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Migration Opportunities and Human Capital Investments

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Abstract

We examine how shocks to migration opportunities affect schooling outcomes in origin communities. We focus on the migration between Mexico and the United States, and exploit the expansion of the Secure Communities program in the US—a federal data-sharing program that substantially increased the risk of deportation for illegal migrants—as exogenous shock to the attractiveness of illegal migration. Our results suggest that the Secure Communities program increased attendance, enrollment, and educational attainment in municipalities that had stronger migration-network links with counties in the US that adopted the program early-on relative to municipalities that had ties with US counties that introduced the policy somewhat later. These results are consistent with the interpretation that the Secure Communities program raised the returns to education for prospective migrants by making low-skill migration to the US less attractive.

JEL classification

I26, J22, O15

Keywords

migration, human capital, Mexico

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1 Introduction

International migration has risen substantially over the last decades both in absolute numbers and as a fraction of the world's population (UNDESA, 2024), with strong implications for destination and origin countries. This dramatic increase has been accompanied by a draft of new migration restrictions imposed around the world, with the aim of restricting migration entirely or regulating the inflow of migrants more selectively (de Haas et al., 2018; DEMIG, 2015; Schreier et al., 2023). As restrictions on migration become more widespread, the question arises as to how these will change migration flows in number and composition, and what their effects will be on the communities that have grown to rely on international migration for their livelihoods.

In origin countries, the welfare effects of (restricting) international migration largely depend on the extent to which migration affects human capital acquisition in the long-run. Early works in the migration literature have hypothesized that the drain of qualified workers, the 'brain drain', hampers economic development in origin countries (Bhagwati and Hamada, 1974; McCulloch and Yellen, 1977; Miyagiwa, 1991; Haque and Kim, 1995). By contrast, Stark et al. (1997) have proposed the opposite effect: As migration is selective on skills, migration prospect increase returns to education, leading to higher human capital accumulation, and thus a 'brain gain' in home communities. Existing empirical evidence largely points to beneficial effects of international migration on human capital accumulation, and development. As migrants send remittances home (Bollard et al., 2011; Yang, 2011; Docquier and Rapoport, 2012), more money can be spent on schooling (Yang, 2008; Dinkelman and Mariotti, 2016; Theoharides, 2017).[1] In addition, the skill-selectivity of migration seems to incentivize human capital accumulation (Beine et al., 2008; Batista et al., 2012; Shrestha, 2017; Chand and Clemens, 2023).

In contexts where migration is low-skill, however, the positive income effects of remittances inflows may be counteracted by a negative incentive effect of migration prospects that works through a reduction in the wage-returns to education (see e.g. McKenzie and Rapoport, 2011, for an early conceptualization of this mechanism). Whether migration opportunities are welfare enhancing in such contexts, depends to substantial degree on

[1] Remittances —-defined as household income received from abroad— have risen immensely over the last decades. In the decade preceding the 2007 financial crisis, the average real annual growth rate of remittances was 12.9% (Yang, 2011). In 2015 alone, remittances to low- and middle-income countries amounted to 424.8 Billion US$. This is almost three times the amount of Official Development Assistance received by these countries in the same time period (152.4 Billion US$), and more than half the value of net inflows of Foreign Direct Investment (641.2 Billion US$). And in contrast to private capital flows, remittances exhibit stability and even counter-cyclicality in the wake of economic crises, such as the global financial crisis of 2007-2009 (Yang, 2011).

which of these two effects dominates with respect to educational attainment. In countries in which domestic returns to education are steep, the return to education effect may well dominate.[2]

This paper investigates the effects of restrictive migration policies on educational outcomes in a context in which migration is typically low-skill and highly seasonal. Our empirical strategy exploits the roll-out of the Secure Communities program throughout the United States (US), which greatly increased the cost of illegal migration to the country. The Secure Communities program is a federal program that implemented automatic data sharing between local law enforcement and federal immigration enforcement agencies, and substantially increased the number of deportations from the US to Mexico in the time-period between 2008 and 2014 (Transactional Records Access Clearinghouse, 2024). By raising the risk of deportation, the Secure Communities program also raised the cost of —and arguably reduced the return to— illegal migration to the US.

For Mexicans, migration to the US (especially seasonal short-term migration) is a highly remunerative occupation that requires relatively few skills. Arguably, such a lucrative outside option flattens the returns to education in Mexico, as the income of low-skill migrants is often more competitive than the income of skilled workers within Mexico. By making low-skill migration to the United States less attractive, then, the Secure Communities program implicitly increased the returns to education for young Mexicans, and could thus incentivize higher investments in education. On the other side, an increase in the number of deportations from the US may also weaken existing migration links, lead to reduced remittance inflows and more competition on local labor markets.

We explore the effects of the Secure Communities program (henceforth SC program) on schooling in Mexico by leveraging two crucial sources of variation. First, the roll-out of the Secure Communities program was staggered in the US, with some counties introducing the program relatively early and others following later (see e.g. East et al., 2023; Alsan and Yang, 2024). Second, migration from Mexico to the US tends to follow pre-existing networks, and these vary by region (Munshi, 2003; Woodruff and Zenteno, 2007; McKenzie and Rapoport, 2010; Allen et al., 2018). In other words, migrants from specific municipalities in Mexico are more likely to migrate to certain US counties than to other counties, or than migrants from other municipalities. We construct the geographical patterns of pre-existing migration networks using confidential records on the place of birth and place of residence of about 3.6 Million Mexicans living in the US. With these network

[2] One empirical study has documented a positive combined effect of increased remittances and altered migration prospects in a low-skilled migration context (i.e. Dinkelman and Mariotti, 2016, in Malawi in the 1970s). However, Malawi had a GDP per capita of about 63$ p.c. at the time, suggesting that domestic returns to education were extremely low.

links, we predict which municipalities should be affected by the Secure Communities program at a specific point in time.

We construct our main outcomes from the ENOE data, Mexico's quarterly labor-force survey, for the time period 2005 to 2012. From this data, we can construct not only enrollment and attainment figures (incl. grade progression), but also weekly attendance, as well as labor-market related outcomes at the individual and household level. While measuring outcomes from survey data as opposed to administrative records limits the statistical power of our analysis, the ENOE allows us to follow individuals and households over time, and to examine survey attrition as well as migration. Our main analysis restricts the sample to the municipalities with the highest migration rates as observed in the 2000 Population census, arguing that shocks to migration should be felt most strongly in places that have higher reliance on international migration to begin with.

Consistent with the notion that the SC program implicitly increased the returns to education for adolescents in Mexico, we find evidence that the roll-out of the Secure Communities Program increased school attendance, enrollment and educational attainment in Mexican municipalities that were more strongly exposed to the Secure Communities program (i.e. that had stronger migration networks with counties that adopted the program early-on). This effect is concentrated among adolescents aged 15 to 17, who also display the highest drop-out rates of all age groups, and thus are most susceptible to changes in the wage-returns to education. Conversely, we find no negative effects on attendance, enrollment, or attainment among youths aged 12-14 or 18-20. The absence of effects among younger children suggests that households on average were able to protect schooling from potential negative income effects that would arise from a decline in the inflow of remittances.

This paper contributes to two strands of literature. First, to the literature that links human capital investments and migration (Yang, 2008; Beine et al., 2008; McKenzie and Rapoport, 2011; Gibson and McKenzie, 2011; Batista et al., 2012; Dinkelman and Mariotti, 2016; Shrestha, 2017; Chand and Clemens, 2023), we contribute novel evidence showing that a negative shock to migration opportunities can indeed incentivize investments in education in contexts in which international migration is low-skill.[3] Most closely related to our work is McKenzie and Rapoport (2011), who show that Mexican provinces that

[3]In work simultaneous to ours, Caballero (2022) uses the same shock to identify the extent to which investments in child human capital respond to reduced remittance inflows. However, in contrast to this paper, her outcome variable is collected from administrative sources, and does not allow distinguishing between increased grade progression (as retention rates are very high in lower-secondary school in Mexico) and dropouts. She finds negative effects on the enrollment of children in lower-secondary school, which she largely attributes to a decline in remittances incomes. As we document in this paper, the negative effects on enrollment by grade in lower-secondary school can be explained by improved grade-progression.

are relatively more dependent on US migration also display lower educational attainment using historical migration rates as an instrument. Our paper, in turn, focuses on high-migration localities, and examines the effect of a policy-shift that alters the future attractiveness of migration as an income-generating activity.

Second, we contribute to a literature that investigates the effects of the Secure Communities program. Most of this literature has focused on outcomes in the US, and has highlighted negative labor market effects (East et al., 2023), reduced care availability (Almuhaisen et al., 2024; Ali et al., 2024), and reduced the take-up of social benefits among legal migrants (Alsan and Yang, 2024). A growing number of papers investigate the spillover effects of the Secure Communities program (and the resulting rise in deportations) in Mexico (Caballero, 2022; Pearson, 2023; Medina-Cortina, 2023; Osuna Gomez and Medina Cortina, 2025). Interestingly, these papers all use slightly different empirical strategies to estimate the effects of the Secure Communities program. As we demonstrate in this paper, the choice of empirical strategy does not meaningfully affect the results, once the sample is restricted to municipalities with high migration propensities.

The remainder of this paper proceeds as follows. In Section 2, we present some background information about the Secure Communities program. Section 3 introduces the conceptual framework, Section 4 the data and the empirical approach. Section 5 presents the results, and Section 6 concludes.

2 Background: The Secure Communities Program

The Secure Communities program is a federal data-sharing program, in which fingerprints that are collected by local law enforcement agencies are automatically shared with Immigration and Customs Enforcement (ICE), the federal agency responsible for immigration enforcement. The fingerprints are then checked against immigration databases in real-time, and depending on the result, immigration officials decide whether to issue a detainer request (which is carried out by local law enforcement). With the introduction of the SC program, any encounter by an undocumented migrant with local law enforcement (be it in traffic, or because a person was victim of a crime) could thus result in imminent deportation. In the time period 2008 to 2014, almost 400,000 deportations have been made in connection with the SC program, the vast majority of those (74%) concerned Mexican citizens (Transactional Records Access Clearinghouse, 2024).

The program was rolled-out throughout the United States between 2008 and 2014, replaced by the more narrowly focused Priority Enforcement Program (PEP) in July 2015, and then reintroduced in January 2017.[4] Initially, participation in the SC program was conditional on a memorandum of understanding between the ICE and State Identification Bureau officials. From 2011 onward, this requirement was revoked. However, throughout the time period, activation of the program was determined at the county level due to various technological constraints. The first counties to join the SC program were those along the US-Mexico border, those with a higher Hispanic population and those that had a 287(g) agreement with ICE (Cox and Miles, 2013), and participation subsequently expanded to the entire country.

Meanwhile, the increased entanglement of local law enforcement with immigration enforcement was met with substantial resistance at the local level. Critics of the SC program have argued that this program severely reduces trust in local law enforcement, creates a climate of fear, and reduces public safety because victims of migration backgrounds are less likely to seek support from local law enforcement (Kubrin, 2014). Cities and communities across the country decided to revoke the collaboration with ICE and not honor detainer requests, giving themselves the status of *sanctuary cities* (Villazor, 2008; Chen, 2016). In 2014, the state of California passed a law that established sanctuary at the state level, forbidding law enforcement throughout the country from collaborating with ICE.

