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Does Parental Migration During Childhood Affect Children's Lifetime Educational Attainment? Evidence From Mexico

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ABSTRACT Existing scholarship reveals important and competing influences of parental migration on children's educational trajectories. On the one hand, in the short term, left-behind children commonly take on additional housework and sometimes place less emphasis on education if they aspire to follow in their parents' migratory footsteps. On the other hand, parental migration often leads to monetary transfers (remittances), which reduces financial pressure on sending households and can strengthen educational aspirations among children left behind. Because previous studies examined these effects on children still completing their educations, the cumulative impact of parental migration on children's educational attainment remains uncertain. In this study, we use retrospective life history data from the Mexican Migration Project to link parental migrations occurring during childhood with children's educational attainment measured in adulthood. Using a novel counterfactual approach, we find that parental migration during childhood is associated with increased years of schooling and higher probabilities of completing lower-secondary school, entering upper-secondary school, and completing upper-secondary school. These associations were strongest among children whose parents did not complete primary school and those living in rural areas. Results from a placebo test suggest that these positive associations cannot be attributed to unobserved household characteristics related to parental migration, which supports a causal interpretation of our main findings. Thus, our analysis suggests that, on average, and particularly among more-disadvantaged households, the long-term educational benefits associated with parental migration outweigh short-term disruptions and strain associated with parental absence.

KEYWORDS International migration/immigration • Education • Intergenerational mobility • Causal analysis • Mexico

Introduction

International migration is a commonly used strategy through which household members seek to accumulate resources, mitigate financial uncertainty, and provide better opportunities for their families (Garip 2016; Massey et al. 1987; Stark 1991). In particular, migrant parents often invest newly acquired resources in their children's education to create more opportunities for their futures (Abrego 2014; Dreby 2010).

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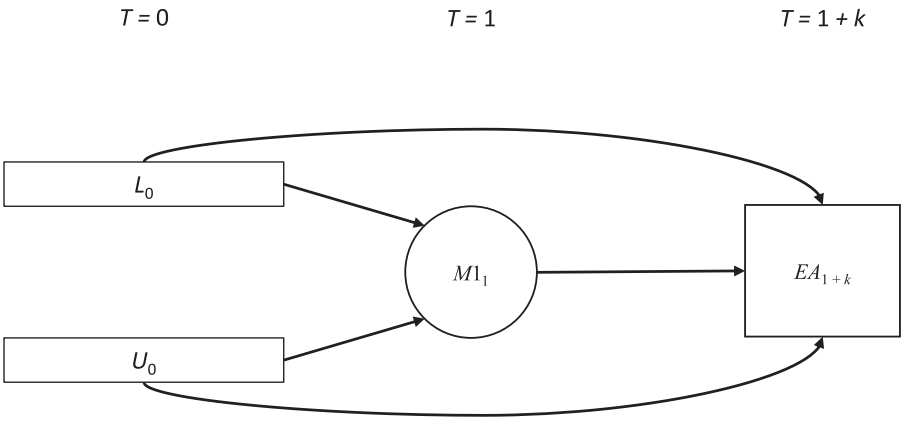
In 2017, about 260 million persons lived outside their countries of birth (United Nations 2017). Estimates suggest that 15% to 30% of children throughout Africa, Asia, and Latin America will live apart from at least one migrant parent (Bryant 2005; DeWaard et al. 2018). Parental absence due to migration occurs most commonly in socioeconomically disadvantaged households—that is, those with parents who have completed little schooling and live in rural areas with weaker educational institutions (Lu and Treiman 2011; Nobles 2013; Rendall and Parker 2014). The ways parental migration affects educational attainment among children could have broad implications, positive or negative, for social mobility in migrant-sending areas, which are commonly characterized by high levels of inequality and low levels of intergenerational mobility (Huerta-Wong et al. 2013; Marteleto et al. 2012; Ravallion 2014; Torche 2014).

Existing scholarship reveals important and competing influences of parental migration on children's educational trajectories. For instance, in the short term, left-behind children commonly take on additional housework or enter the workforce to mitigate the loss of a primary breadwinner. These educational disruptions can be compounded over time by long-term family separation, particularly when children aspire to follow in their parents' migratory footsteps (Amuedo-Dorantes and Pozo 2010; Jampaklay 2007; Kandel and Massey 2002). At the same time, parental migration often leads to monetary transfers (remittances), which reduces financial pressure on sending households and can strengthen educational aspirations among children left behind (Abrego 2014; Edwards and Ureta 2003; Hanson and Woodruff 2003; Nobles 2011). Because previous studies examined these effects on children still completing their educations, the cumulative impact of parental migration on children's educational attainment remains uncertain.

Empirically, it is difficult to identify the cumulative association between parental migration during childhood and children's lifetime educational attainment because it requires linking two temporally distant events. Most surveys that capture international movement, such as those cited earlier, are cross-sectional or contain short panels covering only a few years. As a result, these studies generally restricted their foci to a single stage of a household's migration history—either immediately following parental departure (e.g., Antman 2011; Chang et al. 2011) or while the parent is abroad—but without information about time since emigration or the household's premigration context (e.g., Halpern-Manners 2011; Hanson and Woodruff 2003; Nobles 2011). International migration is a dynamic process that evolves at the household and community levels (Garip 2012; Massey 1990), and evidence suggests that the relationship between parental migration and children's schooling changes across household and community migration histories (Curran et al. 2004; Kandel and Massey 2002). Thus, attempts to identify the relationship between parental migration and children's educational attainment based on single time points could bias research findings upward or downward depending on the timing of data collection.

In this study, we attempt to overcome these limitations by using retrospective life history data from the Mexican Migration Project (MMP) to link parental migrations occurring during childhood with children's educational attainment measured in adulthood. With this information, we address two research questions. First, what is the association between parental migration during childhood and children's lifetime edu-



- L_0: Observed pre-migration household and community context
- U_0: Unobserved pre-migration household and community context
- $M1$: A first parental migration
- EA: Child's educational attainment measured during adulthood

Fig. 1 Causal diagram depicting the effect of a first parental migration during childhood on a child's educational attainment measured in adulthood

cational attainment? Second, does the association between parental migration during childhood and children's lifetime educational attainment vary by level of household and community disadvantage?

Figure 1 illustrates our analytical approach. We define the first parental migration (M1) as occurring at time $T=1$ (which we restrict to households in which the parent's first migration happened while the child was 1–14 years old). We link M1 to children's lifetime educational attainment (EA), which we measure at time $T=1 + K$, where K is a nonnegative integer greater than 6 to ensure that the child has aged out of their standard schooling years (i.e., is 20 years old or more).[1] We use a broad set of covariates (L_0), which we measured at time $t=0$, to match migrant children against the most similar children without migrant parents, thus providing counterfactual information about educational attainment sans parental migration. However, our models cannot control for all household characteristics that may correlate with the likelihood of parental departure (U_0 in Figure 1), such as parent-child relationship quality. To address this limitation, we conducted a placebo test using adult children aged 15 or older at the time of parental migration—children whose schooling should be unaffected by parental departure. The placebo results were nonsignificant, indicating that static unobserved household characteristics did not contaminate our primary models and supporting a causal interpretation of our main findings.

