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ECONtribute
Discussion Paper No. 313

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June 2024

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UNIVERSITÄT BONN
Universität zu Köln

Funding by the Deutsche Forschungsgemeinschaft (DFG, German Research Foundation) under Germany's Excellence Strategy – EXC 2126/1-390838866 is gratefully acknowledged.

Childhood Migration and Educational Attainment: Evidence from Indonesia*

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This version: June 18, 2024

Abstract

Millions of families migrate every year in search of better opportunities. Whether these opportunities materialize for the children brought with them depends on the quality of the destination that their parents selected. Exploiting variation in the age of migration, I analyze the impact of destination quality on the educational outcomes of childhood internal migrants in Indonesia. Using Population Census microdata from 2000 and 2010, I show that children who spend more time growing up in districts characterized by higher average educational attainment among permanent residents tend to exhibit greater probabilities of completing primary and secondary schooling. Moreover, educational outcomes of migrants converge with those of permanent residents at an average rate of 1.7 to 2.2 percent annually, with children from less educated households benefiting more from additional exposure. My findings suggest substantial heterogeneity of returns to childhood migration with respect to destination.

*E-mail: *hschwank@bu.edu*. The author is grateful to Samuel Bazzi for advice and suggestions, and to James Feigenbaum, Robert Margo, Daniele Paserman, participants at the 2019 NovAfrica PhD Workshop on Migration and Development, the 2nd CReAM/RWI Workshop on the Economics of Migration, and the CEMIR Junior Economist Workshop on Migration Research, and four anonymous referees for helpful comments. I also thank Chen Lin and Patricia Mawar Puspitasari for the excellent research assistance. This research is funded by the Deutsche Forschungsgemeinschaft (DFG, German Research Foundation) under Germany´s Excellence Strategy – EXC 2126/1– 390838866 and through CRC TR 224 (Project A03).

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Keywords: Internal Migration; Education; Development; Indonesia.

JEL Classification Numbers: I25, O15, D64.

1 Introduction

Internal migration is an important strategy for individuals seeking better opportunities. In making the migration decision, families not only take into account potential gains for the working generation. Whether better opportunities materialize for the children brought with them depends on the quality of the destination that their parents selected. Children who move to environments more favorable to learning benefit from migration, while those who move to adverse environments may experience negative effects from migration. Existing research implies positive effects of rural to urban migration. However, despite increasing urbanization rates, most destinations of internal migrants are rural, in particular in developing countries. It is therefore crucial to study heterogeneous returns to migration with respect to destination, going beyond the rural-urban distinction.

Using data from Indonesia, I show that destination quality, measured as average educational attainment among permanent residents, is a key determinant for the returns to childhood migration. Destination quality can vary substantially in Indonesia, which is characterized by a rapidly growing economy and increasing urbanization rates. Similarly, educational attainment has increased tremendously with illiteracy rates now in the low single digits and nearly universal primary school education (**?**). This progress is attributed to one of the world's largest school construction programs in history and a large and vibrant religious education sector that caters to poorer and conservative families often left out of formal education markets in the Muslim world (**?**). The country and its highly internally mobile population therefore provide a well-suited context to investigate the importance of migration destination. [1]

To explore the role of destination for the effects of internal migration, I apply **?**'s (**?**) age-at-migration research design. By exploiting differences in the *timing* of migration, the design can identify the causal effect of location on education, even in the presence of sorting into destination. I compare the effect of destination quality between children who migrated

[1]Education is a relevant metric to assess the effect on long-run economic standing: **?** estimates the economic returns to a year of education in Indonesia to be between 6.8 and 10.6 percent.

at different ages - thereby being exposed to the destination for different amounts of time - to estimate the causal location exposure effect on education.

Drawing on Census microdata covering the entire Indonesian population in 2000 and 2010, I find that children who migrate earlier to a better district are more likely to have completed primary school and have higher graduation rates from junior and senior secondary school.[2] My findings show that educational outcomes of migrants converge to those of permanent residents at a yearly rate of 1.7 to 2.2 percent. This implies a 16-year-old's chance of junior secondary school graduation increases by about one percentage point with each year that she migrates earlier from an origin at the 10^{th} percentile to a destination at the 90^{th} percentile.[3] Children with less educated parents benefit more from additional exposure than children of parents with at least a junior secondary school degree, underlining the importance of internal migration as a channel for upward intergenerational mobility.

The identifying assumption of the age-at-migration design is that unobservable family characteristics are orthogonal to the timing of migration. I relax this assumption by identifying the exposure effect from children who belong to the same household. The addition of household fixed effects would decrease the estimated coefficients if selective migration was driving the exposure effect. However, I find that they remain stable or even increase: The baseline specification finds that junior high school graduation rates of migrants converge to those of permanent residents at a yearly rate of 2 percent, while the inclusion of household fixed effects leads to an estimate of a 2.9 percent convergence rate. The change is consistent with some families migrating to a district with more or better schools with the aim of their child's continued education. This type of selective migration would downward bias the exposure effects estimate in the baseline specification without household fixed effects. In addition, following **?** I estimate that

[2] Districts are sometimes translated as "regency", and the Indonesian terms are *kabupaten* (rural) and *kota* (urban). Districts are comparable to commuting zones in the US. I refer to districts with better educational outcomes among non-migrant children as "better" districts without judgment about other district characteristics.

[3] In comparison, **?** finds that each primary school constructed per 1,000 children under the Indonesian INPRES program between 1973 and 1978 increased schooling by 0,12 to 0,19 years on average.

selection on unobservable characteristics would need to be three times higher than selection on parental education in order to entirely explain away my results. In several other exercises, I show that my results are unlikely to be driven by a violation of the identifying assumption, and that they are robust to a broad range of sample restrictions and other checks.

To further characterize exposure effects, I conduct my analysis separately by gender and find that girls benefit more from exposure to better places than boys. In addition, exposure to a better quality district matters more for children from less educated parents. The measure of district quality used in this paper is based on educational outcomes of permanent residents, that is, children who spent their entire life in the same district. Conditional on this measure, urban exposure has a negative effect, consistent with higher crime rates and other urban dis-amenities. Finally, I find no evidence that the effect works through the prevention of early marriage, and I find no effect on health outcomes.

This paper expands the literature on the returns to internal migration, and makes a methodological contribution to the literature on location exposure effects.

First, the literature on the returns to internal migration (**?**;**?**; **?**; **?**; **?**), mostly focuses on the working generation. Recent work points at positive effects of internal migration for children which predominantly benefits those who leave rural areas and move to cities (**?**; **?**; **?**; **?**; **?**;). My paper takes the question about heterogeneous returns to migration further by constructing destination-, cohort-, and household education-specific quality measures. In addition, previous work relies on small longitudinal surveys, while I am able to identify causal destination exposure effects from cross-sectional Census data. **?** attempts to control for selection into migration by comparing the children of migrants to the children of their non-migrant siblings. While this strategy partially controls for shared genetic and family background factors of the parental generation, it does not necessarily alleviate the concern of selection bias: It is likely that the sibling who decided to migrate to a city did so due to unobservable characteristics related to their willingness and ability to invest in their children's education. Acknowledging the challenge of controlling for selection, I focus on the returns to different destinations among internal migrants instead. I uncover that children do benefit substantially from moving to the right places, and that this effect is not captured (primarily) by urbanization.