Substantial evidence documents the dramatic impact the SC program had on migrant communities, and that information about the risk of deportation spread quickly (Alsan and Yang, 2024). Given that previous work has documented the spread of information through migrant networks, information as impactful as the implementation of the SC program would likely have spread equally quickly to the diaspora.

3 Conceptual Framework

To understand how the Secure Communities Program might affect schooling in Mexico, we conceptualize the schooling decision from the perspective of the student who is at the end of compulsory schooling.[5] At that point in time, the student needs to decide whether to stay in school and obtain the next higher level of schooling ($s = 1$) or drop out after completing compulsory schooling ($s = 0$).

Earnings are governed by a (simplified) Mincerian wage function w_s^d, which depends

[4] While similar in nature to the SC, the PEP focused detainer requests on convicted criminals and other individuals who were perceived as posing a danger to public safety (Immigration and Customs Enforcement, 2024).

[5] The framework is sufficiently general to capture the trade-offs faced at any point in time while the student is still in school (continuous years of schooling).

on the individual's location of employment d and level of schooling s as given by:

$$\ln(w_s^d) = a^d + \gamma^d s. \tag{1}$$

In eq. (1), a^d is the base wage in location $d \in [US, MX]$, and γ^d is the (location-specific) wage-return to completing an additional level of schooling. Upon entering the labor market, the individual has to decide every period whether to migrate or not. We assume all migration is seasonal for the sake of simplicity. The probability of migrating p is governed by the cost of migrating c and the probability of being deported ζ, with $\partial p(c, \zeta)/\partial c < 0$ and $\partial p(c, \zeta)/\partial \zeta < 0$. Taking into account the risk of deportation, the expected annual income of the low-skilled individual is: $p[(1-\zeta)a^{US} + \zeta a^{MX} - c] + (1-p)a^{MX}$.

We assume that high-educated individuals always migrate legally ($\zeta|_{s=1} = 0$), but that $\gamma^{MX} > a^{US} - a^{MX} + \gamma^{US} - c$, such that the high-educated individual always finds it optimal to stay in Mexico, even if they would not face the risk of deportation. We further assume that the period-utility of an individual is given by $u = \ln(y)$, with y being the individual's income, and that everyone works full-time. This allows us to rewrite utility to $u = \ln(w)$. The student expects to live forever, and discounts at rate ρ.

The value of dropping out is reflected by the net present value of expected lifetime income for the low-educated individual ($s = 0$). We assume that the low-skill individual remains in Mexico in the first period (as they are of working-age, but not yet legally of age), and migrate from then onwards with migration probability p. The resulting net present value takes the form:

$$V^l = a^{MX} + \sum_{t=2}^{\infty} \frac{1}{(1+\rho)^{t-1}} \left[p[(1-\zeta)a^{US} + \zeta a^{MX} - c] + (1-p)a^{MX} \right]. \tag{2}$$

The value of staying in school and completing an additional level is given by the net present value of lifetime income with $s = 1$, and $d = MX$:

$$V^h = \ln(\bar{y}) + \sum_{t=2}^{\infty} \frac{1}{(1+\rho)^{t-1}} [a^{MX} + \gamma^{MX}], \tag{3}$$

where $\bar{y}$ is the income received while studying, such as the support by parents.

A student will be exactly indifferent between continuing in school or dropping out when: $V^l = V^h$. Solving for $\bar{\rho}$, the threshold level of ρ that makes the student exactly indifferent between staying in school and dropping out, gives the expression:

$$\bar{\rho} = \frac{\gamma^{MX} - p[(1-\zeta)a^{US} + (\zeta-1)a^{MX} - c]}{a^{MX} - \ln(\bar{y})} \equiv \frac{RetS}{OppC}. \tag{4}$$

The student draws their discount rate ρ from a distribution with density $f(\rho)$. They will invest in more education if $\rho \leq \bar{\rho}$, and not otherwise. Eq. (4) illustrates the trade-offs involved in deciding on an extra level of education: As the implicit return to schooling $RetS$ increases, the threshold $\bar{\rho}$ increases, such that more students find it optimal to stay in school for an extra period. As the opportunity cost of schooling increase (for example because the base wage in Mexico a^{MX} increases, or because support while studying declines), more students find it optimal to drop out. Taking the first order differential of $\bar{\rho}$ with respect to ζ illustrates the effect of an increase in deportation risk (e.g. through the introduction of Secure Communities) on schooling:

$$\frac{\partial \bar{\rho}}{\partial \zeta} = \frac{-1}{a^{MX} - \ln(\bar{y})} \left[\frac{\partial p}{\partial \zeta}[(1-\zeta)a^{US} + (\zeta-1)a^{MX} - c] + p(\zeta)(a^{MX} - a^{US}) \right]. \tag{5}$$

Eq. (5) shows that $\bar{\rho}$ is increasing in ζ. To see this, note that $a^{MX} - \ln(\bar{y}) > 0$, $(1-\zeta)a^{US} + (\zeta-1)a^{MX} - c > 0$, and $p(\zeta) > 0$ by assumption, while $\partial p / \partial \zeta < 0$ and $a^{MX} - a^{US} < 0$. This implies that the second part of the right hand side of eq. (5), $\frac{\partial p}{\partial \zeta}[(1-\zeta)a^{US} + (\zeta-1)a^{MX} - c] + p(\zeta)(a^{MX} - a^{US})$, is negative (as is the first part). An increase in the deportation risk will therefore increase the fraction of individuals that are just patient enough to complete an extra level of schooling.

4 Data and Empirical Approach

4.1 Data

In order to analyze the research question outlined above, we collect a series of datasets that we merge at the level of the municipality in Mexico.

Secure communities data. From official records, we hand-code the roll-out of the Secure Communities program at the county by month level in the United States between 2008 and 2014 (Immigration and Customs Enforcement, 2014). We also code the expansion of sanctuary cities in the US from ICE's own records (Immigration and Customs Enforcement, 2017). In line with the literature (Alsan and Yang, 2024), we code any county in the US as having revoked their SC program participation, as soon as at least one city becomes a sanctuary city.

Matricula Consular de Alta Seguridad. We construct pre-existing migration networks between the US and Mexico from the Matricula Consular de Alta Seguridad (MCAS) data for 2005–2008. The MCAS cards serve as an identity card and are issued by the Mexican consulates throughout the United States to all Mexican-born individuals who reside in the US. The cards are accepted by a wide range of institutions, making this an attractive document for registered, as well as for unregistered migrants.[6] The data contain the total count of all individuals who were issued an MCAS card between 2005 and 2008, more than 3.6 million individuals, including their place of birth and county of residence. We use these data to establish the strength of network linkages between all pairs of Mexican municipalities and US counties. The average municipality in Mexico has about 1500 recorded migrants in this database, with a substantial amount of variation: While the median municipality has about 419 migrants registered in the database, this number can be as high as 89,000.[7]

Encuesta Nacional de Ocupacion y Empleo (ENOE). The Mexican labor force survey is conducted every three months and each round samples roughly 120,000 households from the entire country. The survey is a rotating panel, which means that each household is interviewed up to five times. This allows tracking individuals over time, as information about the current whereabouts is collected for individuals who were part of the household in the previous round, but who are not currently present. Similarly, for new household members, information about why they joined the household is collected.

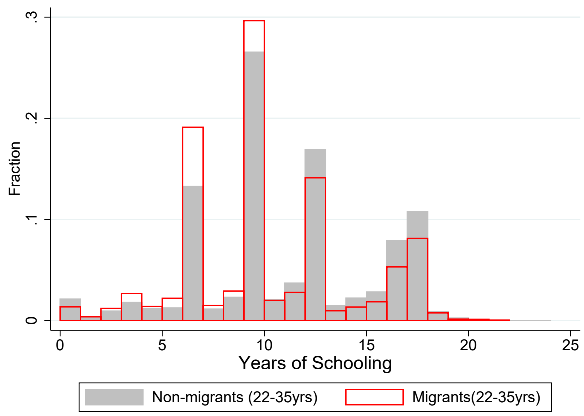
We construct different variables from the ENOE. For children aged 5 and above we obtain information about enrollment, i.e. whether the child is reported to be enrolled in school at the time of interview, and educational attainment, i.e. the highest grade that the child completed, also at the time of interview. For every household member aged 12 and above, we have more detailed time-allocation data (reference period is 7 days prior to the interview). From this, we construct school attendance (which is a dummy that takes the value one if the individual reports to have spent time in school in that period), labor supply, and wages. We use the household roster to construct information about household income, parental education, and whether the household received remittances. Migration variables are constructed from the tracking information.

We also use the ENOE data to corroborate one of the fundamental assumptions of our conceptual framework, namely that migration to the US is relatively low-skill. Exploiting the fact that the ENOE is a rotating panel, we split the sample of adults aged 22-35 into

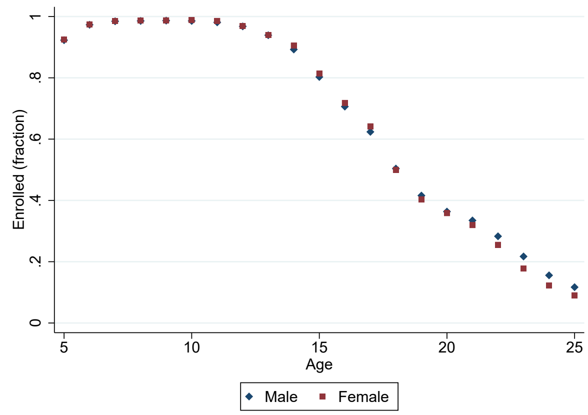
[6] For more information on the MCAS data, see Allen et al. (2018) and Caballero et al. (2018).

[7] There is a strong correlation between the number of individuals from a particular municipality observed in the MCAS database and the fraction of migrants in the population, as collected from the Mexican Population Census, see below.

migrants (any individual household member who was reported to have migrated to the US by their family members in any interview round) and non-migrants. In Figure 1a, we plot educational attainment for migrants and non-migrants, and confirm that migrants have on average lower educational attainment than non-migrants.



(a) Educational Attainment of Migrants and Non-Migrants



(b) Enrollment by age and gender

Figure 1: Migrant Characteristics and Enrollment in Mexico

Censo General de Población y Vivienda. We use the 10.6% subsample of the 2000 Mexican population census, accessed from IPUMS-International, to construct baseline migration rates. The Mexican population census collects from every household the number of former household members who emigrated internationally in the last 5 years.[8] We construct the share of migrants in the total population by counting the number of reported migrants and dividing this number by the sample population in a particular municipality.

All these datasets are merged at the level of the Mexican municipality in borders of 2000 (2,443 municipalities, of which 1,282 are covered by the ENOE at least twice in the relevant time period). We restrict our attention to youths aged 12 to 20 in the main analysis, as this is the age group in which enrollment varies the most (c.f Figure 1b).

[8]While the Mexican population census does not record the destination of the migrant, the fraction of international migrants and the fraction of US migrants in the population should be closely aligned, given that 98% of all Mexican migrants are located in the United States, according to 2010 figures published by the United Nations, Department of Economic and Social Affairs, Population Division, International Migrant Stock: Destination and Origin. Available online: www.un.org/development/desa/pd/content/international-migrant-stock.

4.2 Measuring Migration Shocks

In order to link outcomes in Mexico to the expansion of the Secure Communities program in the US, the first step consists of identifying regional variation in migration networks between Mexico and the United States. The idea is to find out where people from certain regions predominantly migrate to, therewith obtaining variation between municipalities in Mexico with respect to the main destination regions (in the US) of Mexican migrants.

