample of Mexican municipalities. All regressions include the baseline controls detailed in the notes of Table 1 and Mexican state fixed effects. Panel (a) additionally controls for exposure to county-level predictors of early SC activation, which include the Hispanic population share in 2005, indicators for whether there were state or local 287(g) agreements, and an indicator for being in a border state. Panel (b) additionally controls for exposure to early adopter counties (implemented SC in the first year) and counties that adopted sanctuary city policies at any point through 2017. Panel (c) additionally controls for exposure to the severity of the Great Recession across counties, as measured by the percentage change in employment from 2006 to 2010. Standard errors are clustered at the Mexican state level. Significance levels are denoted by $^{*}p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table (5) Robustness Checks: IV

Dependent Variable:	Δ log earnings (1)	Δ employment rate (2)	return share (3)	working age pop. growth (4)	Δ share receive remittances (5)
	(a) Controlling for Exposure to SC Activation Correlates				
SC Deportee Share	-0.013***	0.001	0.271***	0.863**	0.400
	(0.004)	(0.002)	(0.070)	(0.391)	(0.445)
KP Weak Ident., F-stat	122.985	122.985	122.985	122.985	122.985
AR Overident., p-val	0.001	0.602	0.000	0.033	0.365
KP Underident., p-val	0.051	0.051	0.051	0.051	0.051
	(b) Controlling for Exposure to Early Adopter and Sanctuary City Counties				
SC Deportee Share	-0.013**	0.003*	0.248***	1.003***	0.704
	(0.005)	(0.002)	(0.065)	(0.283)	(0.453)
KP Weak Ident., F-stat	223.924	223.924	223.924	223.924	223.924
AR Overident., p-val	0.009	0.092	0.000	0.000	0.123
KP Underident., p-val	0.038	0.038	0.038	0.038	0.038
	(c) Controlling for Exposure to Great Recession				
SC Deportee Share	-0.013***	0.003	0.250***	0.949**	0.524
	(0.004)	(0.002)	(0.075)	(0.386)	(0.401)
KP Weak Ident., F-stat	159.216	159.216	159.216	159.216	159.216
AR Overident., p-val	0.001	0.108	0.000	0.008	0.194
KP Underident., p-val	0.043	0.043	0.043	0.043	0.043
Dep. Var. Mean	-0.017	-0.038	1.088	8.522	0.322
Observations	2,318	2,318	2,318	2,318	2,318
State FEs	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes

Notes: This table shows IV results from estimating equation 3. The dependent variables are the change in mean log monthly earnings for the local working-age population (column 1), the change in the employment rate for the local working-age population (column 2), the return migrant share (column 3), the growth in the working-age population (column 4), and the change in the share of households receiving remittances (column 5). The SC deportee share is instrumented using the activation timing shock ($\tilde{d}_m$) described in Section 3.4. The sample in all panels and in each column includes the baseline sample of Mexican municipalities. All regressions include the baseline controls detailed in the notes of Table 1 and Mexican state fixed effects. Panel (a) additionally controls for exposure to county-level predictors of early SC activation, which include the Hispanic population share in 2005, indicators for whether there were state or local 287(g) agreements, and an indicator for being in a border state. Panel (b) additionally controls for exposure to early adopter counties (implemented SC in the first year) and counties that adopted sanctuary city policies at any point through 2017. Panel (c) additionally controls for exposure to the severity of the Great Recession across counties, as measured by the percentage change in employment from 2006 to 2010. The Anderson-Rubin p-value corresponds to the null hypothesis that the coefficient on the endogenous regressor is zero and that the overidentifying restrictions are valid. The KP Underidentification test p-value corresponds to the Kleibergen-Paap Lagrange Multiplier (LM) test whose null hypothesis is that the equation is underidentified. Standard errors are clustered at the Mexican state level. Significance levels are denoted by $^*p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table (6) Heterogeneity by Gender and Education

Dependent Variable:	All	Women No HS	Women HS Plus	Men No HS	Men HS Plus
	(1)	(2)	(3)	(4)	(5)
	(a) return share				
SC Deportee Share	0.149***	0.050***	0.003	0.089***	0.008**
	(0.025)	(0.007)	(0.002)	(0.019)	(0.003)
Dep. Var. Mean	1.088	0.224	0.064	0.689	0.109
Observations	2,318	2,318	2,318	2,318	2,318
	(b) working-age population growth				
SC Deportee Share	0.602**	0.129	0.045	0.320**	0.106**
	(0.285)	(0.116)	(0.070)	(0.146)	(0.039)
Dep. Var. Mean	8.522	1.235	3.356	1.177	2.819
Observations	2,318	2,318	2,318	2,318	2,318
	(d) Δ log earnings				
SC Deportee Share	-0.007***	-0.001	-0.001	-0.007***	-0.006**
	(0.002)	(0.005)	(0.004)	(0.002)	(0.002)
Dep. Var. Mean	-0.017	0.030	-0.082	-0.052	-0.141
Observations	2,318	2,303	2,274	2,317	2,283
State FEs	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes
	(c) Δ employment rate				
SC Deportee Share	0.002	0.005***	0.004	-0.002	0.001
	(0.001)	(0.001)	(0.005)	(0.003)	(0.004)
Dep. Var. Mean	-0.038	-0.031	-0.060	-0.059	-0.037
Observations	2,318	2,318	2,311	2,318	2,307