Second, I complement the literature on location exposure effects by studying the role of destination in a large developing economy, and by providing a novel approach to derive the timing of migration in a data-sparse context. Experimental studies focus on evaluating relocation experiments in developed countries, such as the Moving to Opportunity for Fair Housing (MTO) project in the US (**?**; **?**; **?**) and other experiments or quasi-experiments like housing demolition (**?**;**?**). Those studies find positive effects on health outcomes, but evidence regarding economic effects is contradictory. **?** introduce a method to identify causal location exposure effects in observational data. The paper studies intergenerational mobility in the United States and shows that outcomes, such as income and college graduation rates of individuals who migrated to better neighborhoods during childhood, improve linearly in proportion to the number of years they were exposed to the destination. **?** replicates the analysis with Australian data and finds that exposure during teenage years is particularly important. In recent work, **?** and **?** show that exposure to place also matters in developing countries. I add to this growing body of literature by exploring location exposure effects on education in Indonesia. I study the effect on several age-specific educational outcomes from primary and secondary school completion of children to the number of years of educational attainment of adults. In addition, I provide evidence for the persistence of childhood exposure effects and their relevance for labor market outcomes later in life.

My methodological contribution is by developing a novel approach to derive the timing of childhood migration based on the timing of siblings' births in cross-sectional data. Panel data, and even cross-sectional data with sufficient information on individuals' migration histories are rare, in particular in developing countries. My proposed approach only requires information on the birthplaces and birth years of all children in the household, but it can be augmented by additional migration variables. It therefore enables the implementation of the age-at-migration approach or other research designs based on migration timing in non-data-rich contexts. In extensive checks, I show that the approach performs well in comparison to approaches which rely on more detailed migration information, such as the data used in **?** and **?**.

This paper is organized as follows. The next section gives an introduction to the educational system in Indonesia and an overview of internal migration patterns. Section 3 explains the empirical strategy and describes the data. Section 4 first presents estimates of the destination exposure effect of childhood migration on education. It further characterizes the location exposure effect by presenting heterogeneity analyses exploring urbanization and schools as relevant factors. Then, I discuss threats to identification and show several robustness checks. The section finishes with a discussion of migrant families' destination choices. Section 5 concludes.

2 Background: Indonesia

This section first gives an overview of the educational landscape, and then provides some background on the history and characteristics of internal migration in Indonesia.

2.1 Education

In Indonesia, children start school at the age of six or seven[4] and go to primary school for six years. Subsequently, junior secondary school (*Sekolah Menengah Pertama*, or JUNSEC) takes three years and senior secondary school another three years (*Sekolah Menengah Atas*, or SENSEC)[5]. In 1973, the Indonesian government started a massive school construction program using revenues from the early 1970s oil boom. Under the program, 61,807 new schools were constructed between the school years 1973–1974 and 1978–1979, which approximately doubled

[4] The official school starting age is seven, but parents are allowed to enroll their children at the age of six since 2003 (**?**). However, even as early as 1995 about half of the six-year-olds were reported going to school in the Intercensal Population Survey (1995 SUPAS).

[5] After junior secondary school, students can also attend vocational high school (*Sekolah Menengah Kejuruan*, or SMK) which in the Census will be recorded as "senior secondary school".

the number of schools **?**. The goal was to put a primary school in every village. The program increased primary school enrollment considerably and also allowed overage students to obtain elementary education.[6] From 1994 onwards, the government has required nine years of compulsory education. However, while junior secondary school enrollment rates have increased over time, even in 2010 only 68 percent of the eligible cohorts attended junior secondary school (**?**).

Geographic differences in education are substantial, and the availability and quality of junior secondary schools is particularly problematic in isolated rural areas (**?**). In 2010, the adult population (age 15 and older) had an average 10.9 years of education in the province of Jakarta, but only 6.7 years of education in the province of Papua. The share of junior secondary school teachers holding the minimum qualification (a four-year degree) ranged from 38 percent (Maluku) to 89 percent (East Java) during the school year 2009/2010 (**?**). While socioeconomic background is an important determinant of educational attainment for children, gender is not. Net enrollment rates are higher for girls than for boys in junior secondary school, and similar in senior secondary school. While in the 1990s there were considerably more male students pursuing tertiary education, women have overtaken men during the 2000s and now have higher enrollment rates (**?**) in universities.

2.2 Internal Migration

Indonesia has a long history of internal migration flows. Already in colonial times, landless households from densely populated areas were moved to sparsely populated islands as part of the "Transmigration Program". This government program was continued by the Indonesian government under Suharto, and saw a massive expansion in the late 1970s. Just in the years between 1979 and 1983, 1.2 million people were moved from the *Inner Islands* Java and Bali to the *Outer Islands* Sumatra, Kalimantan, Maluku, Nusa Tenggara, and Papua (**?**), which makes it one of the largest resettlement programs worldwide. The aim was to reduce perceived over-

[6]See **?** for the program's direct effects on educational attainment, and **?** and **?** for its long-run and intergenerational effects.

population of the Inner Islands, and to provide poor families with land. Migrants were mostly volunteers, but did not have a say in the selection of their destination.

While the Transmigration Program experienced its peak in the early 1980s, non government-organized internal migration remained important. In 2000, 5.9 percent of the population age five or older reported in the population census having migrated across district borders in the past five years. Most migrants in the Outer Islands are from Java, which saw negative net migration flows both in 1995 - 2000, and 2005 - 2010. Migrants are more likely to be prime age (between 15 and 34) and male, although young women aged 15 to 24 are an important migrant group, who likely leave their home upon marriage. Indeed, among married migrants in 2010, almost 60 percent were women, whereas almost two thirds of single migrants were men. In addition, migrants are more educated than non-migrants. Among young migrants, the labor force participation rate is higher than among young non-migrants, but this changes after prime age. Finally, migrants are significantly more likely to be "employees" (**?**).

The decision whether and where to migrate is complex and depends on many factors. **?** finds that in Indonesia, migration after negative shocks, for example after natural disasters, is often characterized by temporary moves to nearby rural destinations. This ex-post risk coping strategy is often used by households with lower levels of wealth. On the other hand, migration can also be characterized as an investment strategy and is more likely to occur after a positive shock. In that case, the destination is more likely to be urban. In addition, investment migration takes place over longer durations and to further away destinations. The need to save up for costly migration is also reflected in the findings by **?** who show that labor productivity would increase by 22 percent if all barriers to internal migration in Indonesia were removed.[7]

[7]Similarly, **?** finds that the income elasticity of migration is heterogeneous with respect to wealth: Positive income shocks increase labor emigration from Indonesia on average, underlining credit constraints, but persistent income shocks reduce emigration in the most developed rural areas.

3 Empirical Strategy

This section first describes the estimation strategy, and then discusses the three data sources, which are the Indonesian Population Census, the Intercensal Population Survey (SUPAS), and the Village Potential Statistics (PODES). It then goes on to discuss the age at migration estimate used in this paper.

3.1 Estimation Equations

To identify the effect of migrating to a better destination, the simple approach would be to regress an individual's educational attainment on some measure of destination quality. However, several issues arise. First, the effect of a destination may depend on the duration of an individual's stay, suggesting that longer periods in a favorable environment could have a stronger impact than shorter ones. Second, it is likely the estimate would be biased, because families with higher ability or willingness to invest in their children's education could be more likely to move to better places. Finally, what is relevant for a counter-factual is not only the quality of the destination but whether and how much better it is than the origin.

I address each of these three empirical challenges by using the age-at-migration approach developed by **?**. I model the impact of childhood migration on educational attainment in Indonesia similar to **?**'s (**?**) semi-parametric specification:

$$y_i = \sum_{m=1}^{M} \beta_m \mathbb{1}(m_i = m)\Delta^S_{odcp} + \alpha_{oc} + \phi_{pm} + \mathbf{X}'_p\boldsymbol{\xi} + \quad \epsilon_i \tag{1}$$

First, $\hat{\beta}_m$ are the M coefficients of interest which measure the location effect at age of migration m separately, allowing for the length of exposure to matter. Second, I will assume that the bias from selection into destination is unrelated to the *timing* of migration[8]. That is, while higher ability families might be more likely to migrate to better areas, they do not do so specifically when their children are *younger*. Under this assumption, it is possible to identify the *location*

[8] I show in Section 4.4 that my results are not driven by a violation of this identifying assumption.

exposure effect with observational data. If there is a causal effect of location on the migrant child, then the effect should be stronger the longer a child was exposed to the location. I simply take the difference of two adjacent β_m coefficients and define the location exposure effect at age m as $\gamma_m = \beta_m - \beta_{m+1}$. $\hat{\gamma}_m$ estimates the effect of spending an additional year at the destination at age m. Since the selection bias differences out by assumption, it is an unbiased estimate of γ_m.