Historically, migration from Mexico to the US followed the three major railway lines that connected the two countries. Due to this process migrants from different Mexican communities have settled in different US destinations. In their destination, migrants established social networks which guide migration flows until today (Munshi, 2003; Woodruff and Zenteno, 2007; McKenzie and Rapoport, 2010; Allen et al., 2018).[9] The predictive power of pre-existing migration networks for subsequent migration flows gives an important angle for causal identification.

Based on the MCAS data, we calculate a migration-network intensity variable q_{jd} for each origin-destination pair. Network intensity is defined as the number of migrants l_{jd} of municipality j that migrate to county d out of the total number of migrants of municipality j that migrate to the US (observed in the MCAS data):

$$q_{jd} = \frac{l_{jd}}{\sum_{d=1}^{D} l_{jd}}. \tag{6}$$

We then combine the information on network intensity with county-level data on the roll-out of the Secure Communities program in the US to construct a measure of how strongly each municipality in Mexico felt the effects of the Secure Communities program at a particular point in time. We define the Secure Communities shock for each municipality by:

$$SCshock_{jt} = \sum_{d=1}^{D} (q_{jd} \times SC_{dt}), \tag{7}$$

where SC_{dt} is an indicator equal to 1 if the Secure Communities program was active in county d at time t. The Secure Communities shock experienced in municipality j at time t is thus the weighted average of the Secure Communities experience of its current migrants. It is important to note that the $SCshock_{jt}$ variable is defined solely on the basis

[9] Similar evidence was produced for the Philippines, where the destination choice of early migrants is shown to strongly determine the subsequent migration decision (and destination choice) of migrants from the same village (Yang, 2008).

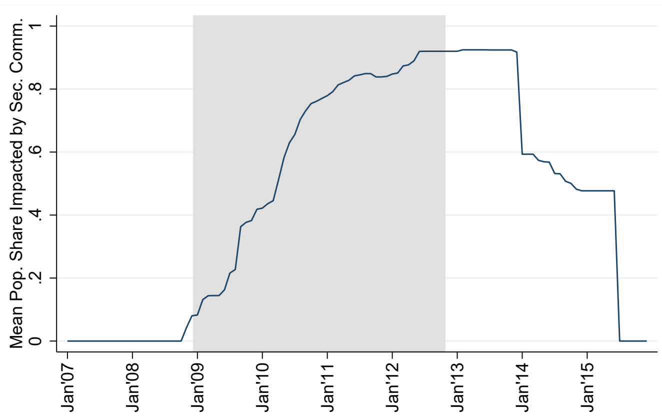


Figure 2: Secure Communities Shock

of migrants' destinations prior to the shock in order to eliminate concerns about reverse causality.

The SC shock is zero for all municipalities prior to the rollout of Secure Communities in December 2008, and takes the value of one as soon as all observed destinations introduce the SC program. The average time trend in the SC shock is depicted in Figure 2. As can be seen, the growth of the SC shock is most pronounced between the years 2009 and 2011, and decelerates thereafter. In 2014, California retracted from the SC program, which reduced the intensity of the SC shock dramatically. The expansion of sanctuary cities explains the continued decline in the SC shock over the course of 2014 before the program was eventually discontinued at the end of 2014.

Figure 3 depicts the temporal and geographical distribution of $SCshock_{jt}$ in Mexico. As can be seen, most variation in the variable occurs between 2008 and 2012. After December 2012 the effect seems to stabilize, as most US counties had introduced Secure Communities by then.

To focus the analysis on the main roll-out period of the SC program, we restrict the observation window for the dependent variables to the time period 2005 to 2012. In the event-study, we extend the observation window for the treatment status by 8 quarters before treatment (2 years), such that the analysis *de facto* extends to December 2014 (when the SC program was discontinued).

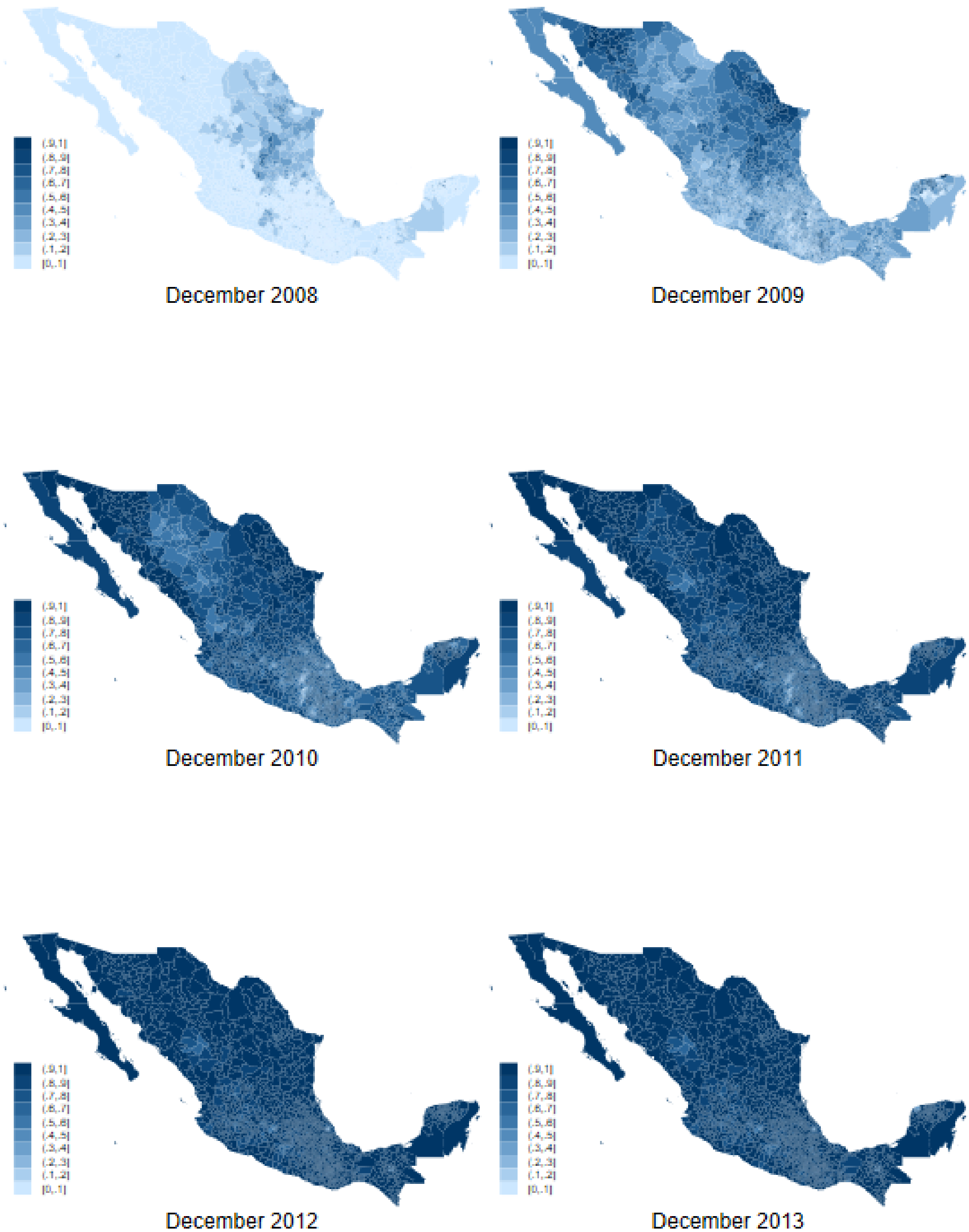


Figure 3: Secure Communities Shock in Mexico

4.3 Sample Construction

One major concern with the approach of tracing shocks through pre-existing migration networks is potential measurement error in the predicted network intensity for any given origin-destination pair. By design, not all migrants can be observed in the MCAS data, such that the network-intensity is always subject to (at least some) measurement error. However, this measurement error is likely non-random, as places that tend to have less migrants also appear in lower numbers in the MCAS data implying that outliers will affect the predicted network intensity more in places with lower migration rates than in places with higher migration rates. This measurement error may systematically bias estimates of the treatment effect, if the treatment effect varies with migration intensity, which is almost certain to be the case.[10]

To address this concern, we restrict our attention to the 611 municipalities in the highest quartile of migration probability in the main analysis, as these municipalities should have the most reliable network data while also being most exposed to the effects of the SC program.[11] We subsequently expand the sample to estimate the effect of Secure Communities separately for the municipalities in the remaining quartiles of migration probability, and test if the effects vary systematically across municipalities with different rates of migration (and potentially measurement error in the network links). Summary statistics for the high-migration as well as the full sample are presented in Table A1.[12] The unit of observation in our analysis is the individual by quarter of interview, and treatment varies at the quarter-by-municipality level. The median number of observations in each quarter-by-municipality group is 18, with a minimum of 1 and a maximum of 236 observations. While this clearly limits the representativity of the data in each quarter-by-municipality group, the analysis at the individual level is unlikely to be exposed to bias from measurement error in the outcome variable, as all units of observation are weighted equally in the analysis.

Of the 611 municipalities, 290 were covered by the ENOE at least twice in the time period 2005 to 2012. One drawback of constructing outcomes from the ENOE is the limited sample size of the survey, which may affect the representativity of our findings. Given ENOE's focus on labor market characteristics, the survey tends to draw a relatively larger

[10]To see this, consider an individual living in a low migration-probability municipality in Mexico. For this individual, any given SC shock should matter less than for a similar individual living in a high migration-probability municipality. For one, remittances income is less likely to change, if less people were relying on migrant networks to begin with. In addition, the change in migration prospects is less likely to affect their own migration decision, if migration is generally less commonly chosen as an income generating strategy.

[11]Migration probability is computed from the 2000 population census, and is the fraction of any municipality's population that is reported to have migrated internationally in the last five years).

[12]Tables A1 to A9, as well as Figures A1 to A7 are available in the Online Appendix.

sample more from urban areas, which also tend to be more affluent (see Table A2). While overall educational attainment seems to be similar between the ENOE sample and the remaining high-migration municipalities, we cannot rule out that the education responses to a migration shock would be different in more rural areas.

4.4 Estimating the Effect of Migration Shocks

Our empirical approach explores how shocks in destination regions (i.e. the Secure Communities program) affect schooling outcomes in the regions of origin. In order to do this, we combine spatial variation in the typical destination of migrants from Mexico within the US with the staggered roll-out of the Secure Communities program in the potential destinations of prospective migrants.

Our most basic specification is a simple difference-in-difference design, in which we regress the outcome of interest on the Secure Communities shock $SCshock_{jt}$, which varies between communities and over time, while controlling for time and municipality fixed effects, as given by:

$$y_{ijt} = \beta\ SCshock_{jt} + \delta_1' X_{ijt} + \delta_2' Z_{j,0} \times t + \zeta_j + \lambda_t + \varepsilon_{ijt}, \tag{8}$$

where y_{ijt} is the schooling outcome (*i.e.* attendance, enrollment and completed years of schooling) observed at the level of the individual, X_{ijt} is a vector of individual (and family) characteristics, which we capture by age-by-gender fixed effects initially, and extend subsequently. $Z_{j,0} \times t$ is the baseline migration probability (computed from the 2000 Population Census) in municipality j (and its square) interacted with time fixed effects, ζ_j is a vector of municipality fixed effects, and λ_t are quarter-by-year fixed effects, and ε_{ijt} is an idiosyncratic error term. Standard errors are clustered at the level of the municipality throughout.