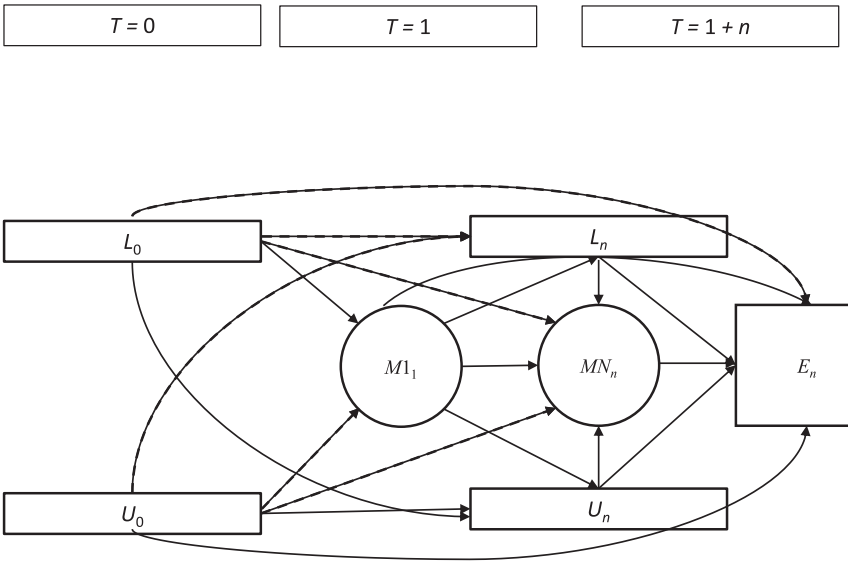
[1] K cannot be less than 6 because the oldest persons identified as children at the time of parental migration were aged 14, and the youngest respondents that we define as "adult children" at the time of the survey are age 20 (20 – 14 = 6). Our results are robust to restricting our sample to adult children aged 25 or older such that K is greater than or equal to 11.

Background

Qualitative studies on the social and emotional hardships that transnational families experience, as well as the resiliency of these kinship ties despite physical separation, have found migration to be a family-centered process that parents undertake in part to promote the socioeconomic mobility of their children (Dreby 2010; Olwig 1999; Orellana et al. 2001). Some parents manage to improve their children's quality of life by sending financial remittances, which children remaining behind may grow to view as symbolic of their parents' sacrifices for their schooling and improved living conditions (Abrego 2014). However, many parents and children also experience strain in their relationships. Children often express feelings of emotional loss and resentment toward their absent parents' decisions to move abroad (Dreby 2010; Jingzhong and Lu 2011; Parreñas 2005). Parental migration can also impose financial burdens on family members remaining behind, particularly in the immediate aftermath of departure while migrant parents repay debts owed to smugglers or recruitment agencies and struggle to secure steady employment abroad (Abrego 2014). Thus, while parental migration may enhance children's long-term academic opportunities, it can also result in behavioral issues and increased financial burdens. These consequences can sometimes lead to worse school performance and even dropping out, thereby reducing children's lifetime educational attainment.

Survey-based research has reflected these contradictory impacts of parental migration. Some scholars found that parental migration is associated with worse educational outcomes, which they attributed to a combination of family separation, a culture of migration, and financial hardships following parental departure (Creighton et al. 2009; Halpern-Manners 2011; Lu 2012). In particular, parent-child separation during migration can result in a loss of social support for children remaining behind (Graham 2011; Lu 2014), which can worsen their school performance and increase their risk of dropping out (Creighton et al. 2009; Zhou et al. 2014). Increased financial burdens immediately following parental departure can compound the consequences of family separation. For instance, parental migration predicts short-term declines in children's time spent studying (Antman 2011) and increases in time spent on housework and farm work (Chang et al. 2011). Consequently, parental migration can worsen schooling among children through various pathways, particularly in the immediate aftermath of departure. The emergence of a "culture of migration" can exacerbate declining social support and increased household responsibilities. Evidence has shown that children living in households and communities within established migrant networks are more likely to aspire to migrate themselves. This focus on migration disrupts their focus on school completion and potentially reduces the benefits of greater financial resources that stem from remittances (Halpern-Manners 2011; Kandel and Massey 2002).

In contrast, some survey-based studies have highlighted a positive association between parental migration and children's educational attainment, finding that the financial benefits associated with a parent's move abroad can offset the migration's adverse effects on children's schooling (Dustmann 2008; Nobles 2011). These studies often conceptualized parental remittances, which sending households commonly invest in children's education (Massey et al. 2013), as a quasi-exogenous boost to household income (Hanson and Woodruff 2003). For example, Curran et al.'s (2004)



- L: Observed household and community context
- U: Unobserved household and community context
- M: Parental migration
- E_n: Children's educational attainment measured at time $T = 1 + n$

Fig. 2 Causal diagram depicting the effect of a parental migration n during childhood on a child's educational attainment measured at time n

study in Thailand found that having at least one remitting parent increased the odds of transitioning from primary to lower secondary school. Nobles (2011) documented a positive association between parental remittances and reported aspirations to attend college among the children of migrants in Mexico (see also Amuedo-Dorantes and Pozo 2010). In this way, remittances can positively influence school retention, potentially offsetting the culture of migration and the adverse effects of parental absence (Edwards and Ureta 2003; Hu 2013; Lu and Treiman 2011).

The lack of consensus in previous research on how parental migration impacts children's education stems from data limitations, which have prevented scholars from directly testing the model shown in Figure 1. As our review of the literature shows, scholars have generally examined specific pieces of the migration-education relationship depending on available information. Figure 2 illustrates the limitations of this approach for drawing causal inferences. Scholars have typically identified the association between the nth parental migration (MN_n)—or characteristics of the nth migration, such as financial remittances—and school-aged children's educational attainment or risk of dropout (E_n), measured concurrently or within a few years of departure. These studies generally adjusted for household and community characteristics measured at time $t = n$ (Acosta 2011; e.g., Amuedo-Dorantes and Pozo 2010; Creighton et al. 2009; Edwards and Ureta 2003; Halpern-Manners 2011; Lu 2014; Lu and Treiman 2011; Nobles 2011) or time $t = n - k$ in the case of short-wave panel studies, such as Antman's (2011) analysis of school and work outcomes among Mexican youth in response to a recent paternal migration (see also Chang et al. 2011).

These approaches introduced two potential sources of bias: retrospective and prospective biases. On the one hand, cross-sectional studies that observe only L_n, MN_n, and E_n (e.g., Creighton et al. 2009; Edwards and Ureta 2003; Halpern-Manners 2011; Hanson and Woodruff 2003; Nobles 2013) cannot adjust for the potentially confounding influence of earlier contextual features (L_0) or previous parental migrations ($M1 \ldots n-1$). Adjustment for L_n—possibly years or decades after the parent's first departure—could introduce substantial bias into these cross-sectional studies because contextual factors change over time in ways that often directly relate to households' and communities' migration histories (Massey 1990; Massey et al. 1994; Mines and Massey 1985). On the other hand, studies that predict educational outcomes, such as school dropout at time $t=n$ in response to parental migration and household context at a previous time $T=n-k$, cannot capture the lifetime effects of parental migration. For instance, children could initially leave school to account for the loss of a migrant breadwinner but later reenroll upon the commencement of a remittance flow.