Third, I define a quality measure that expresses the change in location quality as $\Delta^S_{odcp} = \bar{y}_{dcp} - \bar{y}_{ocp}$, where $\bar{y}_{dcp}$ and $\bar{y}_{ocp}$ are educational attainment of category S of permanent residents in destination district d and origin district o, respectively.[9] Depending on the outcome of interest, this will be the share of junior secondary school graduates or similar measures of educational attainment. I construct the measure for each birth cohort c separately, because the environment for a child could be very different in the same place depending on when they grew up there. It also distinguishes between children of parents p with low and high education as they might face different opportunities.

By including origin $\times$ birth-cohorts fixed effects α_{oc} in Equation (1), each $\hat{\beta}_m$ is identified from comparing children at different destinations who were born in the same year and grew up in the same district. Age-at-migration $\times$ parent-education group fixed effects ϕ_{pm} account for interruption effects of migration and other age- and parent-education specific unobserved factors.[10] The vector X_p controls for parental education in the most flexible way possible: highest completed degree of the household head interacted with gender of household head, and highest completed degree of their spouse interacted with the spouse's gender. I two-way cluster standard errors at the origin and the destination level.

[9] Permanent residents are children who have not moved at all during childhood.

[10] **?** additionally include an interaction of location quality and birth cohort, because their ability to measure location varies systematically across children's birth cohorts. This is not the case in my data.

3.2 Data

3.2.1 2000 and 2010 Complete-Count Population Census

The data for the main analysis sample is drawn from the 2000 and 2010 complete-count Population Census microdata samples.[11] It covers the entire Indonesian population of about 195 million individuals in 2000 and 234 million in 2010. I restrict my sample to children and young adults between the age of 12 and 20 who live with their parents[12]. The lower bound coincides with the age of primary school graduation and the upper bound is chosen to minimize cohabitation bias. This bias might arise if children who cohabitate with their parents are systematically different from those who do not. Given that cohabitation rates decrease with children's age, I expect the potential bias to be stronger for older children. In Section 4.5 I show that the baseline results remain unchanged after restricting the sample to children under the age of 15 who have cohabitation rates above 95 percent. Since the borders of Indonesian districts have been altered over time, in particular through district splitting, I fix districts to the 2000 borders to obtain harmonized neighborhoods, leading to 341 districts in each year.[13]

Educational attainment is captured in the Census by a categorical variable taking the values "No education/less than primary school", "primary school completed", "junior secondary school completed", "senior secondary school completed", and several distinctions for tertiary education. Unfortunately, the variable only shows *completed* levels of schooling. Hence, a student who went to senior secondary school but did not graduate will be reported as a junior secondary school graduate. I construct several binary variables:

1. *Primary12+* = 1 if an individual aged 12-20 has at least primary school completed.

2. *JUNSEC15+* = 1 if an individual aged 15-20 has at least completed junior secondary school.

[11] The 2000 Census data was obtained from Samuel Bazzi through Data Sharing Agreement with SMERU Research Institute. I use a version of the 2010 Census that is available at the Harvard Library Goverment Documents Group.

[12] In both years, about 30 million children between 12 and 20 can be found to live with their parents.

[13] Data from district 6109 is only available in 2000, and data from district 1110 is only available in 2010.

3. *SENSEC18+* = 1 if an individual aged 18-20 has at least completed senior secondary school.

The measures of district quality $\bar{y}_{ocp}$ and $\bar{y}_{dcp}$ are, therefore, defined as the share of permanent resident children who graduated from primary school (*Primary12+*), junior secondary school (*JUNSEC15+*) or senior secondary school (*SENSEC18+*) in the relevant age group of origin o or destination d.[14] The shares are separately calculated for each cell defined by birth cohort c and parent education group p.[15]

This measure of district quality has several advantages, for example low data requirements, because it does not rely on additional variables, and by being cohort specific, it captures variation in conditions that change quickly over time. Maps of district-level graduation rates among permanent residents reveal considerable spatial variation in graduation rates, even in districts of close local proximity, as well as variation over time and across parental education (Appendix Figures A.1, A.2, A.3 and A.4).[16]

3.2.2 Additional Data Sources and Variables

I complement the analysis with district-level data from other data sources, which I use to construct alternative district quality measures in Section 4.3. In addition, Appendix Section C describes in detail a second sample that I construct from the Intercensal Population Survey (SUPAS).[17]

SUPAS and Census Subsamples. I retrieved from IPUMS (**?**) the 1985, 1995, and 2005 SUPAS waves and 10% subsamples of the 1980, 1990, 2000 and 2010 Census waves to calculate popula-

[14] I define permanent residents as those children who have been born in their current district, who have no siblings being born in another district, and who have not been in a different district five years prior.

[15] I divide parents into two education groups, where the lower group includes parents without junior secondary degree whereas in the high education group at least one parent has at least junior secondary school completed. For precision, I only include observations where both $\bar{y}_{ocp}$ and $\bar{y}_{dcp}$ are based on at least 30 permanent residents per cell. Section 4.5 shows the robustness of my baseline results relative to variations of this restriction.

[16] The autocorrelation between $y_{po,c}$ and $y_{po,c-1}$, with y being the share of permanent residents aged 15 to 20 who graduated from junior secondary school, is 0.89 for children from low parental education households, and 0.86 for children from high parental education households. The correlation between y_{oc0} and y_{oc1}, with $p = 0$ indicating low parental education and $p = 1$ indicating high parental education, is 0.85.

[17] Section H presents results from this additional analysis.

tion size, the share of urban households and the population density per district. For the latter, I obtained district shapefiles from IPUMS IHGIS (**?**).

Village Potential Statistics (PODES). In the Village Potential Statistics (PODES), village heads provide information about village/city neighborhood characteristics. This data is collected approximately every three years and covers all villages in Indonesia. I use the 1980, 1983, 1986, 1990, 1993, 1996, 2000, 2003, 2005, 2008, and 2011 waves to obtain data on the number of junior secondary schools. I aggregate the number of all school types (depending on the wave, the data distinguish between public and private schools, for example), and of all villages to obtain a district level measure. To account for different district sizes, I calculate the number of schools per 1,000 inhabitants recorded in the PODES. To account for differences in the age structure of districts, I also construct a variable measuring the number of junior secondary schools per 1,000 children of age 7 - 15. Since this number is not available in all PODES waves, I use the number measured in the closest SUPAS or Census wave.

3.3 Age at Migration Estimate

The age-at-migration design requires identifying children who migrated exactly once, and both knowledge of the origin and destination as well as the age of the child at the time of migration. Like most Censuses globally, the Indonesian Censuses that I use here cover migration history in a rather sparse way. The only information besides the current district of residence is the birth district and the location of residence 5 years prior. In the spirit of **?**, I exploit differences in the place and year of birth among siblings. To identify children who migrated exactly once, I select those families who have at least one child being born in the current district, and at least one child born in a different district. In most families, all children were born in one district; in fact, this is the case for 94 percent of all households with children in 2000 and 95 percent of all households with children in 2010.