For β to have a causal interpretation, a number of identifying assumptions need to be satisfied. The first identifying assumption is that, in the absence of shocks to migration networks, changes over time in schooling outcomes would have been the same for individuals in treated and untreated municipalities (parallel trends and no correlated time-varying shocks). Given the staggered roll-out of the SC program, the second and third identifying assumptions relate to the absence of heterogeneity in treatment effects between early- and late-treated municipalities, and over time, respectively (as discussed e.g. in Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020). As the SC shock variable is continuous, the fourth identifying assumption concerns the absence of treatment effect heterogeneity by dosage (Callaway et al., 2024).

To test whether the first identifying assumption is plausible, we augment our empirical strategy by an event-study design. Given the continuous nature of our treatment, we follow Schmidheiny and Siegloch (2023) and recover dynamic treatment effects from a distributed lag model that takes the form:

$$y_{ijt} = \sum_{p=-7}^{10} \gamma_p \, SCshock_{ij,t-p} + \delta_1' X_{ijt} + \delta_2' Z_{j,0} \times t + \zeta_j + \lambda_t + \varepsilon_{ijt}, \tag{9}$$

where p is the period (in quarters) since treatment, and all remaining variables are defined as in eq. (8). We cumulate the post-treatment and pre-treatment coefficients away from zero to recover dynamic treatment effects. To be specific, we construct $\beta_p = -\sum_{k=p+1}^{-1} \gamma_k$ if $p \leq -2$, $\beta_p = 0$ if $p = -1$ and $\beta_p = \sum_{k=0}^{p} \gamma_k$ if $p \geq 0$. As outlined in Schmidheiny and Siegloch (2023), this procedure delivers consistent estimates of dynamic treatment effects as long as the treatment effect is proportional to the observed treatment intensity (no treatment effect heterogeneity by dosage). In Section 5.2, we explore strategies that allow relaxing the assumptions regarding treatment effect heterogeneities, and find that our findings are robust.

5 Results

5.1 Main Results

Difference-in-Difference estimates of the effect of the Secure Communities program on school attendance, enrollment and attainment in Mexico are presented in Table 1. For each outcome we present the effect on all youth aged 12-20, as well as disaggregated by three-year age groups: 12-14, 15-17, and 18-20.

The first panel explores effects of the SC shock on school attendance. The point estimate in the full sample is positive but not statistically significant (col. 1). Disaggregating by three-year age group reveals a small positive effect among 12-14 years old adolescents, which is not significant (col. 2), a positive and statistically significant effect (0.098) among individuals aged 15 to 17 (col. 3), and a small negative effect among individuals aged 18 to 20 (col. 4). The point estimate in column 3 suggests that the school attendance rate among individuals aged 15-17 increases by about 10pp (21% over the mean) as they move from having zero network-linked counties with SC activated to all. The second panel reveals that the effect on enrollment follows the same pattern: A small and positive effect overall masks a sizeable and statistically significant effect of about 10pp (17%) in the age

Table 1: Effect of Secure Communities on Schooling Outcomes

Age group	All	12-14	15-17	18-20
	(1)	(2)	(3)	(4)
Dep.var.: School Attendance				
SC shock	0.034	0.028	0.098*	-0.018
	(0.033)	(0.044)	(0.052)	(0.036)
Observations	194909	70276	68791	55839
Dep. var. mean	0.484	0.720	0.462	0.216
Dep.var.: Enrollment				
SC shock	0.027	0.002	0.098*	-0.015
	(0.031)	(0.037)	(0.050)	(0.045)
Observations	194908	70276	68791	55838
Dep. var. mean	0.611	0.897	0.589	0.276
Dep.var.: Years of Schooling				
SC shock	0.083	-0.073	0.448**	-0.158
	(0.160)	(0.134)	(0.197)	(0.327)
Observations	194760	70266	68715	55776
Dep. var. mean	7.606	5.858	8.123	9.173
Clusters	290	289	290	288
Municipality, Time FE	✓	✓	✓	✓
Age-by-gender FE	✓	✓	✓	✓
Migration Share(^{2}) $\times$ Time FE	✓	✓	✓	✓

Note: Standard errors are clustered at the municipality level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

group 15-17, and null-effects in the other age groups. The third panel, finally, shows the effects on completed years of schooling (at the time of interview). Again, the effect pattern is similar: We observe small positive effects for the entire group, which are statistically significant only in the age group 15-17. The magnitude of the coefficient suggests that moving all linked US counties from not participating in the SC program to participating, increases educational attainment in this age group by 0.45 years (a bit under half a year and a 5.5% increase). Its important to note that this estimate should not be interpreted as the effect of SC on final years of schooling: As part of these adolescents are still in school, the coefficient likely captures improved grade progression (which could imply that overall attainment in the long run is unchanged if students simply complete their aspired years of schooling earlier) as well as increased enrollment (and thus long-run improvements in attainment).

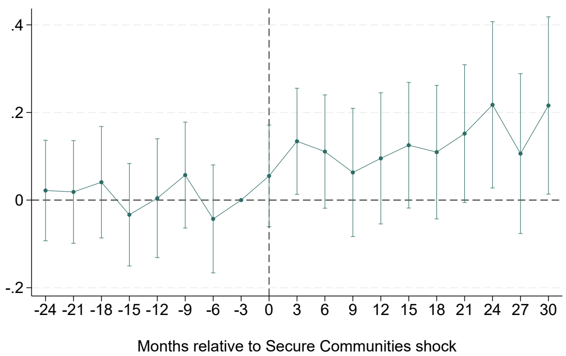
Taken together, our findings suggest that the SC program led to improved schooling outcomes in Mexico among adolescents aged 15-17, with no effects in other age groups. That the effect arises only among students in the age group 15-17 is not surprising given that this is the age at which students typically transition from compulsory schooling (up until grade 9) to voluntary education (grade 10 and beyond). For most students in Mexico, transitioning to high school implies enrolling in a different school, and longer commutes. Consistent with the notion that students' education reaches a natural break point at this age, enrollment declines the fastest in this age group (see Fig. 1b).

Figure 4 shows event-study estimates for the age group 15-17.[13] As can be seen, there is no evidence of pre-trends in any of the outcome variables considered. From these graphs we can identify a number of important differences between the effect timing for the three outcome variables considered. While exposure to the Secure Communities program seems to increase school attendance in this age group immediately, the effects on the other outcomes take about 12 months to materialize. A potential explanation for this could be that enrollment and attainment are quite sticky, i.e. once a student started a given grade they may not dis-enroll even if they effectively stop attending school.

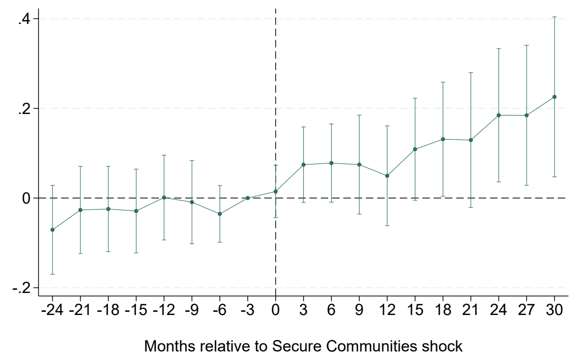
5.2 Robustness

There are three main concerns with the results presented thus far: *First*, the extent to which the results are sensitive to selective attrition or, *second*, the choice of specification. *Third*, whether treatment effects heterogeneities (by treatment intensity or by treatment

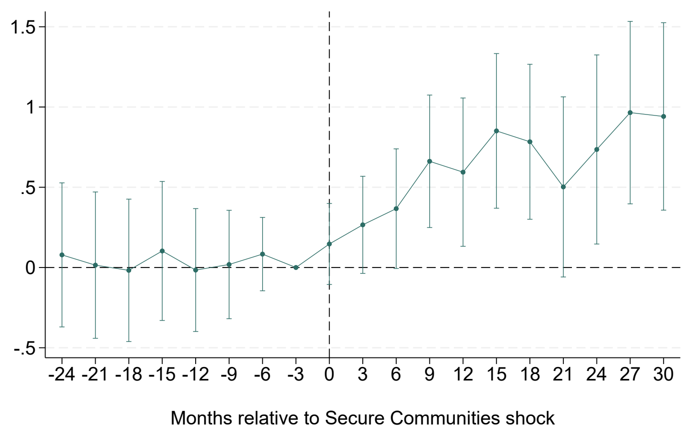
[13]We only show this age group here for brevity, the full results (for all youth aged 12-20, as well as disaggregated by three-year age groups) are available in Figures A1 to A3.



(a) Attendance



(b) Enrollment



(c) Years Schooling

Figure 4: Event-Study Estimates of SC on Schooling Outcomes - Youths 15-17

Note: Graphs depict dynamic treatment effects with 90% confidence intervals computed from eq. (9).

timing) may lead to bias in our estimated effects given the staggered roll-out of the Secure Communities.

To address the first concern, i.e. that our results may simply reflect compositional changes in the population due to internal or international migration responses, we exploit the panel structure of the data, and compute for every individual that was ever sampled and for each of the five survey rounds that the individual should be in the sample: whether the individual has been interviewed, whether the individual is reported by family members to be a domestic migrant, an international migrant, or whether the individual attrited (this can be either because the whole household could not be interviewed, because the individual was away for unexplained reasons or because of death).[14] We then

[14] Note that we can only calculate these outcomes in households that were scheduled to be interviewed at least twice. For the year 2005, we therefore lose about 5% of the sample in this analysis (these are households

investigate whether any of these outcomes vary systematically with the Secure Communities shock. As can be seen in Table 2 there are no statistically significant effects in any of the relevant age groups on the probability of being interviewed, except for the age group 15 to 17. When narrowing down on any of the reasons for non-interview, it becomes apparent that individuals in the age group 15 to 17 are somewhat less likely to be reported as internal migrant, while individuals in the age group 18-20 are somewhat less likely to attrit. This increased probability among 15-17 year old adolescents of remaining at home (and in school) rather than migrating domestically may reflect exactly the mechanisms we have in mind: Individuals in this age group are not generally migrating internationally yet, so after dropping out of school many would choose to migrate domestically for work. The important question is whether this would bias our results with respect to schooling outcomes. In principle, it is hard to imagine that the positive effects can be explained by an increased probability of observing an individual: In this age group, years of schooling among individuals who are ever domestic migrants is lower than among individuals who are never reported as domestic migrants (8.2 vs 8.4 years of schooling). In contrast, the small (and insignificant) negative effect on completed years of schooling in the age group 18-20 could be partly driven by fewer low-skilled individuals attriting from the sample: Individuals in this age group who are ever reported to attrit have lower educational attainment than individuals who are never found to attrit (9.1 vs 9.2 years of schooling, respectively).

We also examine the robustness of our findings with respect to alternative specifications and sample composition. In Table A3, we show that the point estimates are largely unchanged with the step-wise inclusion of the controls, and are also unchanged when adding additional household-level controls, such as the highest level of education observed among adult household members or the labor income in the household. In Table A4, we show that changing the sample to include more or fewer years around the expansion of the SC program does not change the main results. In addition, we show that the results are robust alternative constructions of the SC shock: Focusing exclusively on the SC status of a county (and ignoring the expansion of sanctuary cities) does not change the results. Likewise, the results are similar, although different in magnitude by construction, if the SC index is additionally weighted by the initial migration share in each municipality. In Table A5, finally, we use the same weighted SC index, but expand the analysis to all municipalities covered by the ENOE. Again, the results are robust.