Notes: This table shows OLS results from estimating equation 3. In panel (a), the dependent variable in each column is the population share of the corresponding return migrant gender-education category. In panel (b), the dependent variable is the percentage change in the working-age population broken out by gender-education category. In panels (c) and (d), the dependent variables are the change in mean log monthly earnings and the employment rate for the local working-age population broken out by gender-education category. The sample in all panels and in each column includes the baseline sample of Mexican municipalities. All regressions include the baseline controls detailed in the notes of Table 1 and Mexican state fixed effects. Standard errors are clustered at the Mexican state level. Significance levels are denoted by $^{*}p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table (7) Elasticity of Earnings: Return Migration and Number of Workers

	(1)	(2)	(3)	(4)	(5)	(6)
Return Migrant Share	-0.014***	-0.041***	-0.038***			
	(0.002)	(0.014)	(0.011)			
Working-Age Pop. Growth				0.000*	-0.010*	-0.010***
				(0.000)	(0.005)	(0.003)
Estimator	OLS	IV	IV	OLS	IV	IV
Deportation Intensity Instrument		Yes			Yes	
Activation Timing Instrument			Yes			Yes
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes
Return Migrant Share (2000-05)	Yes	Yes	Yes			
Working-Age Pop. Growth (2000-05)				Yes	Yes	Yes
Dep. Var. Mean	-0.017	-0.017	-0.017	-0.017	-0.017	-0.017
Observations	2,318	2,318	2,318	2,318	2,318	2,318
KP Weak Ident., F-stat		29.2	12.4		5.8	8.3
AR Overident., p-val		0.019	0.052		0.006	0.016
KP Underident., p-val		0.011	0.016		0.075	0.048

Notes: This table shows results from estimating equation 4. The dependent variable in each column is the change in mean log monthly earnings for the local working-age population. Columns 1-3 use the return migrant share as the measure of the percentage change in labor supply ($\Delta\%LS_m$ in equation 4) while columns 4-6 use the percentage change in the size of the working-age population. Columns 2 and 5 instrument the change in labor supply using the SC deportee share (d_m) while columns 3 and 6 use the activation timing shock ($\tilde{d}_m$). The sample in each column includes the baseline sample of Mexican municipalities. All regressions include the baseline controls detailed in the notes of Table 1 and Mexican state fixed effects. Standard errors are clustered at the Mexican state level. Columns 1-3 additionally control for the return migrant share in the 2000-2005 period. Columns 4-6 additionally control for the percentage change in the size of the working-age population in the 2000-2005 period. Standard errors are clustered at the Mexican state level. The Anderson-Rubin p-value corresponds to the null hypothesis that the coefficient on the endogenous regressor is zero and that the overidentifying restrictions are valid. The KP Underidentification test p-value corresponds to the Kleibergen-Paap Lagrange Multiplier (LM) test whose null hypothesis is that the equation is underidentified. Standard errors are clustered at the Mexican state level. Significance levels are denoted by $^*p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table (8) Differential Earnings of Return Migrants

	(1)	(2)	(3)	(4)	(5)	(6)
Return migrant	0.089***	0.073***	0.087***	0.083***	0.076***	0.086***
	(0.025)	(0.021)	(0.021)	(0.020)	(0.021)	(0.020)
Municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls		Yes	Yes	Yes	Yes	Yes
Occupation FEs			Yes			Yes
Industry FEs				Yes		Yes
Employment Type FEs					Yes	Yes
Dep. Var. Mean	3.805	3.805	3.805	3.803	3.805	3.803
Observations	2,728,471	2,728,471	2,728,471	2,707,349	2,728,471	2,707,349

Notes: This table shows results from regressions of log monthly earnings on a return migrant indicator among individuals in the baseline sample of municipalities with non-zero earnings between the ages of 18 and 64 that were not in school at the time of the survey in the 2015 Intercensal Survey. Demographic controls include a cubic in age and gender × educational attainment fixed effects. There are 11 occupation categories based on the International Standard Classification of Occupations (ISCO) and 15 industry categories based on the International Standard Industrial Classification (ISIC). Employment type fixed effects control for whether the individual is an employer, self-employed and working on their own account, or a wage/salary worker (white or blue collar or day laborer). Observations are weighted by the person weights (perwt in IPUMS). Standard errors are clustered at the municipality level. Significance levels are denoted by $^{*}p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Appendix

A.1 Estimating Total Return Migration: Accounting for Within-Period Return Migration.