Beyond these issues of timing, it is well established that household-level migration behaviors evolve (Garip 2012; Massey et al. 1987; Mines and de Janvry 1982; Reichert 1981). First-time migrants often accrue debt, which can limit their ability to send remittances and push family members remaining behind into the workforce (Abrego 2014; Antman 2011; Mines and Massey 1985). However, across multiple trips abroad, migrant-sending households gain considerable asset advantages relative to their nonmigrant peers (Garip 2012; Massey et al. 1994; Mines and Massey 1985), and remittance levels increase significantly with accumulated migration experience (Garip 2012, 2014). Even studies that focused on intervening mechanisms, such as the presence of a remittance flow (e.g., Amuedo-Dorantes and Pozo 2010; Nobles 2011), could introduce bias to the extent that current patterns are endogenous to previous events. Indeed, Curran et al. (2004) contended that the relationship between parental migration and children's schooling changes at various stages of household and community migration histories.

In our study, we aim to overcome the limitations of earlier work by identifying the association between parental migration during childhood and children's total educational attainment measured in adulthood. To do so, we use retrospective longitudinal data from the MMP to connect adult children's educational attainment with their parents' life histories. With this data, we estimate the association between $M1$ and EA, net of L_0, as described in Figure 1. As such, we provide a precisely adjusted estimate of the association between parental migration during childhood and children's educational attainment measured in adulthood.

Based on our initial findings, we also assess whether the lifetime impacts of parental migration are more substantial among children from socioeconomically disadvantaged backgrounds. Given that parental movement can contribute to academic achievement through the remission of financial resources (Abrego 2014; Dreby 2010; Massey et al. 2013; Nobles 2013), we expect that the observed benefits (losses) attributable to parental migration will be largest (smallest) among more socioeconomically disadvantaged children—that is, those who have the most to gain (lose) from an economic infusion (depletion) in their households. This expectation is consistent with prior studies documenting a larger contemporaneous association between parental migration and children's education in rural areas and in households with lower levels of parental education (Hanson and Woodruff 2003; Lu and Treiman 2011).

Building on these studies, we examine differences in the association between parental migration during childhood and children's lifetime educational attainment by household socioeconomic status (i.e., parental education) and community context (i.e., rural/urban).

Data and Methods

Estimating the effect of parental migration during childhood on educational attainment in adulthood requires panel data containing information on parents, children, households, and communities. We are unaware of a sufficiently long panel that includes a large sample of parents who migrated while their children were in school. Therefore, we used retrospective life history data from the MMP to link adult children's educational attainment to their parents' first U.S. migrations that occurred during childhood.

Each year since 1987, the MMP has collected random household samples in four to six Mexican communities and respondent-driven samples of migrant households from those same communities in the United States. As of 2018, the MMP sample included 27,274 households in 170 communities, spread across 24 of Mexico's 32 states. These data are representative of the sample regions compared with nationally representative surveys administered by the Mexican Census Bureau (Massey and Zenteno 2000). Massey and Zenteno (2000) found that the MMP captures areas responsible for sending 90% of Mexican migrants to the United States. Thus, these data provide an ideal source for describing social and demographic processes related to Mexico-U.S. migration, the largest binational migration flow in the world over the last 50 years (Abel and Sander 2014).

Data collection staff use ethnographic and survey techniques to collect detailed demographic information about household heads, their spouses, and all resident and—importantly—nonresident children of the household head. With this information, we can identify years of schooling and the highest level of education completed by resident and nonresident adult children of household heads. Because we are interested in the effect of parental migration during childhood on children's educational attainment in adulthood, we restricted our sample to adult children, here defined as age 20 years or older. Therefore, *lifetime educational attainment* refers to schooling completed during standard educational years.

Educational Attainment

We assessed multiple dimensions of educational attainment: total years of schooling, completion of lower-secondary school (9 years), entry into upper-secondary school (10 years), and completion of upper-secondary school (12 years).[2] Although years of schooling quantifies total educational attainment, it also obscures the structural char-

[2] We also replicated our analysis among children aged 25 years and older with the addition of some college and college completion to our set of educational outcome variables.

acteristics of Mexico's educational system. Throughout the latter half of the twentieth century, the Mexican government enacted significant reforms to increase educational opportunities. Beginning in 1950, Mexico widely expanded its public education system by constructing thousands of primary and lower-secondary schools, principally in poor and rural communities (Creighton and Park 2010). Then, in 1992, Mexico passed the National Agreement to Modernize Basic Education, which made lower-secondary school (seventh through ninth grade) mandatory and tuition-free (Parker et al. 2007).[3] These expansions and reforms significantly increased primary and secondary school completion among Mexicans born since about 1980 (Behrman et al. 2007), with conditional matriculation into lower-secondary school reaching 96% in 2017 (OECD 2018). However, by that same year, only one-half of Mexicans under age 25 had completed upper-secondary school (grades 10 to 12), which remains nonmandatory and requires tuition payments (OECD 2018). Advancement beyond lower-secondary school is a valuable marker of educational mobility, particularly among children with less-educated parents (Urbina 2018).

Parental Migration

We restricted our sample to the children of the household head that were at least 20 years old at the time of the survey and born after 1964 in the post-Bracero era ($n = 38{,}813$). Although our sample could technically include children whose parents migrated as recently as 2012 (children aged 14 in 2012 would turn 20 in 2018 and become eligible for our study), we capped our range at 2003. We excluded children born after 2003 from our sample because Mexican migration to the United States declined precipitously in the twenty-first century (Massey et al. 2015). Despite an average of 47 parental departures per year, we observed 24 total departures from 2003 to 2012, with no departures recorded in multiple years. These minuscule cell sizes raise concerns given our reliance on community and year fixed effects, as described later. Thus, we restricted our analysis to person-years between 1965 and 2003, inclusive.

We defined childhood as occurring between ages 0–14, including children who are at home or attending primary or lower-secondary school but have not yet matriculated into upper-secondary education. We dichotomously identified parental migration based on the year of initial departure of the household head. We restricted our focus to migrations undertaken by the household head because only children of the household head are explicitly linked to their parent (the household head). That is, we cannot guarantee that a household head's spouse at the moment of the survey is also the parent of the household head's children or that the current spouse was present in the household during childhood.

If the household head's first migration occurred when a child was between ages 0 and 14, we identified that child as a migrant-child and placed them in the treated group. We classified children as nonmigrant children if their household heads never migrated, first migrated before the child was born, or first migrated after the child's

[3] Students' families are still responsible for their uniforms, notebooks, and other materials.

fifteenth birthday. We placed these nonmigrant children in the set of potential control observations.[4] Thus, our treatment variable dichotomously identified 2,839 children of the household head whose parent first migrated to the United States when those children were aged 0–14. We counted 574 migrant children whose household heads accumulated fewer than 12 months of total U.S. migration experience as missing,[5] which left 2,265 migrant children in our sample. For each of these adult children, we retained time-invariant measures of educational attainment, year of birth, and sex.

Childhood Context

To measure context during childhood, we linked each adult child to their household heads' retrospective life history. The life history technique locates significant events, such as marriages or migrations, with visual calendar cues (Axinn et al. 1999). The MMP relies on these techniques in tandem with community observations and ethnographic methods to collect accurate retrospective data (Massey 1987). With these life histories, we created panels describing each respondents' yearly household context during childhood, yielding a data set of 587,330 child-years.

For children whose parents migrated to the United States during childhood, we identified the year of a first parental migration, $T=1$. We dropped 39,206 migrant child-years other than year $T=0$—that is, the year immediately before parental migration (see Figure 1)—leaving a total sample of 548,124 child-years. In this way, we linked each migrant child's schooling measured in adulthood to their childhood context immediately before parental departure. However, identifying migrant children's household context in year $T=0$ created a new challenge: the delineation of an appropriate control group. While we reduced each migrant child to a single child-year, there were still 15 child-years for each nonmigrant child. To identify an appropriate control group with which to estimate the association between parental migration during childhood and children's lifetime educational attainment in adulthood, we used propensity score matching (PSM) to compare each migrant child-year with the most similar nonmigrant child-year based on the migrant child's household and community context in year $T=0$.