To address concerns regarding bias in the OLS estimator due to the endogenous rollout of the SC program in the US, we dichotomize the SC shock variable at the median shock

who appeared in the first quarter of 2005 but had already completed four rounds of interviews in 2004)

Table 2: Effect of Secure Communities on Attrition

Age group	All	12-14	15-17	18-20
	(1)	(2)	(3)	(4)
Dep.var.: Individual was interviewed				
SC shock	0.035	0.012	0.060*	0.039
	(0.024)	(0.024)	(0.034)	(0.036)
Observations	225303	76599	79129	69572
Dep. var. mean	0.865	0.917	0.869	0.803
Dep.var.: Individual is abroad				
SC shock	0.002	0.002	-0.001	0.002
	(0.006)	(0.004)	(0.007)	(0.014)
Observations	225303	76599	79129	69572
Dep. var. mean	0.012	0.003	0.010	0.024
Dep.var.: Individual is away domestically				
SC shock	-0.022	-0.011	-0.053*	0.002
	(0.017)	(0.015)	(0.028)	(0.030)
Observations	225303	76599	79129	69572
Dep. var. mean	0.070	0.029	0.072	0.114
Dep.var.: Individual attrited				
SC shock	-0.015	-0.003	-0.006	-0.043*
	(0.016)	(0.020)	(0.020)	(0.022)
Observations	225303	76599	79129	69572
Dep. var. mean	0.053	0.051	0.049	0.059
Clusters	290	289	290	288
Municipality, Time FE	✓	✓	✓	✓
Age-by-gender FE	✓	✓	✓	✓
Migration Share$(^2)$ $\times$ Time FE	✓	✓	✓	✓

Note: Sample are all individuals between the ages 12-20 that ever appear in the household roster, irrespective of whether they were interviewed in that particular round or not. An individual appears in the roster, if s/he was interviewed in the previous round or if s/he is currently part of the household but not present. Standard errors are clustered at the municipality level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

intensity (0.11), and estimate treatment effects using the imputation estimator presented in Borusyak et al. (2024).[15] As shown in Figure A4, the results (shown for enrollment and educational attainment among individuals aged 15 to 17) are robust.

5.3 Generalizability

In light of the differences between our results and those in other works (in particular Caballero, 2022), the question arises whether the positive effects observed here are generalizable to the Mexican population as a whole. As mentioned previously, one of the challenges in identifying the effect of the SC program on outcomes in Mexico is that the effect should vary with migration intensities. It seems plausible to assume that the effect of the Secure Communities program would only be felt in places where a reasonable share of the population actually migrates to the US. If migration probability is low, why would a shock to migration opportunities affect outcomes? At the same time, measurement error in the geographical patterns of migration networks is likely to be most pronounced in places that have a low probability of migrating (and have fewer entries in the MCAS data). If this measurement error is random, then treatment effects should be biased towards zero, and no meaningful trend breaks should be discernible.

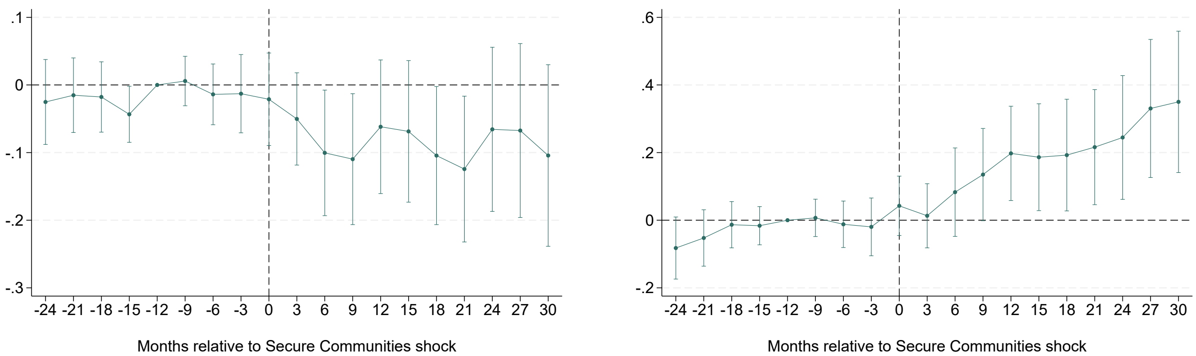
To understand whether treatment effects indeed vary with migration intensity, we separately estimate the effect of the SC shock in each of the remaining quartiles of migration intensity. In the remaining three subsamples of Mexican municipalities (split by migration share quartiles), we find little evidence of any consistent effects of the SC shock on education outcomes (c.f. Table A6).[16] Out of the 36 estimated coefficients only two are statistically significant, suggesting marginal declines in school attendance among 12-14 year old adolescents in municipalities in the second quartile of migration intensity (which is not translating into declines in enrollment or educational attainment), and small increases in enrollment among 18-20 year old individuals in municipalities in the lowest quartile of migration intensity (again without any detectable effects on any of the other schooling outcomes). Evidence from the event-study estimates (Figures A5 to A7) confirms that there are no systematic trend breaks in schooling outcomes around the SC shock in any municipalities except those with the largest migrant populations.

Though indicative of the caveats involved in estimating the effect of migration shocks

[15] de Chaisemartin and D'Haultfœuille (2024) propose an estimator that can handle multi-valued and incremental treatment. Unfortunately, their estimator is unable to handle well the data structure of this paper (unbalanced panel with many gaps).

[16] We split the sample rather than interacting the $SCshock_{jt}$ variable with migration share, in order to avoid comparing municipalities that are on different time trends. Figures A5 to A7 show dynamic treatment effects from eq. (9).

in low-migration populations, the effects in the remaining municipalities still fall short of the large negative effects on middle-school enrollment reported by Caballero (2022). We test if our results are consistent with theirs once we compute grade-specific enrollment, rather than age-specific enrollment.[17] Indeed, as shown in Figure 5, panel (a), we find negative enrollment effects when focusing on middle-school (lower secondary) enrollment although our effects are somewhat noisier (which is not surprising given that we compute enrollment from survey data). In panel (b), we compute high school (upper secondary) enrollment, and find an overall positive effect of Secure Communities, which is consistent with our results presented above.



(a) Middle-school (b) High-school

Figure 5: Event-Study Estimates of SC on Enrollment (log) - Municipality Aggregates

Note: Graphs depict dynamic treatment effects with 90% confidence intervals computed from eq. (9). Samle size: , number of municipalities:

Taken together, these results highlight that the focus on enrollment as outcome variable may lead to misleading conclusions about the true schooling effect of policies in populations with high retention rates (in our sample, about 25% of the individuals enrolled in grade 9 are 16 or older, while the correct age-for-grade would be 15). They also highlight the importance of accounting for treatment effect heterogeneities in populations that are exposed to migration shocks to a very different extent.

[17] We construct grade-specific enrollment from the ENOE data, by coding every individual as being enrolled in a particular grade, if they are currently enrolled and their highest level of education reported is that grade minus one. In these regressions, we use the full sample of ENOE municipalities in the time period 2005-2014 (1,465), and estimate eq. (9) with the (log) population in middle school/ high school age instead of age-by-gender fixed effects in X_{ijt}. Middle school corresponds to grades 7-9, high school to grades 10-12.

5.4 Mechanisms

In the results presented above, we find that the roll-out of the SC program throughout the US improved schooling outcomes in Mexico in the age group 15 to 17, while leaving adolescents of other age groups largely unaffected. While these results are consistent with a decline in the perceived attractiveness of seasonal migration, and an increased focus on the Mexican labor market where returns to education are steeper, other mechanisms may explain the observed effects. To explore the underlying mechanisms more carefully, we first assess whether heterogeneities in the effect of the SC program on schooling outcomes are consistent with the hypothesized mechanism. We then investigate the empirical support for alternative explanations.

The heterogeneities presented in Table 3 suggest that the positive effect of the SC program concentrates in households with relatively lower educational background (columns 1-2). Conversely, we find no statistically significant heterogeneities by the dependency ratio in the household (columns 3-4). These findings underscore the plausibility of the returns to education mechanism. For high education households, illegal migration restrictions should matter relatively less, while households with a higher dependency ratio would face tighter budgets and be more susceptible to negative income effects. The migration background of households, finally, explains some of the variation in treatment effects, with adolescents from migrant households generally experiencing slightly larger effects (columns 5-6). However, the difference between both groups is modest, highlighting the importance of spillovers into non-migrant populations.[18]

When disaggregating the sample by gender, we find that the effect of the SC shock concentrates among females, despite these having better schooling outcomes to begin with (c.f. Table A7). This difference may be driven by the fact that women and girls tend to display higher risk aversion, such that a worsening in the conditions of illegal migration would affect them more strongly.

We proceed by investigating the support for alternative explanations. Potentially, the roll-out of the SC program may have led to more return migration, which could increase the number of adults in the household, and reduce pressure on adolescents to contribute to family incomes. However, we do not find any evidence that the household composition changed due to the SC program for adolescents in the age group 15-17. We cannot find any statistically significant effects on the number of returnees, the number of new migrants, the receipt of remittances, or the household composition, as measured by the highest level of education of adult members or the dependency ratio (c.f. Table A8). [19]

[18] In Table A8, we show that none of the interaction variables are systematically correlated with the SC shock.

[19] This does not imply that the SC program did not deter migration, to the contrary, when focusing on in-

Table 3: Heterogeneous Effects of Secure Communities on Schooling, 15-17 years

Heterogeneity by	Household educ		Dep. ratio		Migrant hh	
	(1) Enroll	(2) Yrs Schooling	(3) Enroll	(4) Yrs Schooling	(5) Enroll	(6) Yrs Schooling
SC shock	0.091*	0.413**	0.091	0.317	0.097*	0.431**
	(0.049)	(0.180)	(0.059)	(0.228)	(0.051)	(0.197)
Highest years schooling (adult in hh)	0.037***	0.157***				
	(0.001)	(0.006)				
SC shock × Yrs Schooling	-0.002	-0.036***				
	(0.002)	(0.010)				
Dependency ratio			-0.189***	-0.918***		
			(0.030)	(0.114)		
SC shock × Dep. ratio			0.020	0.269		
			(0.051)	(0.207)		
Any migrant in hh					-0.051***	0.009
					(0.015)	(0.056)
SC shock × Any migrant					0.022	0.212*
					(0.037)	(0.125)
Observations	68453	68386	68791	68715	68791	68715
Clusters	290	290	290	290	290	290
Interaction var. mean	-0.119	-0.119	0.557	0.557	0.105	0.105

Note: The dependent variable in the odd columns is whether an individual is enrolled in school, and in the even columns the highest years of schooling an individual has attained. The variable *Highest years schooling* is the educational attainment of the adult (>18) household member with the highest years of schooling, and centered at the mean. *Any migrant in household* is an indicator variable that takes the value one if any household member is ever reported to have returned from abroad or to have emigrated. *Dependency ratio* can take any value between 0 and 1 and is the fraction of household members that are below 18 or above 59 years. Standard errors are clustered at the municipality level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Alternatively, an increased number of return migrants (or a reduction in the number of individuals who leave) may increase the competition for low-skilled jobs on local labor markets, thereby reducing low-skilled wages and increasing the wage-returns to education. However, when focusing on the adult population, we find no evidence of a decline in low-skill wages (c.f. Table A9). If anything, it looks as if low-skill wages increase in the age group 15-24 with the roll-out of the SC program.