To gauge how much the baseline measure of return migration may underestimate total return migration —including migrants who leave and return within the five-year period—I predict the number of such returnees using a proxy for emigration outflows and average return probabilities using the following expression:

$$return\ emig_m = \sum_{t=2011}^{2014} matriculas_{mt} \times share\ return\ in\ (2015 - t)\ years \tag{A.1}$$

where $return\ emig_m$ is the predicted number of emigrants who migrated to the U.S. and returned to the Mexican municipality within the five-year period, $matriculas_{mt}$ is the number of matrículas issued to newly arrived Mexican migrants from municipality m in year t, and $share\ return\ in\ (2015 - t)\ years$ is the share of migrants that return within $2015 - t$ years. The number of matrículas issued to newly arrived Mexican migrants provides a proxy for emigration flows during the period. I base the shares on data from the Mexican Migration Project (MMP), using the proportion of migrants whose length of stay in the U.S. was less than or equal to $2015 - t$ years on their first migration. This approach takes the number of arrivals in 2011 and keeps the fraction who would have stayed four years or less. It then repeats this for arrivals in 2012, keeping those who stayed three years or less, and so on for 2013 and 2014. The sum provides a prediction of the number of emigrants wo migrated to the U.S. and returned to the Mexican municipality within the five-year period.

I then calculate the total return migrant population share, which is equal to the number of individuals who were in the U.S. five years prior to the survey plus the predicted number of within-period return migrants ($return\ emig_m$), normalized by the initial size of the working-age population and multiplied by 100. On average, the total predicted return migrant population share in 2015 was 3.7 times larger than the observed return migrant share. Given the size of this factor and the fact that deportations also likely increased within-period return migration, the estimated effect of deportations on return migration in Table 2 likely underestimates the true effect of deportations on return migration.

A.2 Predictors of SC Activation Timing.

In this section, I confirm findings from previous studies on the rollout of SC, highlighting similarities and differences between counties that adopted SC earlier and those that implemented it later. Using the Integrated Public Use Microdata (IPUMS) American Community Survey (ACS)

(Ruggles et al. 2024) from 2005 and the crosswalks provided by Autor and Dorn (2013), I construct several county-level characteristics, including the population size, the Hispanic population share, the employment and poverty rate, and the mean log hourly wage for working-age individuals (18–64) not enrolled in school at the time of the survey. I also examine sectoral composition across key industries: agriculture, mining and extraction, construction, manufacturing, retail, personal services, entertainment and recreation, and professional services.

Additionally, I incorporate county-level crime data from Alsan and Yang (2022) to construct crime rates and measure exposure to pre-existing interior enforcement programs using data on state and local 287(g) agreements active before 2008 (East et al. (2023)). Lastly, I capture pre-SC political preferences using the county vote share for Barack Obama in 2008 from Leip (2021).

Appendix Figure A2 presents results from a regression of the number of days SC was active during the 2010–2015 period on these county characteristics. The maximum duration was 1,826 days (January 1, 2011 – December 31, 2015) for the 864 counties that had activated SC by the end of 2010, while the minimum was 1,073 days for the last counties to implement SC on January 22, 2013. To ease interpretation, I normalize this dependent variable and the continuous right-hand-side variables to have a standard deviation of one, though I do not normalize the 287(g) agreements or Border state indicators. The sample includes the 2,722 counties where matrículas were issued from 2006-2008 to Mexican immigrant residents.

Counties that adopted SC earlier were generally similar to later adopters. Early-adopting counties did not have significantly different crime, unemployment, or poverty rates and had comparable industrial compositions, though they had a slightly lower share of agricultural workers and a slightly higher share in construction. These findings largely align with Cox and Miles (2013), which show that local politics and crime did not significantly influence SC activation timing. Instead, proximity to the border (measured by the Border state indicator), the county's Hispanic population size, and prior agreements with ICE were strong predictors of early activation.

A.3 Shift-Level Balance.

In this section, I provide additional details on the shift-level balance tests discussed in Section 3.4. Following the diagnostics recommended by Borusyak et al. (2022), I assess whether variation in SC intensity across counties is correlated with county-level characteristics to evaluate the plausibility of the exogenous shifts assumption.