The PSM design mimics a randomized control trial with applicability to observational data (Hernán and Robins 2020; Rubin 1974; Winship and Morgan 1999). PSM models approximate the counterfactual framework by matching treated observations to (nearly) identical controls using observable pretreatment information. PSM models allow for the inclusion of numerous, potentially colinear control variables, an essential attribute for research on international migration, which relates to a broad set of sociodemographic and economic factors (Garip 2016; Massey et al. 1999). Treatment and control groups are well matched when differences between the two groups

[4] Our results were consistent when we excluded nonmigrant children whose parents had migrated before or after their childhood.

[5] Our results were substantively unchanged with the inclusion of these respondents as migrant children. The coefficient magnitudes reduced slightly with the inclusion of these shorter trips, but no coefficients changed in direction or significance.

on these observable characteristics are negligible (Hernán and Robins 2020). In our model, the control group represents the counterfactual in which a migrant child's parent had not migrated.

PSM uses a three-stage design to identify appropriate counterfactuals. In the first stage, we estimated a logistic regression model to determine the probability of exposure to treatment (parental migration). These results are used to generate predicted probabilities, or propensity scores, of exposure to treatment. In the second stage, the PSM matches each treated observation to the control observation with the closest propensity score. This stage ensures that each migrant child matches against a single nonmigrant child-year rather than including all 15 child-years that were available for each nonmigrant child. We enforced a fairly strict caliper range of 0.01 to ensure high-quality matches (Morgan and Winship 2015).[6] We excluded 16 treated observations (0.06%) that lacked well-matched controls. In 21 cases where migrant children matched with two identical controls (0.07%), we included both tied control observations.[7] We did not allow replacement, such that our model used each nonmigrant child-year either once or not at all.[8] Once these matches were constructed, the third stage defined the average treatment effect on the treated (ATT) as the difference in educational attainment between the treated and control observations averaged across the entire matched sample.

The ATT captures the net impact of parental migration on children's lifetime educational attainment among migrant children. The average treatment effect on the untreated (i.e., the effect of a hypothetical parental migration on a nonmigrant child) cannot be reliably estimated when the treatment is rare and not evenly distributed across the population (Morgan and Winship 2015), as is the case for parental migration. Therefore, our target outcome of interest is the ATT, which we define as the average difference between the educational attainment of migrant children and their "nearest neighbor" nonmigrant children.[9]

A benefit of nearest neighbor matching is the removal of cases that are extremely "unlike the treated" (Morgan and Winship 2015). In multiple regression models of rare events, such as parental migration during childhood, the inclusion of control observations with marginal propensity scores—that is, those that are extremely unlikely to be treated based on other observable characteristics—can inflate standard errors, which limits the interpretability of coefficient estimates (Hernán and Robins 2020). PSM solves this problem by including only control observations that have propensities to be treated and closely resemble at least one treated individual. As Smith (1997:349) explained, "[B]y focusing attention on the overlap of treatments and controls with respect to the distribution of covariates, matching effectively delimits the range of causal inference." Although this exclusion prevents us from generalizing our

[6] The caliper range defines the maximum difference in the propensity score that is allowed between treated observations and their matched controls.

[7] We weighted our sample such that all treated and unique control observations counted as one observation, and each tied control contributed one-half of an observation.

[8] Our results were substantively identical in models that allowed replacement—that is, when single controls could be paired with multiple treated observations.

[9] Morgan and Winship (2015:173–175) discussed why the average treatment effect on the untreated can rarely be estimated in observational studies.

results to the entire population, it enables precise estimation of the ATT with a well-matched control group (Angrist et al. 1996).

Because we could adjust for selection on only observed variables, we invoke the ignorability assumption (IA), which states that potential outcomes are uncorrelated with unobserved variables, conditional on observed covariates (Morgan and Winship 2015). In practice, the IA cannot be verified and should not be taken as true when applied to observational data. Instead, the quality and variety of available covariates can render the IA *more* plausible (Brand and Xie 2010).

To increase confidence in the IA, we conducted our matches within communities and included year fixed effects in our propensity score equation. As a result, contextual social, cultural, and economic structures, which influence both adults' migration behavior and children's schooling outcomes (Massey 1990; Valentine et al. 2016), cannot confound our results because these exposures are held constant between treated and control observations. We could not match within households because siblings experience identical parental migration behavior. Thus, we relied on a set of observable covariates, which Table 1 summarizes.[10] First, we included children's age, sex, birth order, and year of observation to remove concern due to age, period, sibship, and gender effects. Second, we included sociodemographic characteristics of the household head: year of birth, sex, marital status, education, internal migration history, and occupation, as well as household properties, business holdings, land holdings, and family composition. The inclusion of these characteristics mitigates the possibility that children are differentially selected into parental migration according to household sociodemographic context or class background, which are highly correlated with schooling outcomes in Mexico (Marteleto et al. 2012; Urbina 2018). Third, we included specific measures of household migration networks to adjust for the possibility that parental migration is selective on transnational ties, which increase parents' opportunities to migrate but can reduce their children's school attachment (Kandel and Massey 2002; Palloni et al. 2001). Together, these variables capture a broad range of social, economic, and demographic factors related to parental selection into migration and children's educational attainment.

To reduce our dependence on the IA assumption, we also conducted a placebo test by replicating our PSM analysis among respondents whose parents migrated after they had aged out of their primary schooling years. This test, which we report after our main findings, suggests that unobserved factors did not bias our primary conclusions.

Results

Descriptive and Multivariable Results

Before presenting our PSM results, we first examined educational attainment within the full MMP sample. Figure 3 shows that parental migration during childhood was associated with significantly lower adult educational attainment across three of our

[10] We also address the possibility of unobserved household-level confounding with a placebo test that we describe later.

Table 1 Description of variables used to estimate propensity scores

Variable	Mean/%	Min.	Max.	SD	Variable Definition
Child Characteristics					
Migrant child (%)	0	0	1	—	Parent migrated to the United States during childhood
Age (mean)	6.91	0.00	14.00	4.30	Child's age in child-year
Sex (%)	50	0.00	1.00	—	Child's sex (1 = male, 0 = female)
Year (mean)	1983	1965	2003	8.55	Year of observation
Survey year (mean)	2004	1987	2018	8.33	
Household Head Characteristics					
Year born (mean)	1945	1895	1981	11.03	Year in which the household head was born
Sex (%)	18	0	1	—	Sex of the household head (1 = male, 0 = female)
Married (%)	88	0	1	—	Marital status of the household head (1 = married, 0 = unmarried)
Education (mean)	4.47	0	23	3.95	Years of schooling of the household head
Domestic migration (%)	26	0	1	—	Household head previously migrated within Mexico
Occupation of the household head					
Agricultural	38	0	1	—	Held agricultural occupation
Unskilled	33	0	1	—	Held unskilled manual occupation
Skilled/professional	5	0	1	—	Held skilled manual or professional occupation
Household Context					
Minors (mean)	4.65	0	18	2.36	Number of minors in the household
Adults (mean)	0.62	0	17	1.46	Number of adult children of the household head
Sibling rank (mean)	3.69	1	18	2.58	
Land (%)	21	0	4	0.48	Number of land parcels owned by the household
Property (%)	62	0	6	0.52	Number of properties owned by the household
Business (%)	14	0	4	0.37	Number of businesses operated by the household
Household Migration Experience[a]					
Parent migrated	4	0	1	—	One or both parents of the household head have previously migrated to the United States
Brothers migrated	20	0	11	0.67	Number of brothers of the household head with prior migration to the United States
Sisters migrated	6	0	7	0.35	Number of sisters of the household head with prior migration to the United States

Table 1 (continued)

Variable	Mean/%	Min.	Max.	SD	Variable Definition
Number of Adult Children		38,813			
Child-Years		470,723			

[a] We did not include receipt of legal status by family members because less than 0.1% of household heads had family members with LPR status before their first U.S. migration trips.

four schooling outcomes. Of course, these bivariate associations do not account for numerous household and community-level factors that have been shown to affect exposure to parental migration (Massey and Espinosa 1997).