Finally, we may be simply capturing a compositional effect, if more motivated youths decide to stay at home in response to the SC roll-out, and these would have been more likely to be in school (rather than working) in the absence of migration. Given that we do not find any direct migration effects in this age group (c.f. Table 2), and migrants to the US tend to be of lower education overall (as discussed previously), this seems unlikely to explain our findings.

6 Conclusion

The results presented in this paper suggest that a decline in the perceived attractiveness of migration from Mexico to the US increased educational investments among Mexican adolescents. These results are consistent with adolescents reassessing their labor markets opportunities in Mexico, and an implicit increase in the returns to education.

Our results are in line with prior evidence from the same country, which suggests that higher migration dependency is associated with lower schooling outcomes at the province level (McKenzie and Rapoport, 2011). At the same time, these results are in stark contrast with empirical evidence from a variety of countries that suggests that high-skill migration improves educational outcomes in origin communities, as the prospect of skill-selective migration raises returns to education (see e.g. Beine et al., 2008; Batista et al., 2012; Shrestha, 2017; Chand and Clemens, 2023). With this paper, we provide important complementary evidence from a low-skill migration corridor.

It is important to understand that this phenomenon by no means needs to be restricted to the US-Mexico context: Low-skill immigration is an important source of labor for many countries, particularly for countries with low labor-force participation rates among the domestic population (e.g. Saudi Arabia, UAE, Qatar, etc.) or rapidly aging populations (e.g. Germany, Italy, Japan).

With respect to the welfare implications of the SC program, the results of this paper

dividuals of prime working age (25-34), we indeed see a decline in the propensity to migrate, despite the window over which migration is observable being very small (i.e. between three months and one year, see Table A9).

complement previous evidence that has documented substantial costs for migrant and non-migrant populations in the US (East et al., 2023; Almuhaisen et al., 2024; Alsan and Yang, 2024). This paper finds that the program has also led to improved educational outcomes in Mexico, with the potential to improve incomes and economic development in the medium-run. However, we also find evidence suggestive of compositional changes (by gender, and possibly education) among future migrants to the US, which may further undermine the goals of the Secure Communities program.

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A Online Appendix

A.1 Additional Tables

Table A1: Summary Statistics

	All			2005			2012		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
Full Sample									
MCAS and Pop. census variables:									
Secure Communities Shock	0.31	0.38	2,351,698	0.00	0.00	312,020	0.90	0.07	271,699
Migration rate (5 years)	0.02	0.02	2,351,698	0.02	0.01	312,020	0.02	0.02	271,699
ENOE variables:									
Age	15.95	2.56	2,351,698	15.86	2.57	312,020	16.03	2.58	271,699
Female	0.50	0.50	2,351,698	0.50	0.50	312,020	0.49	0.50	271,699
Currently enrolled	0.71	0.45	2,351,657	0.70	0.46	312,007	0.72	0.45	271,697
Went to school in past 7days	0.57	0.50	2,351,698	0.56	0.50	312,020	0.58	0.49	271,699
Completed years of schooling	8.13	2.61	2,350,199	8.00	2.64	311,839	8.30	2.56	271,456
High-Migration Sample									
MCAS and Pop. census variables:									
Secure Communities Shock	0.30	0.37	194,909	0.00	0.00	25,032	0.88	0.11	23,211
Migration rate (5 years)	0.06	0.02	194,909	0.05	0.02	25,032	0.05	0.02	23,211
ENOE variables:									
Age	15.74	2.51	194,909	15.56	2.50	25,032	15.90	2.55	23,211
Female	0.51	0.50	194,909	0.52	0.50	25,032	0.49	0.50	23,211
Currently enrolled	0.61	0.49	194,908	0.60	0.49	25,031	0.64	0.48	23,211
Went to school in past 7days	0.48	0.50	194,909	0.48	0.50	25,032	0.52	0.50	23,211
Completed years of schooling	7.61	2.48	194,760	7.35	2.48	25,013	7.93	2.41	23,179

Note: ENOE outcomes restricted to age group 12-20. High-migration sample restricts observations to municipalities in the highest quartile of migration rate (5 years), as constructed from the 2000 Mexican Population Census.

Table A2: Representativity of ENOE Data

Variable	N	All Mean	SD	N	ENOE Mean	SD	All - ENOE Diff.	p-val	Norm. Diff.
Years of schooling	611	9.09	4.28	290	8.97	2.99	-0.13	0.61	0.03
Labor force participation	611	0.44	0.09	290	0.45	0.08	0.01	0.38	-0.10
Hourly wages (log)	611	9.46	0.35	290	9.58	0.25	0.11	0.00	-0.40
Household size	611	4.41	0.51	290	4.43	0.48	0.02	0.61	-0.04
Household owns a phone	611	0.13	0.11	290	0.15	0.10	0.02	0.00	-0.20
Household owns refrigerator	611	0.52	0.21	290	0.57	0.19	0.05	0.00	-0.24
Household owns a TV	611	0.76	0.17	290	0.80	0.15	0.04	0.00	-0.25
Household owns the dwelling	611	0.86	0.08	290	0.84	0.08	-0.02	0.00	0.20
Access to hot water	611	0.27	0.21	290	0.31	0.20	0.04	0.01	-0.20
Dwelling has a dedicated kitchen	611	0.91	0.07	290	0.91	0.07	0.00	0.73	0.00
Dwelling has a toilet	611	0.77	0.16	290	0.77	0.16	0.00	0.84	0.00
Fraction of migrants in pop(last 5 yrs)	611	0.06	0.02	290	0.06	0.02	-0.00	0.11	.
Population density	611	62.69	86.91	290	74.27	104.77	11.58	0.10	-0.12
Urban	611	0.32	0.30	290	0.41	0.29	0.09	0.00	-0.31

Note: All outcomes are municipality averages constructed from the 2000 Mexican Population Census for high-migration municipalities (i.e. municipalities in the highest quartile of migration rates).

Table A3: Robustness of Main Results to Alternative Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep.var.: School Attendance							
SC shock	0.079	0.092*	0.089*	0.098*	0.099*	0.091*	0.097*
	(0.053)	(0.052)	(0.052)	(0.052)	(0.052)	(0.052)	(0.052)
Dep.var.: Enrollment							
SC shock	0.079	0.094*	0.091*	0.098*	0.099**	0.091*	0.097*
	(0.051)	(0.050)	(0.050)	(0.050)	(0.050)	(0.049)	(0.050)
Dep.var.: Years of Schooling							
SC shock	0.524**	0.446**	0.441**	0.448**	0.423**	0.418**	0.445**
	(0.210)	(0.196)	(0.195)	(0.197)	(0.193)	(0.180)	(0.197)
Observations	68715	68715	68715	68715	68715	68386	68715
Clusters	290	290	290	290	290	290	290
Municipality, Time FE	✓	✓	✓	✓	✓	✓	✓
Age-by-gender FE		✓	✓	✓	✓	✓	✓
Migration Share × Time FE			✓	✓	✓	✓	✓
Migration Share$(^2)$ × Time FE				✓	✓	✓	✓
Number of Interview FE					✓		
Adults' highest education						✓	
Labour income in hh (log)							✓

Note: Standard errors are clustered at the municipality level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table A4: Robustness of Main Results to Alternative Sample and Shock Definitions

Time period:	2005-2013	2008-2013	2008-2012	2005-2012	
	(1) bsl	(2) bsl	(3) bsl	(4) excl. sanc cities	(5) weighted
Dep.var.: School Attendance					
SC shock	0.099*	0.105*	0.116**	0.085	2.074**
	(0.052)	(0.055)	(0.052)	(0.052)	(0.852)
Dep.var.: Enrollment					
SC shock	0.114**	0.085	0.074	0.085*	1.843**
	(0.052)	(0.057)	(0.052)	(0.050)	(0.832)
Dep.var.: Years of Schooling					
SC shock	0.444**	0.436**	0.443**	0.399**	7.930**
	(0.203)	(0.207)	(0.197)	(0.197)	(3.396)
Observations	76012	49437	42140	68715	68715
Clusters	337	330	282	290	290
Municipality, Time FE	✓	✓	✓	✓	✓
Age-by-gender FE	✓	✓	✓	✓	✓
Migration Share$(^2)$ × Time FE	✓	✓	✓	✓	✓

Note: Standard errors are clustered at the municipality level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table A5: Municipality-aggregate Effect of Secure Communities on Schooling, 15-17 years

	(1)	(2)	(3)	(4)
Dep.var.: School Attendance				
SC shock (weighted)	1.108***	1.164***	1.163***	1.200***
	(0.304)	(0.299)	(0.346)	(0.365)
Observations	33443	33443	33443	33443
Dep. var. mean	0.561	0.561	0.561	0.561
Dep.var.: Enrollment				
SC shock (weighted)	1.010***	1.072***	0.919**	1.062***
	(0.345)	(0.338)	(0.376)	(0.399)
Observations	33443	33443	33443	33443
Dep. var. mean	0.701	0.701	0.701	0.701
Dep.var.: Years of Schooling				
SC shock (weighted)	2.113*	1.889	1.931	2.900**
	(1.253)	(1.210)	(1.338)	(1.318)
Observations	33439	33439	33439	33439
Dep. var. mean	8.339	8.339	8.339	8.339
Clusters	1275	1275	1275	1275
Municipality, Time FE	✓	✓	✓	✓
Mean age		✓	✓	✓
State × Time trends			✓	
State × Time FE				✓

Note: Standard errors are clustered at the municipality level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table A6: Effect of Secure Communities on Schooling Outcomes in Non-Migrant Commnunities

	Q1 of Migr Share				Q2 of Migr Share				Q3 of Migr Share			
Age group	All	12-14	15-17	18-20	All	12-14	15-17	18-20	All	12-14	15-17	18-20
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dep.var.: School Attendance												
SC shock	0.007	0.017	-0.040	0.044	-0.012	-0.042*	0.009	-0.000	0.021	0.004	0.025	0.031
	(0.029)	(0.036)	(0.042)	(0.034)	(0.021)	(0.022)	(0.030)	(0.026)	(0.033)	(0.041)	(0.044)	(0.031)
Observations	398389	135401	134588	128400	866925	288678	292576	285671	891475	297289	302244	291941
Dep. var. mean	0.580	0.758	0.596	0.375	0.586	0.772	0.605	0.378	0.559	0.746	0.564	0.364
Dep.var.: Enrollment												
SC shock	0.031	0.013	0.004	0.076**	0.003	-0.013	0.019	0.007	0.020	0.020	0.013	0.023
	(0.022)	(0.028)	(0.033)	(0.034)	(0.018)	(0.012)	(0.026)	(0.030)	(0.019)	(0.014)	(0.030)	(0.032)
Observations	398379	135400	134586	128393	866911	288676	292572	285663	891459	297288	302239	291931
Dep. var. mean	0.715	0.934	0.736	0.463	0.725	0.952	0.752	0.470	0.714	0.940	0.725	0.472
Dep.var.: Years of Schooling												
SC shock	0.045	0.010	0.080	-0.042	-0.088	-0.073	-0.071	-0.120	-0.045	-0.063	-0.053	-0.079
	(0.127)	(0.169)	(0.169)	(0.218)	(0.085)	(0.076)	(0.103)	(0.159)	(0.077)	(0.074)	(0.096)	(0.191)
Observations	397930	135359	134382	128189	866549	288661	292443	285445	890960	297230	302020	291709
Dep. var. mean	0.715	0.934	0.736	0.463	8.239	5.986	8.548	10.199	8.221	5.984	8.515	10.195
Clusters	262	262	262	262	373	373	373	373	357	357	357	355
Municipality, Time FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Age-by-gender FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Migration Share$^{(2)}$ $\times$ Time FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Note: Standard errors are clustered at the municipality level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table A7: Effect of SC by Gender, Adolescents 15-17