The birth years of the two adjacent children - ordered by age - who are born in different districts determine the upper and lower bound of the year of migration of their older siblings.

I construct an estimate as the midpoint of both birth years.[18] From this I can infer an estimate of the age at migration for each child born in the origin district. I show that the magnitude of my baseline results remains unaffected after placing restrictions on the maximum difference of the two adjacent birth years (Section 4.5). The baseline restriction is a maximum birth year gap of eight years and therefore a maximum error of four years.

I select all children who have migrated exactly once in line with this definition. In addition, I use the information on residence five years prior to increase the precision of the year of migration estimate, and discard individuals who migrated more than once according to this additional information.[19] Section D in the Appendix discusses the representativity of the regression sample.

How good is the age at migration estimate based on siblings' births, and which fraction of childhood migration do I miss? To validate the siblings-based migration estimate, I replicate it using the 1995 and 2005 SUPAS waves. I then compare it to the richer information in the SUPAS which contains information on the previous district and the duration in the current district, therefore allowing to directly identify one-time migrants and their age at migration.[20] The detailed discussion in Section E in the Appendix shows that, while the siblings-based approach under-counts the number of one-time migrants, it does not do so in a systematic way. In addition, the measurement error from inferring the timing of migration from siblings' birth year gaps is moderate, centered around zero, and not systematic with respect to parental education. It does improve with the number of children in the household, and when tightening the restriction on the maximum age gap between the relevant siblings.

[18] If the midpoint is not a round number, the estimate will be assigned the "earlier" year, i.e. if one sibling was born in 1996 and one in 1999, the mean is 1997.5 and the migration year estimate will be 1997.

[19] If, for example, a family migrated between 1993 and 1997, the residence in 1995 variable can shrink these bounds to 1995 to 1997 or 1993 to 1995, depending on whether the family lived in the origin or the destination district in 1995. If they lived in a district that is different to both origin and destination, they must have migrated at least twice and are excluded from the sample.

[20] More on the construction of the age at migration in the SUPAS data can be found in Section C.

4 Results

In this section, I first present the estimates of the destination exposure effect of childhood migration on education. I further characterize the location exposure effect by presenting heterogeneity analyses exploring urbanization and schools as relevant factors. Then, I discuss threats to identification and show several robustness checks. The section finishes with a discussion of migrant families' destination choices.

4.1 Exposure Effects on Graduation Rates

Is internal migration not beneficial for children because negative disruption effects almost eliminate the gains from better education opportunities? For adult migrants, productivity depends on how well their skills are transferable to the destination (**?**). As **?** documents, there is substantial variation both across and within provinces in terms of average years of schooling[21], as well as in the qualification of teachers. If these differences are reflective of factors influencing the ability of acquiring education (through the supply and quality of schools, through peer effects, or other factors), then it might be crucial for children *where* a family migrates to.

To estimate the causal location effect for childhood migrants, I implement **?**'s age-at-migration design on the sample of one-time migrants in the Census. The design exploits differences in the timing of migration of children who were born in the same year, grew up in the same district, and migrated to similar locations. The child who migrated earlier will be exposed to the destination for longer. If there is a causal location effect on education, it should be stronger the longer a child was exposed to the new environment.

First, I estimate the effect of migration at different ages during childhood on the likelihood of having junior secondary school completed. Figure 1 Panel (a) shows the regression coefficients of Equation (1) with *JUNSEC15+* (indicator for having at least junior secondary school completed) as the outcome. Every single coefficient $\hat{\beta}_m$ shows the *location effect* at age m and

[21] According to **?**, average schooling by district ranged from two years in the lowest performing district to twelve years in the highest performing district in 2010.

can be interpreted as a counter-factual. This means $\hat{\beta}_m$ measures how much moving to a better district increases the likelihood that a child has graduated from junior secondary school. $\hat{\beta}_4 = 0.46$ implies that moving to a district with ten percent more junior secondary school graduates among permanent residents at the age of four increased a child's likelihood for a junior secondary school degree by 4.6 percentage points relative to the counter-factual where the child had never left the birth district. $\hat{\beta}_4$ is identified by variation of different values of district quality change measures $\Delta_{odcp}^{JUNSEC15+}$ among all children who migrated at the age of four. It is possible that there is selection into destination, i.e. parents with higher propensity to invest in their children's education could be more likely to select high-quality destinations. All $\hat{\beta}_m$ are therefore possibly upward biased.

However, as stated in Section 3, the extent of selectivity should not vary depending on the age of the child. Calculating the differences in magnitude of $\hat{\beta}_m$ for different ages at migration m produces a causal estimate of the yearly destination effect for the migrant child. Panel (a) of Figure 1 reveals a clear downward pattern of $\hat{\beta}_m$. The declining pattern of the coefficients is consistent with positive location exposure effects from childhood migration. Children who are younger when they move gain more from migrating than children who migrate later and, therefore, have less exposure to the new environment. Panel (b) of Figure 1 shows the linear fit of all $\hat{\beta}_m$ coefficients. The slope of the fitted line is -0.02, implying that each year of migrating earlier to a one standard deviation better district increases the migrant child's likelihood to graduate from junior secondary school by 0.26 percentage points per year.[22]

While junior secondary school graduation is the main outcome, I find that there are similar exposure effects on the completion of primary school and senior secondary school, respectively. Figure 2 presents $\hat{\beta}_m$ coefficients on $\Delta_{odcp}^{Primary12+}$ with primary school completion as the outcome. The sample are one-time migrants aged 12 to 20 who migrated before the age of 12. The overall pattern is very similar, with larger magnitudes for younger ages at migration in Panel (a). While the pattern is somewhat flatter for the first five years, the fitted line in Panel

[22]The standard deviation of $\Delta_{odcp}^{JUNSEC15+}$ is 0.13.

(b) has a slope of -0.024, implying that each additional year in a one standard deviation better district increases the migrant child's likelihood to complete primary school by 0.17 percentage points. Figure 3 shows a similar pattern with a negative slope of -0.016, implying an increase in senior secondary graduation rates of 0.22 percentage points per additional year in a one standard deviation better district.

To improve power, I impose a linear structure on the exposure effect instead of estimating slopes for each age at migration separately. Equation (2) replaces the age at migration dummies by estimating the linear effect of age at migration interacted with Δ^S_{odcp}. This regression still controls for disruption effects and origin quality.

$$y_i = \beta^0 \Delta^S_{odcp} + \underbrace{\gamma^{lin} m_i \times \Delta^S_{odcp}}_{\text{location exposure effect}} + \alpha_{oc} + \phi_{pm} + \mathbf{X}'_p \boldsymbol{\xi} + \epsilon_i \tag{2}$$

γ^{lin} captures the effect of migrating a year later to a destination of different quality (as measured by Δ^S_{odcp}), with an interpretation analogous to the slope of the fitted values of $\hat{\beta}_m$.