Accordingly, Table 2 presents two multivariable models that estimate the association between parental migration during childhood and children's educational attainment in adulthood. These models adjusted for time-invariant characteristics that we linked to each adult child's educational attainment (see note below Table 2). The first model included time-invariant demographic characteristics of children and their household heads, which are not at risk of being endogenous to parental migration. The second model incorporated community fixed effects. Both models revealed a weak positive association between parental movement and completion of lower-secondary school and a modest negative association between parental migration and continued education into and through upper-secondary school. These cross-sectional analyses identify an ambiguous association between parental migration and children's educational attainment. This ambiguity is consistent with the mixed evidence from prior cross-sectional studies on parental migration and children's education (Creighton et al. 2009; Halpern-Manners 2011; Hanson and Woodruff 2003; Nobles 2011). However, like those studies, the models shown in Table 2 ignore the contribution of the premigration context (L_0) to selection into parental migration.

These cross-sectional models also provided an opportunity to assess our invocation of the IA with the present set of control variables. Oster (2019) proposed a method for assessing the coverage derived from a set of observed covariates. Her approach compared a regression coefficient of interest between uncontrolled and controlled models to infer the "degree of selection on unobservables relative to observables that would be necessary to explain away the result" (2019:195). To assess the set of observed covariates used throughout our PSM models, we estimated the delta coefficient for Model B in Table 2. The test returned a delta coefficient of 24.435. Thus, children's education would need to be selected on unobserved factors at a rate of 24 times that of our observed variables to depress the household head migration coefficient down to 0. Such a large delta coefficient provides considerable support for our invocation of the ignorability assumption, particularly given our robust set of contextual and individual-level fixed effects. Thus, we now turn to our PSM analysis.

PSM Results: Full Sample

Table A1 in the online appendix presents the results from the first-stage logistic regression model that we estimated to generate propensity scores. We retained non-

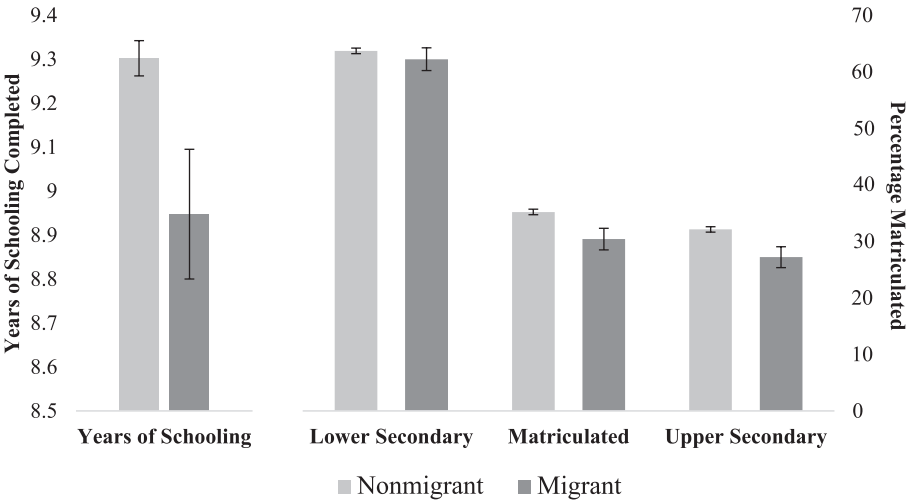


Fig. 3 Bivariate differences in educational attainment between children with parental migrants and those without observed for the full sample ($N = 38{,}813$)

significant predictors because overfitting improves PSM results (Lunceford and Davidian 2004). However, readers should not substantively interpret these coefficients. Figure A1 plots the estimated propensity scores for treated and control observations. We observed significant divergence in the propensity to have a parental migrant between treated and control cases, which confirms our expectation that migrant children are a nonrandom segment of the population. After matching each treated observation with its nearest within-community neighbor (stage 2), we calculated summary statistics for our control variables, which we present in Table A2. Table A2 also shows the percent bias between treated and control observations and treated and unmatched observations. The average bias fell by 77%, from 19.1% in the unmatched sample to just 4.4% in the treated-control sample. Thus, our propensity scores adjusted for most of the selection on household and individual-level observables. Recall that we also corrected for 100% of potential selection bias on community-level factors by matching within communities and including year fixed effects in our propensity score models.

Table 3 presents the ATTs for the full sample, and Figure 4 plots the proportional treatment effects implied by these ATTs (i.e., ATT/mean educational attainment among controls, henceforth *PTT*). The PTTs contextualize the ATTs relative to baseline educational attainment among controls—that is, the PTTs report proportional increases in education attributable to parental migration compared with the counterfactual of no parental migration. Parental migration was associated with 0.45 additional years of schooling ($p < .001$) and a 7 percentage point higher probability of lower-secondary school completion ($p < .001$). Parental migration also predicted an increased likelihood of entry into (2.97%, $p < .01$) and completion of (2.88%, $p < .01$) upper-secondary school. The PTTs, shown in Figure 4, equated to 10% to 13% increases in the probability of matriculation and completion across the three schooling levels. Thus, net of community and household context immediately before parental departure, we find evidence of a substantial positive effect of parental migration

Table 2 Cross-sectional models regressing adult children's educational attainment in the year of the survey on parental migration during childhood

	Years	Completed Lower Secondary	Began Upper Secondary	Completed Upper Secondary
A. Model 1[a]				
Household head migrated during childhood	0.00381	0.012	–0.0178**	–0.0133*
	(0.003)	(0.007)	(0.006)	(0.006)
B. Model 1 + Community Fixed Effects				
Household head migrated during childhood	0.00357	0.0156*	–0.0207***	–0.0156*
	(0.003)	(0.007)	(0.006)	(0.006)
Number of Adult Children	38,813	38,813	38,813	38,813

Note: Standard errors are shown in parentheses.

[a] Model 1 adjusts for time-invariant characteristics: children's age and sex; survey year; birth cohort, sex, and education of the household head; community size; and region of Mexico.

*$p < .05$; **$p < .01$; ***$p < .001$

during childhood on children's educational attainment in adulthood, with significant impacts identified at the lower- and upper-secondary levels.

However, this aggregate estimate reflects the average effect of parental migration on children's education across 40 years and 170 communities surveyed between 1987 and 2018. As discussed earlier, Mexico experienced rapid economic development and invested in a substantial expansion of its education system throughout this period. Thus, our aggregate estimates likely obscure contextual variations in the relationship between parental migration and children's educational attainment across time and space. To better understand when and why parental migration increases children's lifetime educational attainment, we now report a series of stratified models that capture theoretically distinct pieces of the overall relationship depicted in Table 3.