Effect on:	Females			Males		
	(1) Study	(2) Enroll	(3) Yrs Schooling	(4) Study	(5) Enroll	(6) Yrs Schooling
SC shock	0.160**	0.182***	0.576**	0.050	0.026	0.339
	(0.062)	(0.062)	(0.259)	(0.064)	(0.061)	(0.222)
Observations	34359	34359	34343	34430	34430	34370
Clusters	289	289	289	288	288	288
Dep. var. mean	0.488	0.618	8.301	0.436	0.561	7.944

Note: Standard errors are clustered at the municipality level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table A8: Effect of SC on Household Composition of Adolescents 15-17

	Any returnee	Any migrant	Remittances	Max. education	Dep. ratio	Hh labour income
	(1)	(2)	(3)	(4)	(5)	(6)
SC shock	-0.004	0.000	-0.021	0.132	0.017	-0.210
	(0.007)	(0.011)	(0.045)	(0.475)	(0.019)	(0.324)
Observations	55026	55026	26030	68453	68791	68791
Clusters	288	288	283	290	290	290
Dep. var. mean	0.011	0.024	0.145	8.769	0.557	6.589

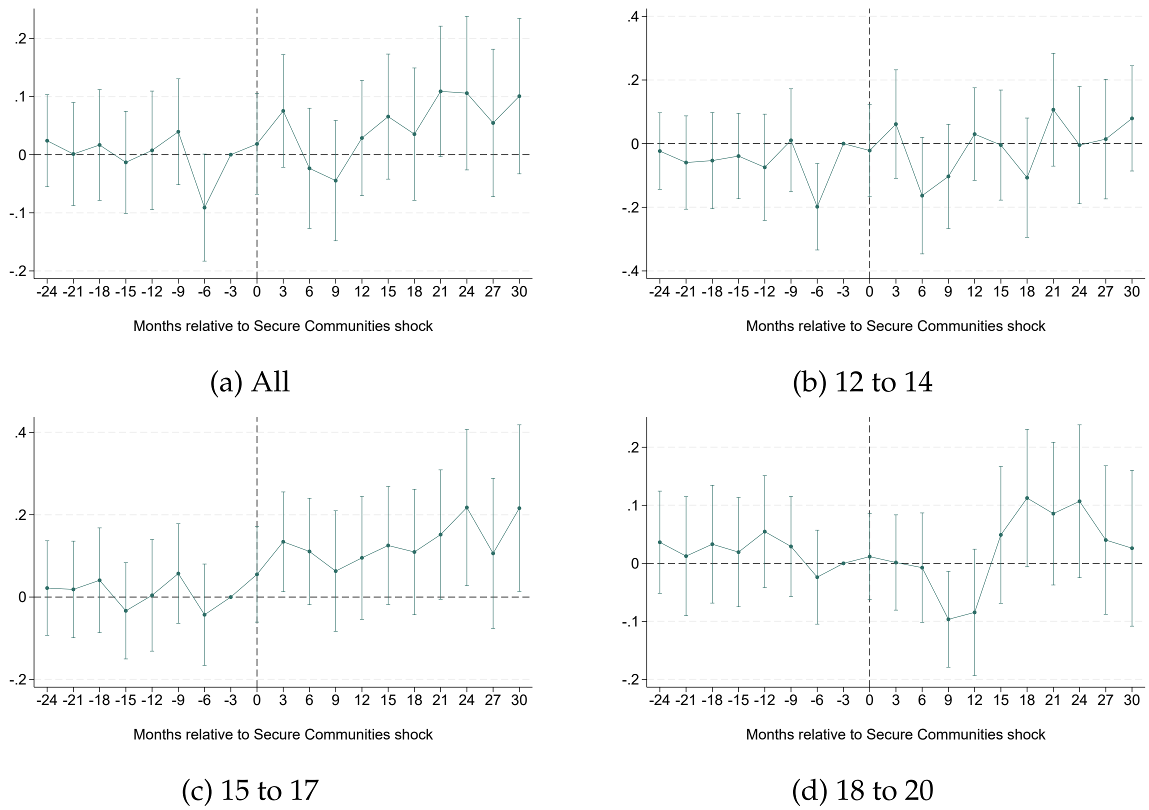
Note: Dependent variable is in the column header. The variables any returnee and any migrant cannot be computed for the first interview (of five per household) and is thus available for fewer observations. Whether the household received any remittances is only collected once per year (not every quarter). Maximum education is the highest years of schooling observed among household members aged 18 and above. The dependency ratio is the number of household members below the age of 18 or above the age of 59 divided by the number of household members. Standard errors are clustered at the municipality level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

Table A9: Effect of SC on Labor Market Outcomes for Adults

Age group	All	15-24	25-34	35-44
	(1)	(2)	(3)	(4)
Dep.var.: Individual was interviewed				
SC shock	0.043***	0.050**	0.045*	0.029
	(0.017)	(0.025)	(0.023)	(0.020)
Observations	509330	223735	152314	133281
Clusters	290	290	290	290
Dep. var. mean	0.856	0.824	0.858	0.909
Dep.var.: Individual is abroad				
SC shock	-0.003	0.003	-0.016*	0.004
	(0.006)	(0.008)	(0.009)	(0.010)
Observations	509330	223735	152314	133281
Clusters	290	290	290	290
Dep. var. mean	0.020	0.020	0.022	0.018
Dep.var.: Individual is working				
SC shock	0.007	-0.010	0.034	0.009
	(0.015)	(0.023)	(0.025)	(0.026)
Observations	436125	184377	130639	121109
Clusters	290	290	290	290
Dep. var. mean	0.565	0.462	0.624	0.659
Dep.var.: log(hourly wage)				
SC shock	-0.019	0.026	0.012	-0.118*
	(0.040)	(0.046)	(0.065)	(0.072)
Observations	179247	57441	62038	59763
Clusters	290	287	288	290
Dep. var. mean	2.839	2.693	2.895	2.921
Dep.var.: log(low-skilled hourly wage)				
SC shock	-0.007	0.090*	-0.059	-0.080
	(0.043)	(0.051)	(0.058)	(0.081)
Observations	130094	41558	44003	44526
Clusters	290	284	287	290
Dep. var. mean	2.721	2.641	2.757	2.761

Note: Low-skilled wage is computed for individuals with 9 years of schooling or less (the median in the sample of individuals aged 15-44. Standard errors are clustered at the municipality level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10% (*).

A.2 Additional Figures



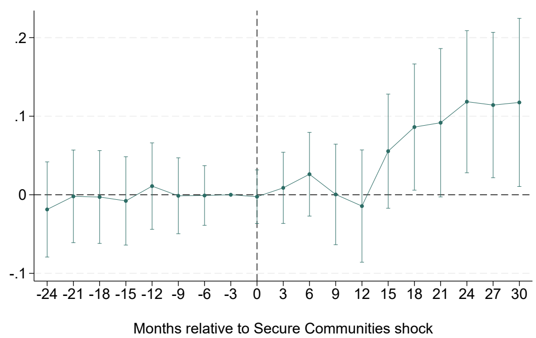
(a) All

(b) 12 to 14

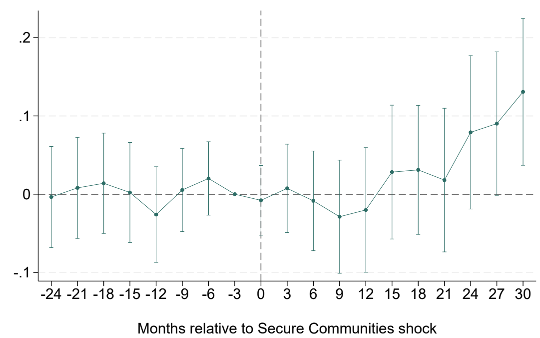
(c) 15 to 17

(d) 18 to 20

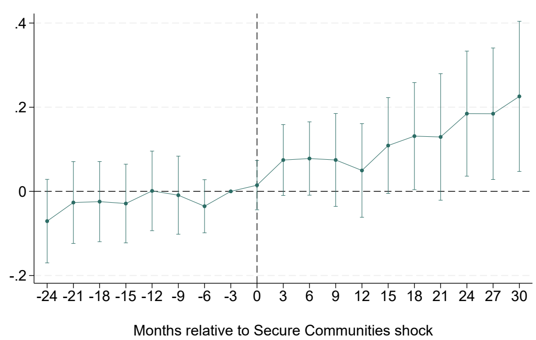
Figure A1: Effect of SC on School Attendance - Event Study Results



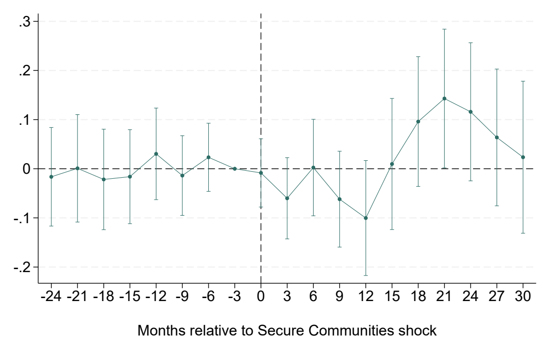
(a) All



(b) 12 to 14

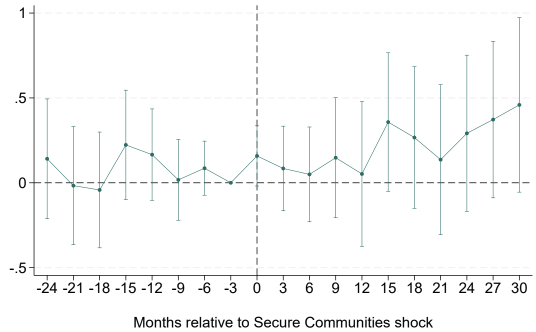


(c) 15 to 17

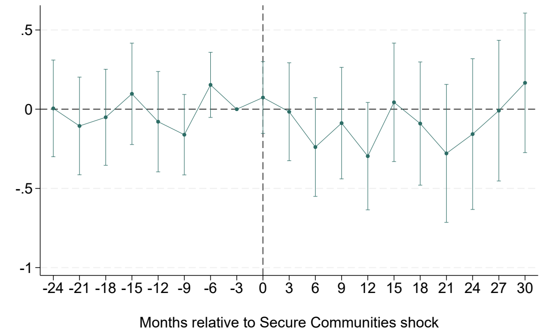


(d) 18 to 20

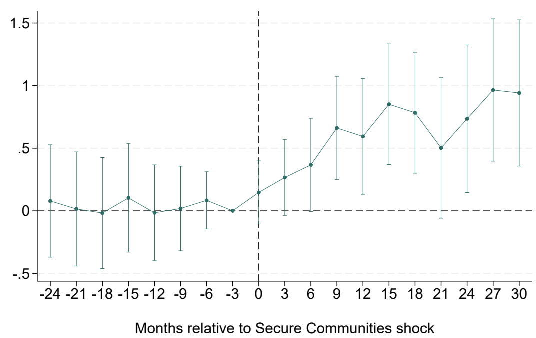
Figure A2: Effect of SC on Enrollment - Event Study Results