I use two measures of SC intensity. The first, SC deportation intensity, is defined as the number of interior removals of Mexican nationals from 2011 to 2015 ($Deportations_j$ in equation 1) divided by the number of matrículas issued before SC (2006–2008). The second measure replaces $Deportations_j$ with the number of days SC was active during the period, multiplied by the number of Mexican non-citizens in 2005.

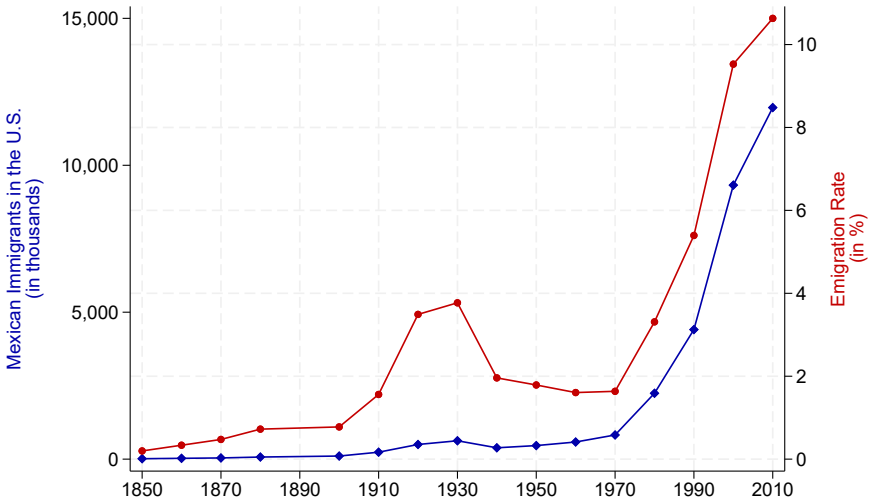
Panels (a) and (b) of Appendix Figure A3 present results of regressions of these two SC in-

tensity measures on various county characteristics (see Section A.2 for additional details on the construction of these variables). To ease interpretation, I normalize dependent variables and the continuous right-hand-side variables to have a standard deviation of one, though I do not normalize the 287(g) agreements or Border state indicators. As in Appendix A.2, the sample includes the 2,722 counties with matrículas issued from 2006-2008 to Mexican immigrant residents.

Panel (a) shows that counties with higher SC deportation intensity were similar to those with lower intensity across all county characteristics, supporting the assumption that these shifts are exogenous. Panel (b) shows similar patterns for SC activation timing intensity, though counties with larger Hispanic populations tended to have higher measured intensity. This likely reflects the correlation between activation timing and Hispanic population size, as shown in Appendix Figure A2. Counties in border states had lower activation timing intensity, likely because they had larger undocumented Mexican populations and thus a greater number of matrículas issued. All else constant, a larger undocumented population as a share of the total Mexican noncitizen population reduces the activation timing intensity measure.

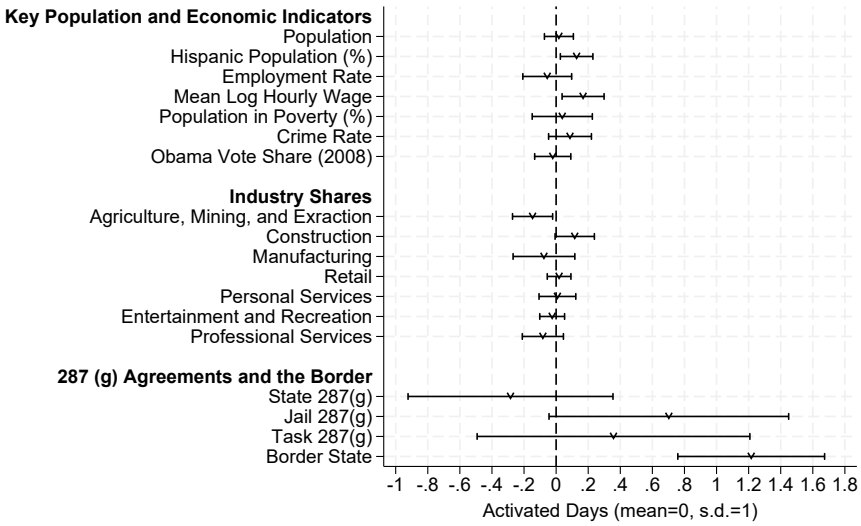
A.4 Additional figures

Figure (A1) Number of Mexican Immigrants in the U.S.: 1850-2010



Notes: This figure shows the number of Mexican immigrants in the U.S. and the percent of the Mexican population that resided in the U.S. (i.e., the emigration rate) in each decade from 1850 to 2010 except for 1880. Data on Mexican immigration levels comes from decadal censuses from 1850-2000 and the 2010 American Community Survey (ACS). Data on the size of the Mexican population comes from McCaa (2000) for the 1850-1890 period, from Alba and Potter (1986) for the 1900-1940, and from the World Bank from 1950-2010.