PSM Results by Parental Education, Rural/Urban Residence, and Migration Prevalence

Table 4 reports PSM results that we estimated separately by parental education (panels A and B), rural/urban context (panels C and D), community migration prevalence (panels E and F), and period (panels G and H). Figure 5 plots PTTs based on these stratified results. Parental migration during childhood was associated with substantial absolute and proportional increases in lifetime educational attainment among children whose parents did not complete primary school (fewer than six years of schooling). It predicted an increase of 0.49 years of schooling ($p < .001$) and a 7.5 percentage point (18%) increase in the probability of completing lower-secondary school among children whose parents did not complete primary school ($p < .001$). Parental migration also predicted higher rates of matriculation into and completion of upper-secondary school. Indeed, parental migration was associated with a 20% increase in the probability of completing upper-secondary school among children whose parents attained fewer than six years of schooling ($p < .001$). By contrast, we

Table 3 Average treatment effects of parental migration on children's educational attainment

Outcome	Treated	Controls	ATT	SE	*Z* Score	
Years	8.95	8.50	0.45	0.100	4.48	***
Completed Lower Secondary (%)	62.4	55.2	7.16	0.013	5.49	***
Began Upper Secondary (%)	30.5	27.5	2.97	0.012	2.46	**
Completed Upper Secondary (%)	27.4	24.5	2.88	0.012	2.48	**
N	2,222	2,222				

**$p < .01$; ***$p < .001$

found little evidence of a relationship between parental migration and educational attainment among children whose parents had completed primary school. Only the ATT for lower-secondary school was statistically significant, and the absolute and relative effect sizes among children with more-educated parents were far smaller than among their less-advantaged peers.

We also found large positive ATTs among children in rural areas ($p < .001$ for all educational outcomes). As was the case among children with less-educated parents, the PTTs increased at the upper-secondary levels, with parental migration during childhood increasing the probability of entry into upper-secondary school by 20% and increasing the likelihood of completing upper-secondary school by 22%. Similar to children with more-educated parents, we found little evidence of an effect of parental migration on educational attainment in urban areas. Three of the four ATTs did not reach statistical significance, and the absolute and relative magnitudes of the effects in urban areas were far smaller than those in rural communities. These stratified results indicate that parental migration during childhood increases lifetime educational attainment among children in more socioeconomically disadvantaged households and communities, but that it is unrelated to educational attainment among children whose parents have more education and those living in more advantaged areas.

Our stratified analysis of communities with high and low migration prevalences showed a strong effect of parental migration on educational attainment in low-prevalence communities and little to no effect in high-prevalence communities. In low-prevalence communities, we again observed the largest proportional effects at the upper end of the education spectrum, with parental migration associated with a 23% increase in the likelihood of entering upper-secondary school and a 25% increase in the likelihood of completion. These results suggest that the educational benefits associated with parental migration may be offset in communities with established cultures of migration where children often aspire to follow in their parents' footsteps rather than pursue higher education (Abrego 2014; Amuedo-Dorantes and Pozo 2010; Kandel and Massey 2002; Nobles 2011).

We also stratified our sample into those whose parents migrated before and after the passage of the Immigration Reform and Control Act (IRCA) in 1986. Despite major changes in U.S. immigration enforcement and a shift from circular to permanent migration encouraged by IRCA (Massey et al. 2002), we observed substantively similar ATTs between the two periods, although the smaller sample size reduced coefficient significance following IRCA. Nevertheless, we observed a shift in the

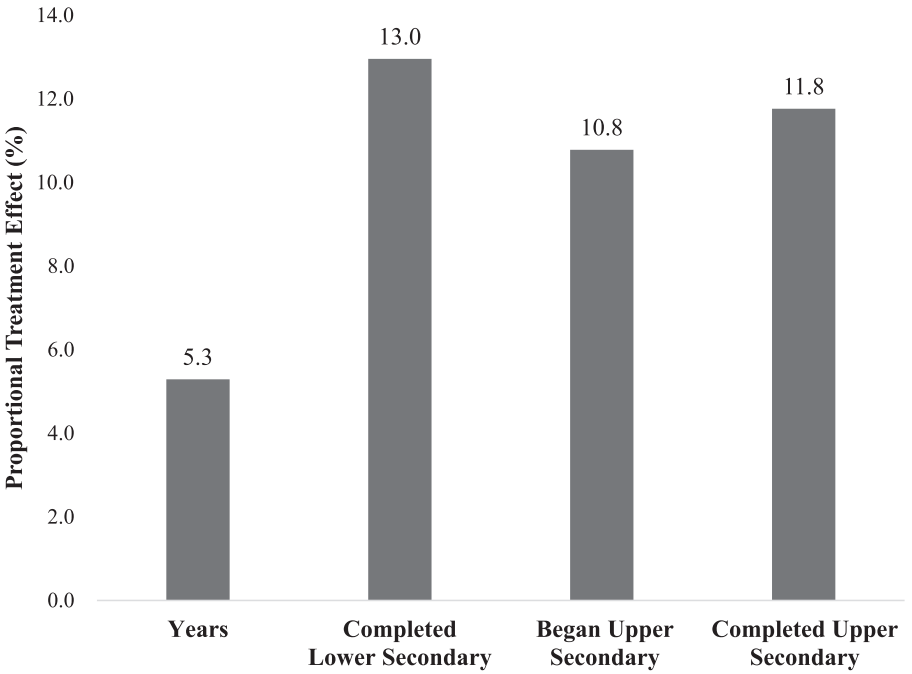


Fig. 4 Proportional treatment effects of parental migration on children's educational attainment

coefficients between the two periods. Notably, parental migration's effect on completion of lower-secondary school declined, but the effect on entry into and completion of upper-secondary school increased. These changes correspond to Mexico's educational reforms, which made lower-secondary schooling free and enhanced access to upper-secondary school (Behrman et al. 2007; Parker et al. 2007). Not surprisingly, parental migration became less important for lower-secondary school after the financial barrier was removed. The larger impact of parental migration on entry into and completion of upper-secondary school corresponds to the substantially higher rate of completion of lower-secondary school. The rise in completion of lower-secondary school created a larger population of children at risk of entering upper-secondary school. Given the larger population of potential upper-secondary school entrants and the fact that most upper-secondary schools continue to charge tuition fees, it is not surprising that we observed a larger effect of parental migration on entry into upper-secondary school in the more recent period.