The identifying assumption of Equations (1) and (2) is that unobservable family characteristics are orthogonal to the timing of migration. To relax this assumption, I additionally estimate a specification including household fixed effects Λ_h:

$$y_i = \beta^0 \Delta^S_{odcp} + \gamma^{lin} m_i \times \Delta^S_{odcp} + \alpha_{oc} + \phi_{pm} + \Lambda_h + \epsilon_i \tag{3}$$

This allows me to control for all time-invariant household characteristics which might affect both migration choice and the investment in the child's education. With the inclusion of household fixed effects, identification stems from the comparison of children in the same household, who migrated at different ages, but at the same time. This means that they were exposed to the destination for the same number of years, but not to origin district: older siblings spent more time there than younger ones. In addition, Δ^S_{odcp} is cohort-specific and, therefore, varies within household. Moreover, this specification requires observing at least two migrant children per household which in the Census implies that it can only be estimated on the sample of families with at least three children. Therefore, in Table 1, I report results from estimating model (2) in Panel (a) along with results from model (3) in Panel (b). I find that the linear location exposure effect is similar across outcomes; the coefficients imply that, in terms of educational outcomes, migrants converge to permanent residents at a yearly rate of 1.7 to 2.2 percent. In comparison, the coefficients in the model with household fixed effects, reported in Panel (b), have somewhat larger magnitudes. If differential selection into destination was driving the exposure effects estimates, the magnitude of these coefficients should be considerably smaller than the baseline result. How can the increase in magnitude be explained? First, given the difference in identifying variation, those two specifications cannot be compared with each other directly. Second, one selection-based explanation is consistent with the increase: If a number of households migrated to districts with more or better junior secondary schools after a child has completed primary education with the aim of continued education of the child, the upward bias in the $\hat{\beta_m}$ coefficients for *later ages* of migration would be mechanically larger than at younger ages. Figure 1 indeed shows that the slope of β_{10} to β_{12} is steeper than β_{12} to β_{14}, which is consistent with a more positive selection of families into destination when they move after completion of primary school. This type of selection would work against finding exposure effects, and the household fixed effect would account for it.[23] In addition, γ^{lin} in Equation (3) is identified from differences in duration in the origin only, changing the interpretation relative to Equation (2).

4.2 Heterogeneity

Thinking about intergenerational mobility, do children of less educated parents benefit more from better destinations? I split the sample by parental education group, and separately estimate the destination effect on junior secondary school completion of the migrant child. Table 2 reports the results in the first two columns, with children from low parental education families in Column (1) and those from high parental education families in Column (2). I find that

[23]Interestingly, **?** uncovers a similar pattern: The estimation coefficient of urban exposure effects on primary school completion in Africa increases from 0.091 to 0.156 with the inclusion of household fixed effects.

exposure to a better destination matters more for children of less educated parents. This regression can be interpreted as estimating upward mobility: Spending an additional year in a one standard deviation better district increases the likelihood of a junior secondary school degree by 0.34 percentage points[24] or 0.66 percent, given that in my sample 52 percent of children of parents without junior secondary school degree complete junior secondary school. Exposure effects on intergenerational education mobility are found to be of similar magnitude in Africa: **?** find that an additional year in a one standard deviation better region between the age of one to eleven increases the likelihood of primary school completion of a child of illiterate parents by 0.47 percentage points.

Columns (3) and (4) of Table 2 split the sample by gender of the child. $\hat{\gamma}^{lin}$ is slightly larger for girls, in both specifications, but the difference is not statistically significant in the fixed effects specification.

4.3 Urbanization and Schools

Δ^{S}_{odcp} captures the change in district quality as a composite measure of many factors that influence children's educational attainment. But what makes some places better than others? **?**, **?**, and others document positive returns to urban places for children. One mechanism, specific to the Indonesian context, could be the access to and the quality and cost of secondary schools. Remote rural areas are still underserved in terms of public secondary schools. In some areas, private schools meet the demands for secondary education, but at a lower quality. In addition, while officially only private schools can charge fees, the collection of alternative revenue is widespread (**?**). Such fees might be prohibitive, since financial constraints are the predominant reason for children to drop out of school early (**?**). I show a binned scatter plot of the district-level junior secondary school graduation rates among permanent residents aged 15 to 20 with the log of the number of junior secondary schools per 1,000 residents, or per 1,000 children

[24]Looking at the distribution of $\Delta^{JUNSEC15+}_{odcp}$ for children of low-educated parents only, a standard deviation equals 0.18.

age 7 to 15, respectively in Figure A.5. It shows that there is a positive but noisy relationship between the number of schools and education outcomes of permanent residents. Similarly, Figure A.6 reveals a positive relationship of educational outcomes of permanent residents with the share of urban households or log population density, respectively. Finally, Appendix Table B.1 reveals that in 65% of migrations with a positive change in destination quality, the destination is denser, and in 69% it has a higher share of urban households, than the origin. To explore how much of these factors are captured by Δ^{S}_{odcp}, I add quality change measures based on these alternative factors and add them as additional independent variable in horse race regressions.

Table 3 shows the results of this horse race. In Column (1), I add the change in the share of households reporting to live in an urban area as an additional measure of quality change. This increases the estimate of $\Delta^{JUNSEC15+}_{odcp}$ slightly relative to the model without the urban measure, both in the baseline (Panel (a)) and the specification with fixed effects (Panel (b)). However, there is no additional value of urban places that is not captured by $\Delta^{JUNSEC15+}_{odcp}$. If anything, the fixed effects specification implies a negative effect of urbanization which might capture higher crime rates and other urban dis-amenities. The pattern in Column (2) which adds a measure based on population density is very similar. Columns (3) and (4) add a measure based on the number of junior secondary schools per 1,000 inhabitants or per 1,000 children, respectively. While the coefficient on $\Delta^{JUNSEC15+}_{odcp}$ is stable, the results in Panel (b) suggest positive exposure effects over and above what is captured by permanent residents' graduation rates. Taken together, these findings imply that $\Delta^{JUNSEC15+}_{odcp}$ captures the change of a complex composition of factors contributing to causal location effects, extending beyond mere urbanization or school availability. These factors may include the quality of schools and peers, local perceived returns to education, the presence of favorable economic conditions that facilitate parental support for their children's education, alongside cultural and religious norms. While $\Delta^{JUNSEC15+}_{odcp}$ serves as a proxy for these multifaceted influences, its exact nature remains subject to further investigation.

4.4 Threats to Identification

The main identifying assumption of the age-at-migration design is that unobservable family characteristics are orthogonal to age at migration of the child - implying that the bias on $\hat{\beta}_m$ is constant across age at migration m. A violation of this assumption would arise if families who are more able or willing to invest in their children's education migrate to better places when the children are younger. In that case, the upward bias would be higher for $\hat{\beta}_1$ compared to $\hat{\beta}_6$ or $\hat{\beta}_{11}$, and this could diminish the effect of Δ^S_{odcp} by age at migration. In addition, measurement bias might arise from the siblings-based approach to infer age at migration. I have several strategies to address these concerns. First, I draw on **?** to estimate the extent of potential selection on unobservable characteristics into migration at certain ages necessary to entirely explain away my results. Second, I estimate exposure effects on the subset of individuals who were likely to be forced to migrate by a displacement shock. Third, I indirectly test the impact of time-varying characteristics. Lastly, I discuss how classical and non-classical measurement error would bias the estimates.

It is reasonable to assume that any unobservable factor such as the motivation to invest in the child's education should be highly related to parental education. Therefore, it is instructive to observe coefficient stability between the full model and one that omits all parent-related covariates which I assess following **?**. Oster proposes a measure to assess the regression coefficient robustness to the presence of omitted variables. δ^{Oster} is commonly interpreted as the relative degree of selection on observed and unobserved variables. It can be inferred from the changes in the coefficient estimates and R^2 following the inclusion of control variables under general assumptions.[25] Calculating the value of δ^{Oster} that would produce $\beta = 0$ under the assumed R^{max} allows researchers to assess how large the relative selection on observables and unobservables would need to be to produce a treatment effect of zero.[26] Oster proposes $\delta^{Oster} = 1$ as a cutoff

[25]Formally, $\delta^{Oster} = \frac{cov(X,\gamma_2' W_2)}{var(\gamma_2' W_2)} / \frac{cov(X,\gamma_1' W_1)}{var(\gamma_1' W_1)}$ where X is the treatment variable, W_1 is a vector of observed covariates, W_2 is a vector of unobserved covariates, and γ_1 and γ_2 are the respective coefficients on W_1 and W_2 of the hypothetical regression $Y = \beta X + \gamma_1' W_1 + \gamma_2' W_2 + \epsilon$.