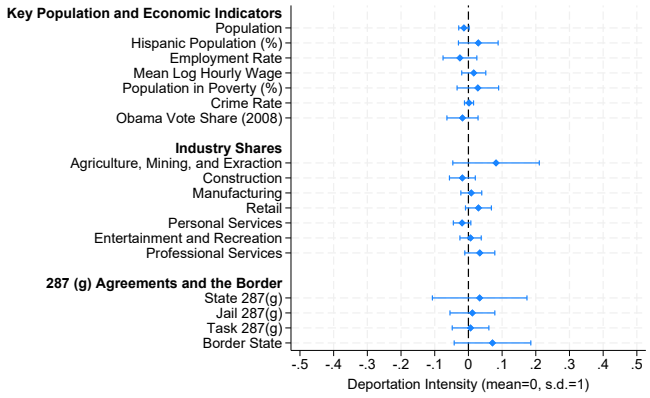
Figure (A2) Predictors of SC Activation



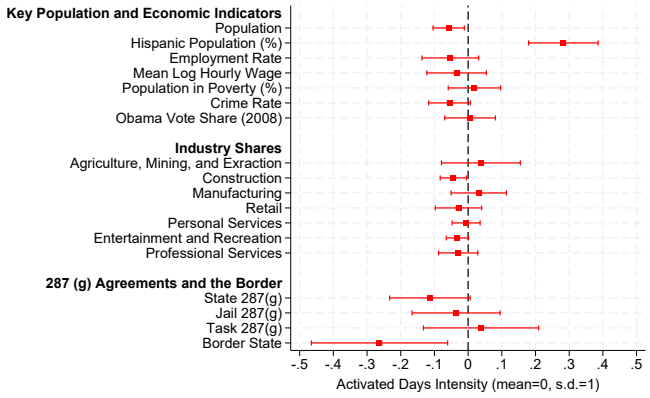
Notes: This figure shows point estimates and confidence intervals from a regression of the number of days SC was active during the 2010-2015 period on county-level characteristics (see Section A.2 for details on the construction of these characteristics). The dependent variable and the continuous right-hand-side variables are normalized to have a standard deviation of one. The sample includes the 2,722 counties with matrículas issued from 2006-2008 to Mexican immigrant residents. Standard errors are clustered at the U.S. state level.