Robustness Checks

We assessed the robustness of our results to various specifications, which we report here and present in section B of the online appendix. First, we estimated our models separately among male and female children to assess whether our findings varied significantly by sex (Table B1). Our results were nearly identical among men and women, suggesting that our main findings were not driven by one sex or

Table 4 Average treatment effects of parental migration on children's educational attainment by parental education

Outcome	Treated	Controls	ATT	SE	*Z* Score		Outcome	Treated	Controls	ATT	SE	*Z* Score	
A. Neither Parent Completed Primary School							B. Parent Completed Primary School						
Years	7.88	7.39	0.49	0.12	4.09	***	Years	10.70	10.71	−0.01	0.17	−0.06	
Completed lower secondary (%)	50	42	7.4	1.7	4.38	***	Completed lower secondary (%)	83	80	2.8	1.8	1.57	
Began upper secondary (%)	19	17	1.4	1.3	1.05		Began upper secondary (%)	50	51	−1.4	2.1	−0.68	
Completed upper secondary (%)	16	13	2.7	1.2	2.2	*	Completed upper secondary (%)	45	47	−1.3	2.1	−0.63	
Number of observations	1,364	1,364					Number of observations	854	854				
C. Rural							D. Urban						
Years	8.80	8.25	0.55	0.13	4.39	***	Years	9.26	8.91	0.35	0.17	2.13	*
Completed lower secondary (%)	61%	52%	9%	1.7	5.09	***	Completed lower secondary (%)	65	60	6	2.1	2.82	**
Began upper secondary (%)	29%	25%	5%	1.5	3.27	***	Began upper secondary (%)	33	35	−2	2.0	−1.13	
Completed upper secondary (%)	26%	21%	5%	1.4	3.34	***	Completed upper secondary (%)	29	30	−1	1.9	−0.36	
Number of observations	1,367	1,367					Number of observations	856	856				

Table 4 (continued)

Outcome	Treated	Controls	ATT	SE	*Z* Score		Outcome	Treated	Controls	ATT	SE	*Z* Score	
E. Low Migration Prevalence							F. High Migration Prevalence						
Years	9.67	8.85	0.82	0.15	5.59	***	Years	8.27	8.16	0.11	0.14	0.81	
Completed lower secondary (%)	72	60	11.6	1.8	6.42	***	Completed lower secondary (%)	54	50	4.0	1.9	2.12	*
Began upper secondary (%)	37	30	6.9	1.7	4.06	***	Began upper secondary (%)	24	26	−1.4	1.7	−0.83	
Completed upper secondary (%)	34	27	6.7	1.6	4.10	***	Completed upper secondary (%)	21	22	−0.3	1.6	−0.20	
Number of observations	1,111	1,111					Number of observations	1,108	1,108				
G. 1965–1986							H. 1987–2008						
Years	8.51	7.93	0.57	0.12	4.61	***	Years	10.06	9.82	0.24	0.21	1.71	
Completed lower secondary (%)	57	49	8.0	1.5	5.45	***	Completed lower secondary (%)	76	74	2.2	2.3	1.33	
Began upper secondary (%)	26	21	4.3	1.3	2.68	***	Began upper secondary (%)	43	38	4.8	2.4	2.15	*
Completed upper secondary (%)	22	18	4.5	1.2	3.11	***	Completed upper secondary (%)	40	35	4.8	2.4	1.98	*
Number of observations	1,583	1,583					Number of observations	640	640				

*$p < .05$; **$p < .01$; ***$p < .001$

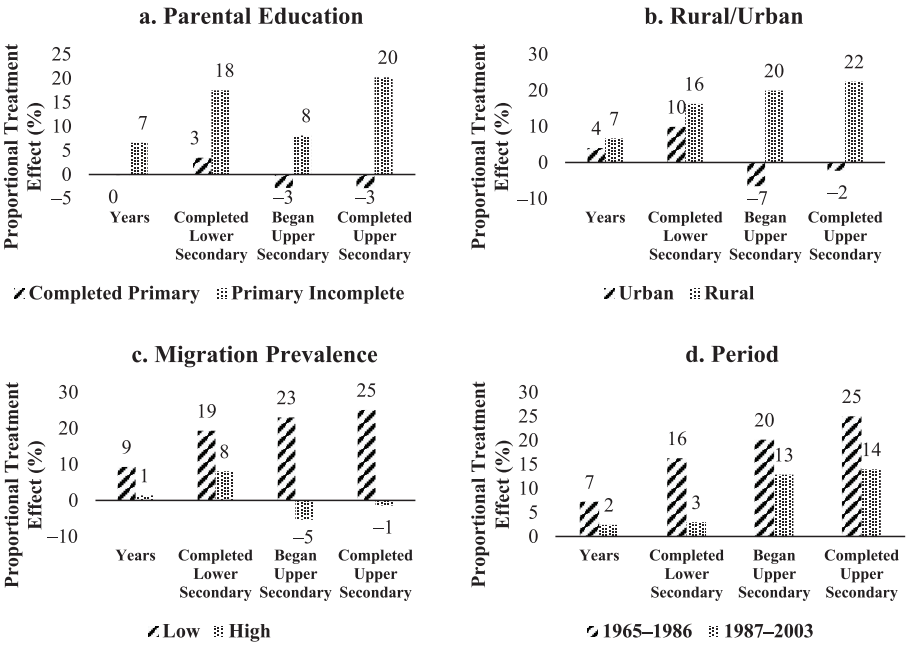


Fig. 5 Proportional treatment effects of parental migration on children's educational attainment by parental education, rural/urban context, community migration prevalence, and period

the other. Second, we replicated our PSM models, with each treated case matched to their three or five nearest neighbors (Table B2). Consistent with expectations (Morgan and Winship 2015), these expanded control samples resulted in stronger significance from the larger sample sizes but smaller ATTs due to the less precise matches. Third, to check for potential issues with the quality of our matched propensity scores, we computed ATTs conditional on the variable with the most substantial post-treatment bias: brother of the household head with migration experience (see Table A2). We estimated a PSM model that excluded respondents whose household head had a brother with prior migration experience. Table B3 reports these results (panel A). Fourth, we replicated our study results without including households that were interviewed in the United States (Table B3, panel B). These results indicate that our main findings were not driven by unusually high educational attainment among children who joined their parents abroad. Fifth, we restricted our definition of adult children to include only those 25 years old or older (Table B3, panel C). Again, these models confirmed our main findings. Among these older respondents, we also found evidence that the positive effect of parental migration on schooling persists at the postsecondary level, with parental migration associated with a 2 percentage point (25%) increase in the probability of entering and completing college ($p < .05$). Collectively, these supplemental analyses provide confidence that our findings did not result from model misspecification or biased sample construction. However, our primary conclusions still rest on the IA assumption that unobserved factors not included in our PSM estimation did

not affect our findings. To support a causal interpretation of our analysis, we turn to the results from our placebo test.

Placebo Test

Our primary analyses could not adjust for unobserved household characteristics, which may correlate with the likelihood of parental migration (U_0 in Figure 1) and children's lifetime educational attainment. U_0 could include genetic traits passed down across generations or the quality of parent-child relationships, both of which could reasonably covary with parental migration and affect children's schooling (Abrego 2014; Dreby and Stutz 2012; Hagan et al. 2015). For example, if international migrants are innately ambitious and risk-taking, those same traits might motivate their children to excel in school regardless of the benefits that are directly attributable to their parents' migrations. To assess the influence of these or other unobserved household-level characteristics, we conducted a placebo test (Hartman and Hidalgo 2018; Heckman et al. 1987). In econometrics, placebo tests involve showing that the effect of interest does not exist when it "should not exist" (Rothstein 2010).

In our case, we considered the effect of parental migration on children who had aged out of lower-secondary school before their parents' first departures. We replicated the PSM analyses presented in Table 3 and Tables A1 and A2 (online appendix) among children aged 15 years or older when their parents first migrated to the United States.[11] If parental migration affects children's progression through lower-secondary school and into upper-secondary school via the remission of financial resources, changes in parent-child relationship quality, or other factors that are directly attributable to international movement, then parental migration should be uncorrelated with educational attainment among adult children who were beyond lower-secondary school at the time of parental departure. On the other hand, if parental migration is associated with children's educational attainment because of unobserved household characteristics, we would expect to observe similar associations between parental migration and the educational attainment of all children regardless of their age at the time of migration: in that case, parental migration would simply provide an indicator of other unobserved and exceptional characteristics of sending households that are shared among their inhabitants.