[26]R^{max} is the R-squared from the hypothetical regression with a full set of controls including both observables and unobservables. **?** suggests that it is appropriate to assume R^{max} equals 1.3 times the estimated R^2 from the

for robust results. A δ^{Oster} value greater than one suggests that an exceptionally high degree of selection on unobservables is required for the true effect to be zero, indicating that the result is unlikely to be driven by omitted variable bias. When I estimate a model that omits all variables related to parental education and gender of the household head and their spouse, I find that relative to the full model, the coefficient in the restricted model has a higher magnitude (-0.02 for the full model vs. -0.027 in the restricted model). I use those coefficients and R^2 values of the respective regressions to obtain a selection coefficient of proportionality of $\delta^{Oster} = 2.79$. This implies that selection on unobservables would have to be almost three times as large as selection on observables in order to entirely undo my findings.

Based on **?**, I isolate a subset of moves that was arguably caused by an aggregate displacement shock, identified by a large population outflow at the district level. This is based on the yearly migration flows estimated from the timing of siblings' births in the Census. Let F_{ot} denote the number of individuals estimated to leave district o in year t in a sample of one-time migrants aged 0 to 20 in each Census. To eliminate overlap, I take into account migration years 1980 to 1993 from the 2000 sample, and years 1994 to 2010 from the 2010 sample. Due to the nature of the timing estimate, some years will see higher numbers of migrants by design. Therefore, I compute F'_{ot}, the yearly number of migrants from district o, net of year effects. Let's denote the 1980 - 2010 mean of this residual outflow $\bar{F}'_o$. The displacement shock in district o in year t is then defined as $z_{ot} = \frac{F'_{ot}}{\bar{F}'_o}$. Following **?**, I then instrument for the difference in district quality Δ^S_{odcp} by $E[\Delta^S_{odcp}|o,p]$, which is the mean district quality change for someone in origin district o and parental education p, averaged over all migration years. While families in displacement shock areas move for exogenous reasons, they still select into their destination. Instrumenting for Δ^S_{odcp} can reduce the resulting bias. I then estimate the linear exposure effects Equation (2) using 2SLS in order to identify the instrumented location exposure effect, γ_{IV}.

Figure 4 presents $\hat{\gamma}_{IV}$ estimated on 25 different subsets of the data. The starting point represents the $\hat{\gamma}_{IV}$ estimated among individuals who migrated during above-median displacement

regression with controls.

shock events. The estimation is repeated with a tightened sample restriction in two-percentile step and continues until the sample consists of individuals with the highest two percentiles of displacement shock values. The instrumented exposure effects estimate is significantly larger than the baseline, possibly because it addresses attenuation bias from measurement error. Appendix Figure A.7 shows the same figure, but replacing the household head and spouse variables with family fixed effects. In both figures, the coefficient remains stable across the range of sub samples. This is evidence against selection driving my results, as in that case we would expect the estimates to move towards zero, as I restrict more and more to individuals who are likely to have migrated for exogenous reasons.

While the household fixed effects specification controls for all household characteristics that do not change over time, it is still possible that there are time-varying unobservable characteristics that introduce a bias. Due to the cross-sectional nature of the data, I cannot directly test this, but I do it indirectly: This type of bias should be the largest in families that migrate when they only have one child. However, Table B.2 shows that the $\hat{\gamma}^{lin}$ is robust to restricting to those cases (Column (2)).[27]

Finally, there is an additional concern of bias arising from measurement error, in particular from measurement error in the age at migration estimate. Classical measurement error will lead to attenuation bias, and therefore to an underestimation of the exposure effect. Given that age at migration is measured with some error, my findings can be viewed as a lower bound. Table B.3 successively tightens the restriction on the bounds of the year of migration estimate, from a maximum error of four (maximum difference of eight years between the lower and the upper bound) down to a maximum error of one. The exposure estimate is largely unaffected by this. The somewhat larger magnitude of the coefficient in the most stringent specification using family fixed effects is consistent with some attenuation bias. The overall stability of the estimate suggests that the bias is relatively small.

[27]The coefficient decreases somewhat in magnitude when focusing on families who migrated with exactly 2, or with 3 and more children, but those differences are statistically not significant. In the fixed effects specification, the coefficient is even higher for families who migrated with more children.

Using the information on residence five years prior decreases concerns about systematic bias from estimating migration timing. If the estimation relied entirely on siblings' births, then one might be concerned that migration at older ages is only identified based on families with more than two children.[28] However, in some cases the upper or lower bound of the year of migration can be replaced by the year 1995 (2000 Census) or 2005 (2010 Census), allowing to identify older ages at migration even in families with two children.[29] To further investigate these concerns, I first implement different restrictions on the number of children in the household in Table 4. The exposure estimate is stable towards excluding families with only two children, or splitting the sample at the median sample fertility, which is three children. Second, Appendix Table B.4 explores whether families differ in a systematic way in how they space their children's births, which might violate the identifying assumption through the siblings-based migration timing estimate. By regressing the span between the upper and the lower bound of the timing of migration estimate (first two columns) and the average age gap between all siblings (last two columns) on parental education, I investigate whether more educated families time their children's births "better". In fact, I find that the year of migration bounds are 34 days (coefficient: 0.094) wider in families with high parental education after accounting for the number of children. The spacing of all births measured by the average age gap of all children is very similar: Conditional on number of children fixed effects, the spacing of births is 32 days (coefficient: 0.087) narrower in more educated families. While this difference is statistically significant, it is small overall. Section G in the Appendix explores the impact of non-classical measurement error through a bounding exercise.

Taken together, the evidence implies that my estimates are robust towards different types of measurement error in the age at migration estimate.

[28] Since in the baseline, the maximum age gap of the defining siblings is eight years, the resulting age at migration of the older sibling would be at most four years in families with two children.

[29] Appendix Table B.5 shows the number of observation across the dimensions of age at migration, number of children, and maximum error.

4.5 Robustness

Cohabitation bias. My sample of 12- to 20-year-old children is restricted to those who live with their parents at the time that the Census is taken. However, the older the children, the more likely it is that they live outside of their parents' households. In 2010, about 93 percent of children aged 12 live with at least one parent, but that share falls to 88 percent among 15-year-olds, and to 63 percent among 20-year-olds. Cohabitation bias arises if children who stay with their parents are systematically different from children who live with a non-parent caretaker or who moved out. Since cohabitation rates fall as children get older, I expect the potential bias to be more severe for older children.[30] Therefore, I successively exclude older children from the analysis to observe coefficient stability. Table 5 reports coefficients on $\Delta_{odcp}^{JUNSEC15+}$ from Equation (2) in Panel (a) and with household fixed effects from Equation (3) in Panel (b). Column (1) shows the baseline estimate of -0.02 and -0.029, respectively, with the full sample of 15- to 20-year-old children. I then restrict to children aged 15 to 19 (Column (2)), aged 15 to 18 (Column (3)), and finally aged 15 to 17 (Column (4)). The coefficients in Panel (a) are remarkably stable across all specifications with a high overlap of all 95 percent confidence intervals. With the inclusion of household fixed effects, the coefficients are a bit less stable to the different sample restrictions (Panel (b)). However, if cohabitation bias was driving the baseline findings in a systematic way, then the coefficient should decrease as I restrict the sample to younger individuals. I find (weakly) the opposite, which implies that the very demanding fixed effects specification is sensitive to the more and more selective and smaller samples. Since cohabitation rates are between 80 and 88 percent among the group of 15- to 17-year-olds, cohabitation bias remains a caveat for my main outcome. In order to further address this, I repeat the analysis for the outcome of primary school degree where I can focus on even younger children. For this analysis, I successively restrict the sample to children aged 12 to 18, 12 to 16, and finally 12 to 14 (Table B.6 in the Appendix).[31] Coefficients are equally stable in this specification.