Figure (A3) Shift-Level Balance

(a) SC Deportation Intensity

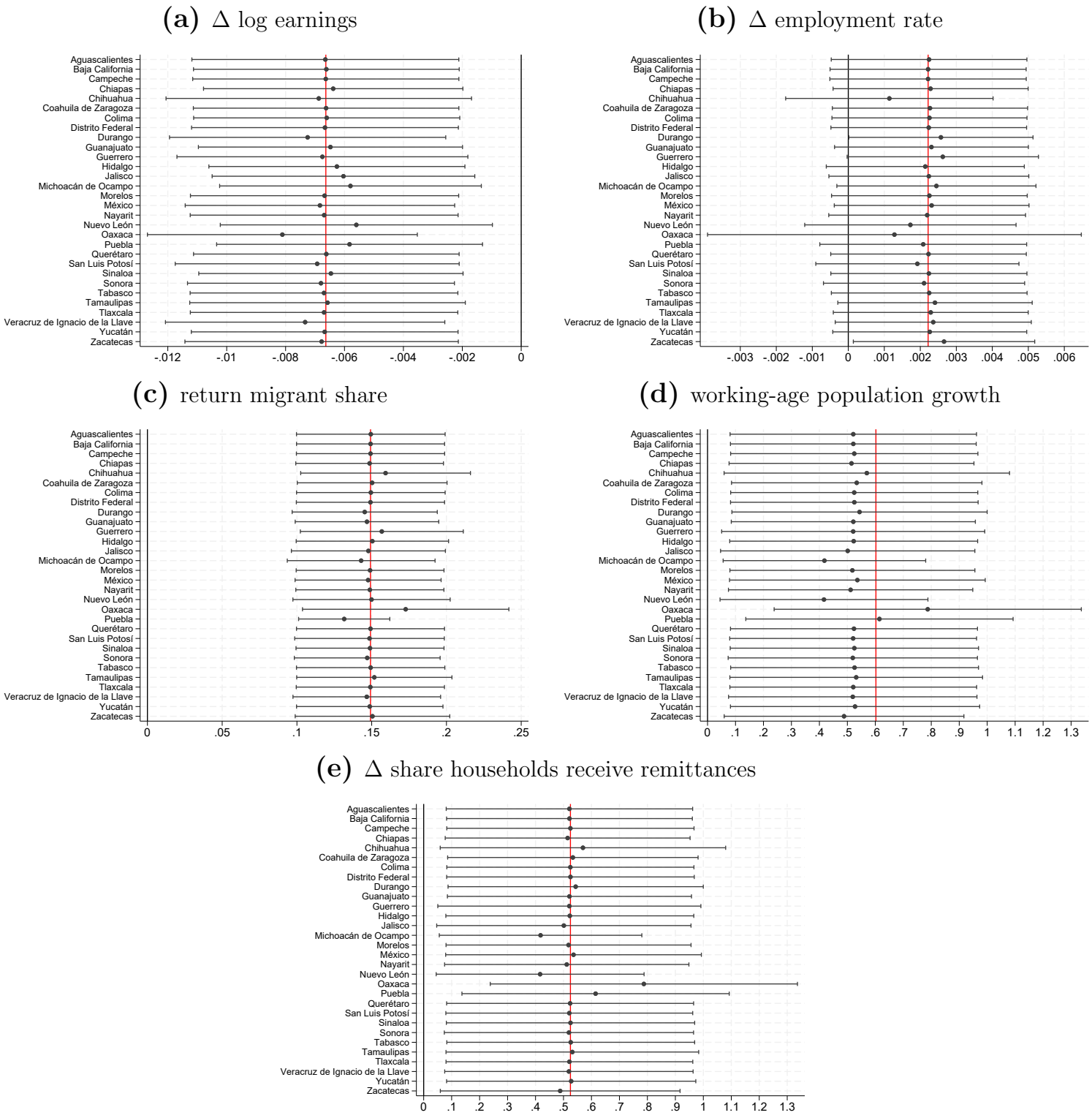


(b) SC Activated Days Intensity



Notes: These figures shows point estimates and confidence intervals from regressions of a measure of SC intensity on county-level characteristics (see Section A.2 for details on the construction of these characteristics). In panel (a), the dependent variable is the SC deportation intensity, which is equal to the number of interior removals of Mexican nationals from 2011 to 2015 ($Deportations_j$ in equation 1) divided by the number of matrículas issued before SC (2006–2008) to Mexican immigrants. In panel (b), the dependent variable is the SC activated days intensity, which is equal to the product of the number of days SC was active during the period and the number of Mexican non-citizens in 2005 normalized by the number of matrículas issued before SC (2006–2008). The dependent variables and the continuous right-hand-side variables are normalized to have a standard deviation of one. The sample includes the 2,722 counties with matrículas issued from 2006-2008 to Mexican immigrant residents. Standard errors are clustered at the U.S. state level.

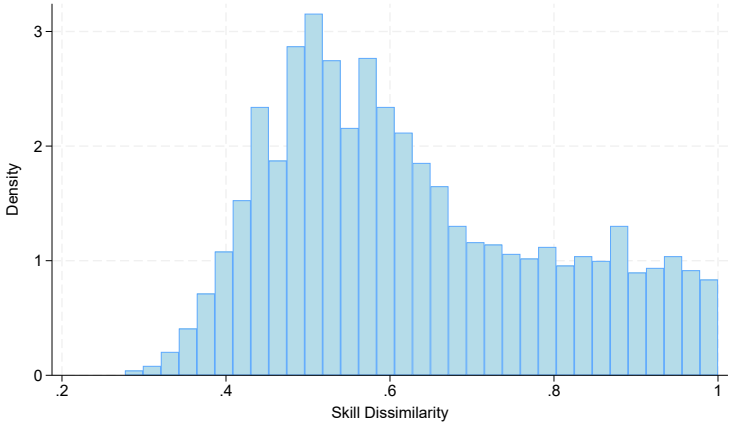
Figure (A4) Sensitivity of Baseline Results: Dropping Mexican States from Sample