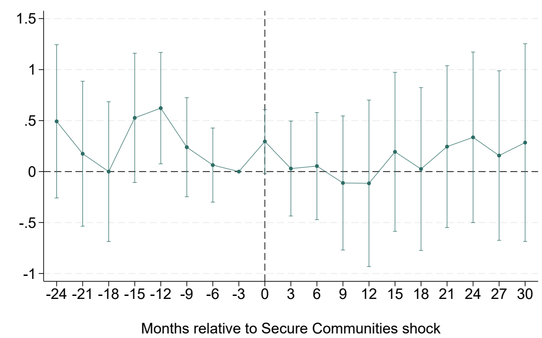
(a) All



(b) 12 to 14

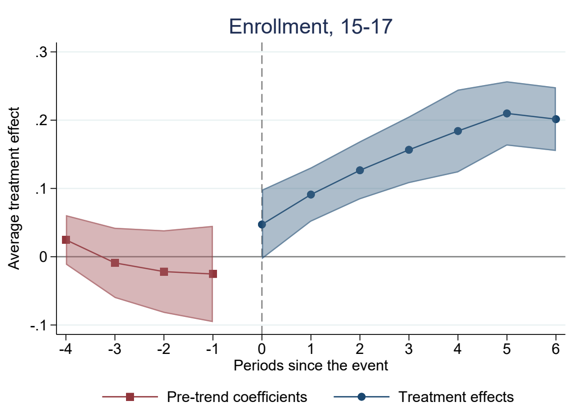


(c) 15 to 17

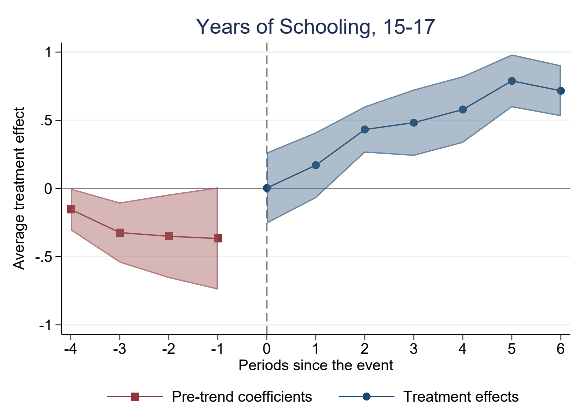


(d) 18 to 20

Figure A3: Effect of SC on Completed Years of Schooling - Event Study Results

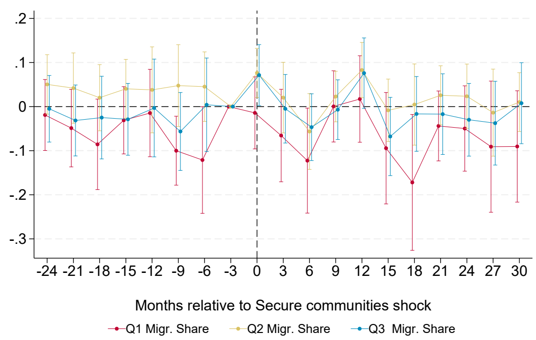


(a) Enrollment

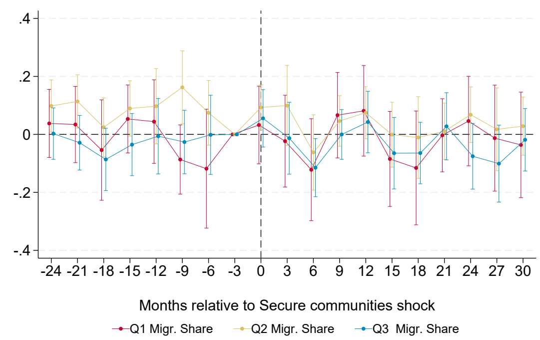


(b) Years of Schooling

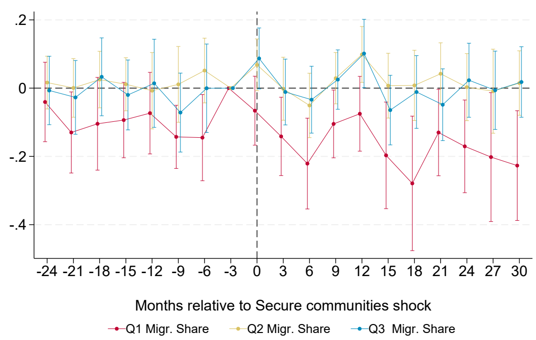
Figure A4: Effect of dichotomized SC Shock, Borusyak et al. (2024) imputation estimator



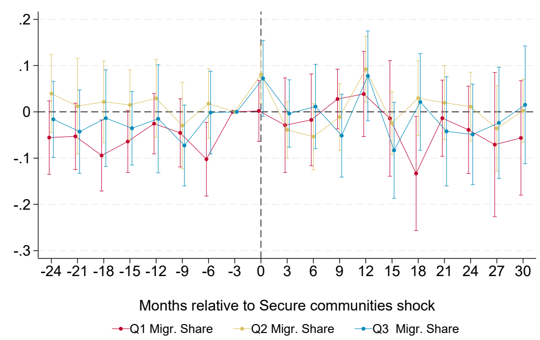
(a) All



(b) 12 to 14

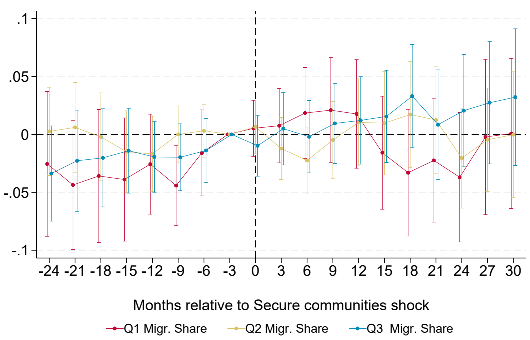


(c) 15 to 17

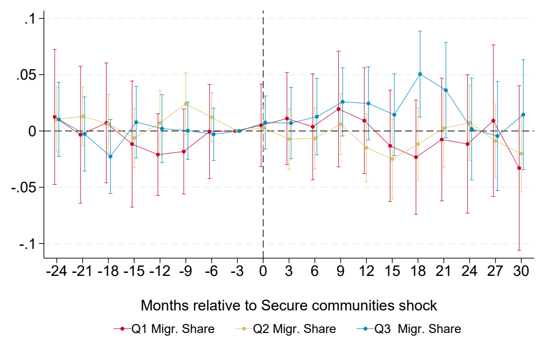


(d) 18 to 20

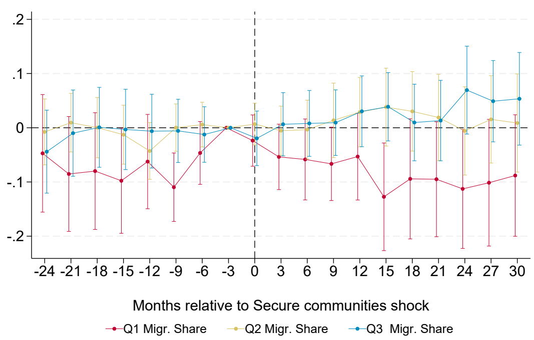
Figure A5: Effect of SC shock (at different levels of migrant share) on School Attendance



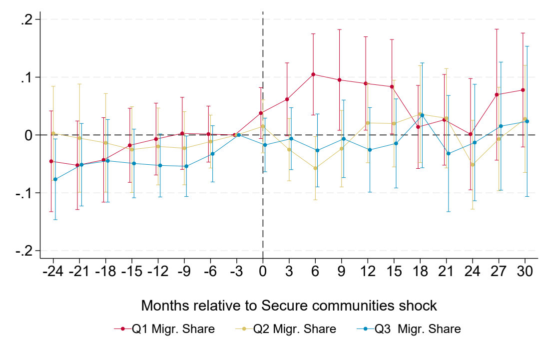
(a) All



(b) 12 to 14

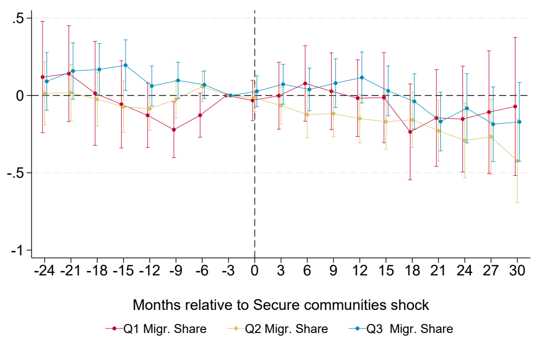


(c) 15 to 17

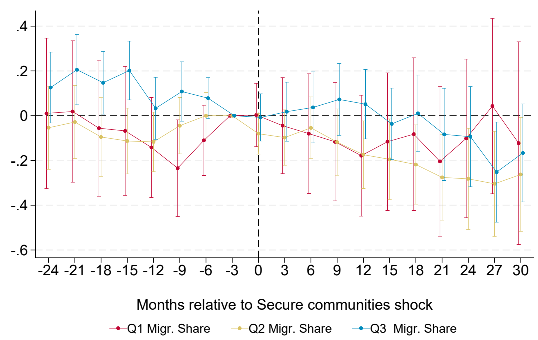


(d) 18 to 20

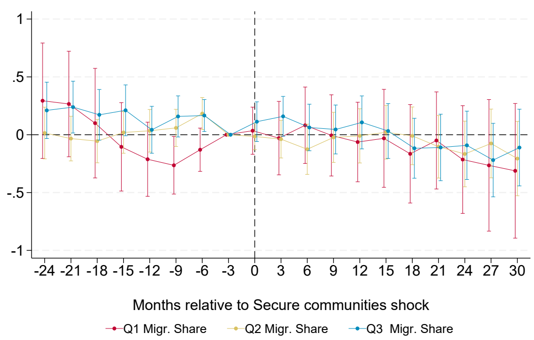
Figure A6: Effect of SC shock (at different levels of migrant share) on Enrollment



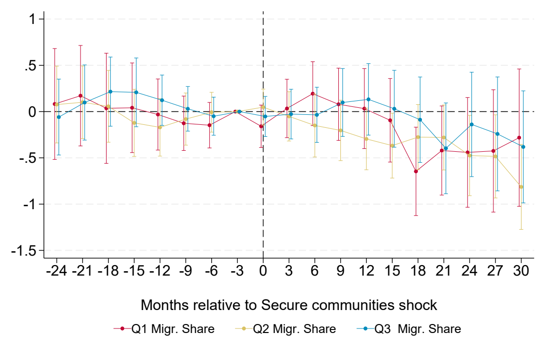
(a) All



(b) 12 to 14



(c) 15 to 17



(d) 18 to 20

Figure A7: Effect of SC shock (at different levels of migrant share) on Completed Years of Schooling