Table 5 presents the placebo test results. Among these older children, none of the ATTs were statistically significant, and all the coefficients were small in magnitude. Comparing the placebo results with our main findings presented in Table 3, the size of the ATTs ranged from 0% to 30% of the size of the coefficients in our study sample, with an average of 12%. These results fail to reject the null hypothesis that unobserved household characteristics related to selection into migration do not explain the positive association between parental migration during childhood and children's educational attainment in adulthood. The null results from our placebo test strengthen the IA that underpins our PSM models and support a causal interpretation of our main findings.

[11] The PSM yielded good matches among these older children, with a mean bias of just 5%. There were not sufficient observations to reliably restrict the sample to children who were even older than 15, such as 20+, when a parent first departed for the United States.

Table 5 Average treatment effects of parental migration on children's educational attainment among children who were beyond their schooling years at the time of parental departure

Outcome	Treated	Controls	ATT	SE	*Z* Score
Years	8.7266	8.60	0.132	0.188	0.7
Completed Lower Secondary (%)	60	58	1.8	2.3	0.77
Began Upper Secondary (%)	27	27	–0.1	2.0	–0.07
Completed Upper Secondary (%)	24	24	0.0	2.0	0
Number of Observations	684	684			

Note that the results from our placebo test could stem from differences between households in which parents migrate while their children are young versus those where migration occurs after children have aged out of lower-secondary school. Because we cannot capture these variations beyond the observed control variables, our placebo test does not supply ironclad evidence of causality. Rather, it offers another source of support for causality, one that augments our propensity score analysis and the forgoing robustness checks.

Conclusion

We investigated the effect of parental migration on children's lifetime educational attainment by matching adult Mexicans whose parents migrated during childhood against adults without migrant parents who grew up in nearly identical households within the same communities. Data from the MMP enabled us to include both resident and nonresident children of the household head in our analysis. We found that parental migration during childhood predicted 0.45 additional years of schooling and increased both the probability of completing lower-secondary school by 7 percentage points and the likelihood of entering and completing upper-secondary school by nearly 3 percentage points (an increase of more than 10% relative to counterfactual nonmigrant children). The effects of parental migration on children's schooling were most substantial among those who grew up with more considerable disadvantages. Among children whose parents did not complete primary school, parental migration increased the likelihood of entry into and completion of upper-secondary school by 18% and 31%, respectively. Parental migration increased the likelihood of attaining these educational milestones by 24% and 23%, respectively, among children who grew up in rural communities. In contrast, we found little evidence that parental migration affects schooling among children with more-educated parents and those who grew up in urban areas.

We also assessed whether unobserved factors that we did not include in the matching algorithm could have biased our results. We conducted a placebo test in which we replicated our PSM analysis among children who were at least 15 years old in the year before their parent's first U.S. migration. If parental migration and higher schooling outcomes among children result from unobserved household characteristics, then we would expect to observe similar results among older children—that is, those who were no longer of school age when their parents first migrated. The placebo test, which yielded null results with substantively small magnitudes, increased

our confidence in the IA that underpins our PSM models. The null results of the placebo test support a causal interpretation of the positive associations between parental migration during childhood and children's total educational attainment measured in adulthood.

Our results clarify the mixed evidence generated by studies that applied causal methods to cross-sectional data. Collectively, these causally inclined analyses find that relative to their peers, children with currently absent parents or those living in households supported by remittances are less likely to matriculate throughout lower- and upper-secondary school or attain more years of schooling. They are also both more and less likely to enter the workforce before turning age 18 (Amuedo-Dorantes and Pozo 2010; Halpern-Manners 2011; Hanson and Woodruff 2003; McKenzie and Rapoport 2011). These mixed results likely reflect the multiple direct and indirect impacts of parental departure. Children may be forced into the labor market temporarily while their parents repay smuggling debts and secure employment abroad, but children can also benefit from educational opportunities via the remittance of foreign earnings (Curran et al. 2004; Hu 2013; see also Figure 2). Studies that observed currently absent parents could not capture the long-term financial implications of parental migration. Our study's retrospective longitudinal results corroborate previous noncausal studies, which found that the benefits of these educational investments offset short-term costs of parental departure (Chang et al. 2011; Creighton et al. 2009). One extension of our project would be to examine school-work-school sequences among migrants' children. These sequences could highlight critical points for intervention to reduce school dropout and maximize the benefits of parental migration.

Beyond our study's substantive contributions, we also introduced a novel propensity score approach that can link events occurring during childhood to temporally distal outcomes measured years or decades later within a counterfactual framework. Longitudinal data is limited in developing countries, and existing panels most commonly track individuals or households across only a few years. Our approach suggests that community-based studies that collect retrospective information about households could be used to estimate long-term effects of household member migration and other major life events for various long-term outcomes among the migrants and other household members. These studies could explore the consequences of migration during childhood for children's future marital behavior, childbearing, occupational attainment, long-term health, and migration behavior.

Despite these advantages, retrospective panel data can introduce sampling bias (Assaad et al. 2018; Beauchemin 2014). Retrospective surveys that rely on community sampling yield biased samples to the extent that current residents (those present at the time of the survey collection) differ from ex-residents—those who were present during some portion of the household-years included in the survey but relocated before the actual moment of the survey (Riosmena 2016). Moreover, because the MMP collects information about nonresident children, our results include children who relocated before survey collection—either internally or internationally. However, retrospective studies such as the MMP will lose entire households that relocate internally or to U.S. communities other than those surveyed by the MMP. Thus, our coefficients should be interpreted as the effect of parental migration on children's edu-

cation within households with at least one long-term resident, either in the sampled Mexican community or its primary sister community in the United States.[12] Large-scale panel studies, such as the Mexican Family Life Survey (MxFLS), could be used to overcome this limitation by following households from an initial set of communities as their migration trajectories unfold internally and abroad. Still, these studies present their own limitations, such as the high cost of data collection and observation of only one cohort. We hope that future scholars will address the unavoidable limitations herein as we continue to advance this important area of research.

Our study's findings have policy implications. In areas with high rates of out-migration, children would benefit from more academic and social support in the classroom to offset some of the short-term costs of family separation. This support would be particularly beneficial in rural communities where parental migration is prevalent, and children often face greater pressure to enter the workforce before finishing their studies. Empirical analyses have highlighted the unique educational challenges faced by children with migrant parents (Abrego 2014; Curran et al. 2004; Zhou et al. 2014). Yet, our results suggest that when children overcome short-term challenges associated with parental departure, migration can lead to greater lifetime scholastic attainment. This nonlinear association suggests that targeted interventions aimed at the period surrounding parental departure could mitigate temporary school dropout and enhance long-term educational gains among migrants' children. For example, policymakers could partner with researchers to test the effectiveness of short-term loans for migrants, intended to bridge the gap between border-crossing and foreign employment. This policy could be implemented in partnership with the United States' H-2 visa programs, which include a rapidly expanding proportion of Mexican migrants with low educational attainment (Hernández-León 2021). ■

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Author note The authors contributed equally.

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[12] Household respondents provide educational information for nonresident children of the household head. Thus, our results capture the effects of parental migration on educational attainment among children who relocated between the moment of parental migration and survey completion.

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