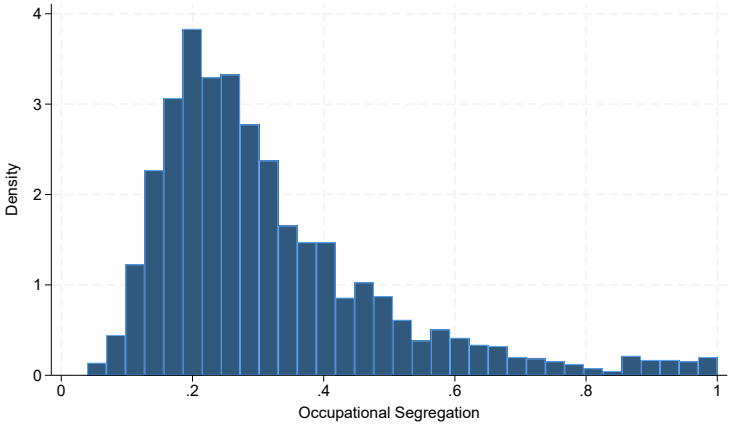
Notes: These figures show point estimates of the SC deportee share coefficient and 95 percent confidence intervals from regressions of equation 3 after dropping municipalities from individual Mexican states from the baseline sample. The dependent variables are the change in mean log monthly earnings for the local working-age population (panel (a)), the change in the employment rate for the local working-age population (panel (b)), the return migrant share (panel (c)), the growth in the working-age population (panel (d)), and the change in the share of households receiving remittances (panel (e)). The sample in each regression includes the baseline sample of Mexican municipalities after dropping municipalities from the given Mexican state. All regressions include the baseline controls detailed in the notes of Table 1 and Mexican state fixed effects. Standard errors are clustered at the Mexican state level. The red lines demarcate the baseline estimate of the SC deportee share coefficient from Column 1 in Table 1 for panel (a), column 3 of Table 1 for panel (b), column 1 of Table 2 for panel (c), column 4 of Table 2 for panel (d), and column 1 of Table 3 for panel (e).

Figure (A5) Distributions of Skill Dissimilarity and Occupational Segregation

(a) skill dissimilarity



(b) occupational segregation



Notes: This figure shows the distributions of skill dissimilarity and occupational segregation between return migrants and locals across the baseline sample of municipalities with non-missing measures in 2010.

A.5 Additional tables

Table (A1) Identification Checks

Dependent Variable:	Economic Census Outcomes						
	Δ log mean earnings		Δ log hourly wage		Δ employment rate		Δ homicide rate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SC Deportee Share	0.008	0.034	0.020	0.046	0.006**	0.008	-4.449
	(0.025)	(0.029)	(0.032)	(0.036)	(0.002)	(0.006)	(7.935)
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employment ≥100		Yes		Yes		Yes	
Dep. Var. Mean	0.124	0.121	0.462	0.444	0.044	0.044	14.790
Dep. Var. St. Dev.	0.751	0.616	0.856	0.713	0.091	0.091	544.758
Observations	2,119	1,764	2,119	1,764	2,318	1,779	2,318

Notes: This table shows results from estimating equation 3 where the dependent variable is the change in an outcome in a period before SC was established. The dependent variables are the change from 2003 to 2008 in log earnings per worker (columns 1 and 2), the change in log hourly wages (columns 3 and 4), the change in the employment rate (columns 5 and 6), and the change in the homicide rate (column 7). The sample in each column includes the baseline sample of Mexican municipalities with a non-missing value for the dependent variable. In columns 2, 4, and 6, the sample is further restricted to municipalities where at least 100 workers were employed (and thus reported wages) in the 2004 and 2009 Mexican Economic Censuses to ensure reliable estimates of average earnings and wages in the two periods. All regressions include the baseline controls detailed in the notes of Table 1 and Mexican state fixed effects. Standard errors are clustered at the Mexican state level. Significance levels are denoted by $^{*}p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table (A2) Alternative Approaches to Inference

	(1) Δ log earnings	(2) Δ employment rate	(3) return share	(4) working-age pop. growth	(5) Δ share receive remittances
SC Deportee Share	-0.007***	0.002	0.149***	0.602**	0.525**
	(0.007)	(0.121)	(0.000)	(0.043)	(0.027)
Adão et al. (2019)	0.000	0.000	0.000	0.000	0.000
Conley (1999) 75 km	0.000	0.268	0.000	0.068	0.000
Conley (1999) 100 km	0.000	0.177	0.000	0.062	0.005
Colella et al. (2020) 75 km	0.035	0.253	0.000	0.067	0.015
Colella et al. (2020) 100 km	0.020	0.243	0.000	0.067	0.008
Wild cluster bootstrap	0.008	0.321	0.000	0.092	0.030
State FEs	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes
Dep. Var. Mean	-0.017	-0.038	1.088	8.522	0.322
Observations	2,318	2,318	2,318	2,318	2,318

Notes: This table re-estimates the baseline regressions using different methods for conducting inference and reports the corresponding p-values for each approach. The p-values when using the baseline inference method (standard errors clustered at the Mexican state-level) are in parentheses. All regressions include the baseline controls detailed in the notes of Table 1 and Mexican state fixed effects.