[30] Appendix Figure A.8 plots cohabitation rates by age and urban status of the household.

[31] Cohabitation rates for 12-year-olds are 93 percent and 90 percent for 14-year-olds.

Outcome-Based Placebo Test. Following **?**, I implement several outcome-based placebo tests which exploit variation in permanent residents' graduation rates across birth cohorts, parental education and gender. Graduation rates have been growing strongly over time, but at different regional speeds. This could be due to a variety of underlying environment factors, such as the availability and quality of schooling, the composition of peer groups, the health and crime environments, and others. While some of these factors will have an impact on all children, some might be quite age-, and therefore, cohort-specific. Under a model of causal exposure effects, the conditions affecting the own birth cohort should matter the most. Figure A.9 plots coefficients of exposure effects on junior secondary school completion (JUNSEC15+) for children aged 15 - 20 from nine separate regressions according to Equation (2), where $\Delta_{odcp}^{JUNSEC15+}$ is replaced by $\Delta_{pod,c+t}^{Junsec}$ with t = -4, ..., 4. Panel (a) shows the baseline specification, and Panel (b) replaces the household head and spouse variables with family fixed effects. In Panel (a), exposure estimates based on district quality changes of older cohorts are indistinguishable from the baseline estimate, but the magnitude is smaller when replacing $\Delta_{odcp}^{JUNSEC15+}$ with values of younger cohorts. This is consistent with a rapid and accelerating improvement of conditions for children. Panel (b) shows that, when controlling for household fixed effects, only $\Delta_{odcp}^{JUNSEC15+}$ of the own cohort leads to a statistically significant coefficient. This specification is likely more sensitive to the replacement, since cross-cohort differences in $\Delta_{odcp}^{JUNSEC15+}$ are a more important part of the identifying variation.

Table B.7 presents the placebo tests with respect to parental education in Panel (a) and with respect to gender in Panel (b). In Panel (a), Δ_{podc}^{JUNSEC} (own) is the change in district quality based on educational outcomes of permanent residents of own parental education group and Δ_{podc}^{JUNSEC} (other) is the change in district quality based on educational outcomes of permanent residents of the other education group. Columns (1) and (2) compare Δ_{podc}^{JUNSEC} (own) and Δ_{podc}^{JUNSEC} (other) by using them separately and show that the estimate of the exposure effects is higher when using Δ_{podc}^{JUNSEC} of the correct parental education group. When using both measures together, the coefficient on Δ_{podc}^{JUNSEC} (own) is large and statistically significant. In the fixed effects specification, the magnitude of $\hat{\gamma}^{lin}$ is somewhat larger in Column (5) which uses Δ_{podc}^{JUNSEC} (other) compared to Column (4) which uses Δ_{podc}^{JUNSEC} (own), but the difference is statistically not significant. When both enter the model, the magnitude of the coefficients is very similar. In Panel (b), Δ_{podcg}^{JUNSEC} (own) is the change in district quality based on educational outcomes of permanent residents of the own gender, and Δ_{podcg}^{JUNSEC} (other) is the change in district quality based on educational outcomes of permanent residents of the other gender. All models show very similar coefficients with no statistically significant differences. This is likely due to the fact that Δ_{podcg}^{JUNSEC} (own) and Δ_{podcg}^{JUNSEC} (other) are highly correlated.[32]

Precision of $\Delta_{odcp}^{JUNSEC15+}$, Migration within and across provinces, and Birth Order and Family Size. Section F discusses robustness checks with respect to the precision of $\Delta_{odcp}^{JUNSEC15+}$, migration within and across provinces, and birth order and family size.

4.6 Destination Choice

The empirical evidence presented in this paper supports that there are positive location exposure effects for children who migrate across districts in Indonesia. This suggests the question whether parents are aware of location quality differences and whether they take them into account when making the decision where to migrate. Figure 5 shows the distribution of $\Delta_{odcp}^{JUNSEC15+}$ for all one-time migrants aged 15 to 20. The distribution is almost perfectly centered around zero (average $\Delta_{odcp}^{JUNSEC} = -0.01$) indicating that migration flows both ways and about half of the children migrated to a district with higher quality, while the other half migrated to a district with lower quality.[33] This indicates that either district quality is not perfectly known or that parents take into account other factors in their selection of a destination. Testing

[32]The coefficient of correlation is 0.95.

[33]Appendix Table B.1 shows that education is somewhat higher among household heads who migrate to a better destination relative to those who migrate to a district with a lower quality.

those alternatives is difficult in census data, but there are arguments for both playing a role.

Other research suggests that family decision to move also takes into account the opportunities available for the children in the destination. For example, **?** uses historical data of 1,300 families from the 1900 Bohemian Pilsen population census to test this hypothesis and finds evidence that families moved to urban areas to maximize their children's education, showing a positive link between family migration and children becoming apprentices in urban settings in the 19th century. However, as shown in Section 4.3, urbanization and the number of schools are likely to be only a small factor determining district quality. Moreover, because my measure of district quality is based on cohort-specific graduation rates, it cannot be perfectly known at the time of migration. While the auto-correlation of the quality measure is fairly high with 0.88 for children from low parental education households and 0.86 for children from high parental education households, there is quite some variation over time. Similarly, the correlation between the measure for children from low parental education households and high parental education households is 0.84. In addition, I find that in 15.1% of all households with at least two migrant children aged 15 to 20, $\Delta_{odcp}^{JUNSEC15+}$ varies to the extent that it takes on a positive value for one child and a negative value for another child. That is, although a family migrated together, the cohort-specific conditions are so dynamic that the move *increases* district quality for one child while *decreasing* district quality for another child.

While the scope to target specific ages is therefore limited in families with several children, it is still possible that parents optimize migration with respect to one child, for example the first-born or youngest child. To explore this possibility, I regress $\Delta_{odcp}^{JUNSEC15+}$ on the gender of the child who was youngest at the time of migration, on the gender of the oldest child, and a household-level average of $\Delta_{odcp}^{JUNSEC15+}$ on the total number of children in the household. Table 6 shows the results. If parents chose to move to a better destination when their youngest or oldest child is a son rather than a daughter, because they put more weight on their sons' outcomes, we would expect a positive coefficient in Columns (1) and (2), respectively. However, the coefficients are negative. Furthermore, I find in Column (3) that move quality declines slightly with the number of children. To address this question further, I test whether the gender of the oldest or the youngest child, respectively, is predictive of their age at migration and find a negligible gender difference: Firstborn boys are about 9 days older when they migrate (Appendix Table B.8 Column (1)), and there is no gender difference in the age at migration of the youngest child. These findings imply that child characteristics, including gender and others that I have not tested, might influence the destination decision. This is consistent with survey evidence showing that children have agency to be able to influence parents' decision to move in a developed country such as the UK (**?**).

Furthermore, **?** shows that having children increases the probability of return migration. Indeed, I find that about a third of all migration events in my estimation sample are cases of return migration: Either the mother or the father was born in the destination district. This highlights the complex nature of destination decisions. While some families migrate for parental work opportunities which are likely to be positively correlated with education opportunities of their children, a large share is likely moving for family reasons: They might move back to take care of older relatives or to enjoy their families' support in raising their children. In those cases parents might have to compromise on district quality for the sake of their extended family. Indeed, I find that the household average of $\Delta_{odcp}^{JUNSEC15+}$ is significantly lower in the case of return migration, and it is more likely to be negative.

Taken together, it is unlikely that parents have perfect knowledge of district quality, and even if they had, they would not be able to make an optimal decision for all their children simultaneously. In addition, children's opportunities are unlikely to be the only deciding factor. However, parents have some information on destination quality, and the amount might differ across families. In addition, the weight they place on their children's education versus other factors in their destination selection is likely to vary across both children's and parents' characteristics.