Table (A3) First Stage: Return Migration and Working-Age Population Growth

	return share		working age pop. growth	
	(1)	(2)	(3)	(4)
SC Deportee Share	0.142***		0.674**	
	(0.026)		(0.277)	
SC Days Active Share		0.226***		1.003***
		(0.064)		(0.347)
State FEs	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes
Return Migrant Share (2000-05)	Yes	Yes		
Working-Age Pop. Growth (2000-05)			Yes	Yes
Dep. Var. Mean	1.088	1.088	8.522	8.522
Observations	2,318	2,318	2,318	2,318

Notes: This table shows results from the corresponding first stage regressions of the IV results in columns 2, 3, 5, and 7 of Table 7. See the notes of Table 7 for a description of the controls. Standard errors are clustered at the Mexican state level. Significance levels are denoted by $^{*}p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table (A4) Differential Selection of Return Migrants on Demographic Characteristics

					No High School Deg.			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Age	Male	Single	No High School Deg.	No Schooling	Some Primary	Primary	Lower Secondary
Return migrant	-1.168***	0.215***	-0.008**	0.021**	-0.025***	-0.019***	0.025***	0.041***
	(0.131)	(0.006)	(0.004)	(0.009)	(0.002)	(0.002)	(0.007)	(0.006)
Municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep. Var. Mean	38.514	0.475	0.210	0.646	0.044	0.096	0.193	0.314
Observations	5,881,685	5,881,685	5,881,685	5,881,685	5,881,685	5,881,685	5,881,685	5,881,685

Notes: This table shows results from regressions of demographic characteristics on a return migrant indicator among individuals in the baseline sample of municipalities between the ages of 18 and 64 that were not in school at the time of the survey in the 2015 Intercensal Survey. The dependent variable in column 1 is continuous. The dependent variables in columns 2-8 are indicators. In columns 5-8, the dependent variable is equal to 1 if the corresponding category is the individual's highest degree of schooling. For example, if an individual's highest completed level of education is primary, then "Primary" is equal to 1 and "Some Primary" (the category below "Primary") and "Lower Secondary" (the category above "Primary") are equal to 0. All regressions include municipality fixed effects. Observations are weighted by the person weights (perwt in IPUMS). Standard errors are clustered at the municipality level. Significance levels are denoted by $^{*}p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table (A5) Differential Selection of Return Migrants on Occupational Choice

	(1) Agricultural/ fishery	(2) Clerical support	(3) Crafts/ trades	(4) Elementary occ.	(5) Legislators, officials, managers	(6) Operators, assemblers	(7) Professionals	(8) Service/ sales	(9) Technicians, assoc. professionals
Return migrant	0.039***	-0.018***	0.051***	-0.017***	-0.005**	0.003	-0.033***	0.013**	-0.030***
	(0.004)	(0.003)	(0.005)	(0.005)	(0.003)	(0.007)	(0.003)	(0.006)	(0.002)
Municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep. Var. Mean	0.090	0.063	0.164	0.163	0.028	0.122	0.085	0.212	0.062
Observations	3,255,390	3,255,390	3,255,390	3,255,390	3,255,390	3,255,390	3,255,390	3,255,390	3,255,390

Notes: This table shows results from regressions of occupation indicators on a return migrant indicator among employed individuals in the baseline sample of municipalities between the ages of 18 and 64 that were not in school at the time of the survey in the 2015 Intercensal Survey. All regressions include municipality fixed effects. Observations are weighted by the person weights (perwt in IPUMS). Standard errors are clustered at the municipality level. Significance levels are denoted by $^{*}p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table (A6) Differential Selection of Return Migrants on Employment Types

	Self-Employed			
	(1) Any Self-Employed	(2) Working Own Account	(3) Employer	(4) Wage or Salary Worker
Return migrant	0.039***	0.029***	0.010***	-0.048***
	(0.004)	(0.004)	(0.003)	(0.005)
Municipality FEs	Yes	Yes	Yes	Yes
Dep. Var. Mean	0.226	0.195	0.031	0.750
Observations	3,219,504	3,219,504	3,219,504	3,219,504

Notes: This table shows results from regressions of employment type indicators on a return migrant indicator among employed individuals in the baseline sample of municipalities between the ages of 18 and 64 that were not in school at the time of the survey in the 2015 Intercensal Survey. All regressions include municipality fixed effects. Observations are weighted by the person weights (perwt in IPUMS). Standard errors are clustered at the municipality level. Significance levels are denoted by $^{*}p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$.