5 Conclusion

In this paper, I study the effect of internal migration during childhood on education in Indonesia. Families migrate for various reasons, for example as a coping strategy after a negative shock such as a natural disaster or for better prospects in the future (**?**). Expected income in the destination is a major economic influence on the migration decision (**?**). But what is the effect of internal migration on children? For them, migration can come at the cost of (temporary) disruption of school attendance, but with the chance of better education opportunities in the destination.

This paper shows the important role of district quality, measured as the average education among non-migrant children. Drawing from two complete count Population Censuses, I find that spending an additional year in a one standard deviation better district increases the migrant child's likelihood to graduate from junior secondary school by 0.26 percentage points. The length of exposure matters for girls more than for boys, and for children from less educated parents more than for children from more educated parents.

The identifying assumption of the age-at-migration design is that unobservable family characteristics are orthogonal to the age at migration of the child. That implies for example, that while higher ability families might be more likely to migrate to better areas, they do not do so specifically when their children are younger. To relax this assumption, I estimate a specification including household fixed effects, eliminating any time-fixed household specific characteristics. I test the impact of time-varying characteristics by isolating cases where the impact should be the strongest: In families who only migrate with one child. However, in this subsample, the estimate barely changes. I also show that selection on unobservables would have to be almost three times as much than selection on observables to explain away my findings. Moreover, I restrict to a sample that is more likely to be characterized by "push-migration" and instrument for the district change measure in order to further reduce bias from selection into migration and into destination and find that the estimates remain stable. In addition, I find that child characteristics have significant, but small impacts on the timing of migration and destination choice.

The collection of this evidence corroborates that my results are not driven by a violation of the identifying assumption.

Future research should explore the key determinants of what constitutes a "high-quality" district. This entails examining numerous potential factors, such as school and peer quality, local perceptions of educational returns, favorable economic conditions enabling parents to support their children's schooling, alongside cultural and religious norms, among others. Understanding which local factors foster children's education is not only crucial for policymakers seeking to enhance educational attainment but also for families themselves. My findings indicate that assessing district quality may pose challenges for families. Thus, offering information to households considering migration could also enhance their decision-making process and subsequently improve children's outcomes.

6 Acknowledgements

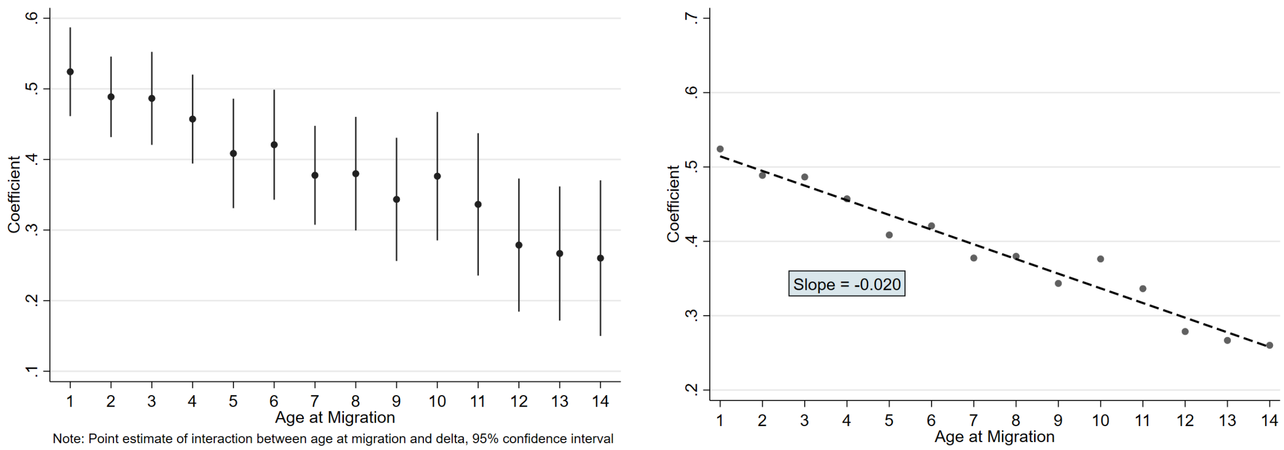
I am grateful to Samuel Bazzi for advice and suggestions, and to James Feigenbaum, Robert Margo, Daniele Paserman, participants at the 2019 NovAfrica PhD Workshop on Migration and Development, the 2nd CReAM/RWI Workshop on the Economics of Migration, and the CEMIR Junior Economist Workshop on Migration Research, and four anonymous referees for helpful comments. I also thank Chen Lin and Patricia Mawar Puspitasari for the excellent research assistance.

Funding: Funded by the Deutsche Forschungsgemeinschaft (DFG, German Research Foundation) under Germany´s Excellence Strategy – EXC 2126/1– 390838866.

References

Figures

Figure 1: Location Exposure Effect on Junior Secondary School Graduation (Age 15 - 20)

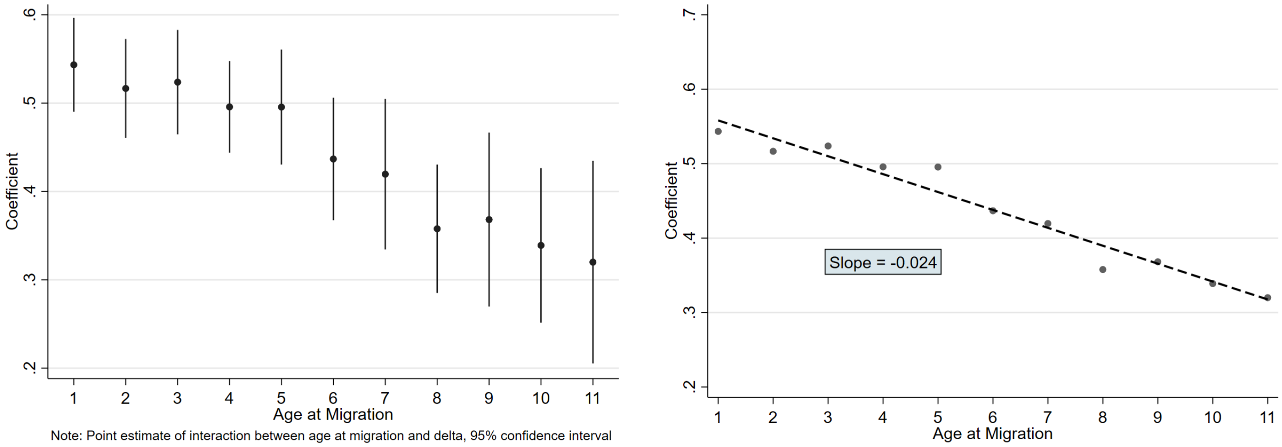


(a) $\hat{\beta}_m$

(b) Linear Fit of $\hat{\beta}_m$

Note: Panel (a) shows the effect of location quality change $\Delta_{odcp}^{JUNSEC15+}$ on junior secondary school graduation by age at migration during childhood ($\hat{\beta}_m$ from Equation (1), with 95% confidence intervals, standard errors clustered two-way on origin and destination district). Panel (b) shows the linear fit of those regression coefficients, the location exposure effect $\hat{\gamma}$. The regression in Panel (a) controls for origin $\times$ birth-cohorts fixed effects, age-at-migration $\times$ parent-education group fixed effects, and highest completed degree of the household head interacted with gender of household head, and highest completed degree of their spouse interacted with the spouse's gender. Maximum age at migration is 14 years. Sample: one-time migrants aged 15 to 20 from 2000 and 2010 Indonesian Population Census.

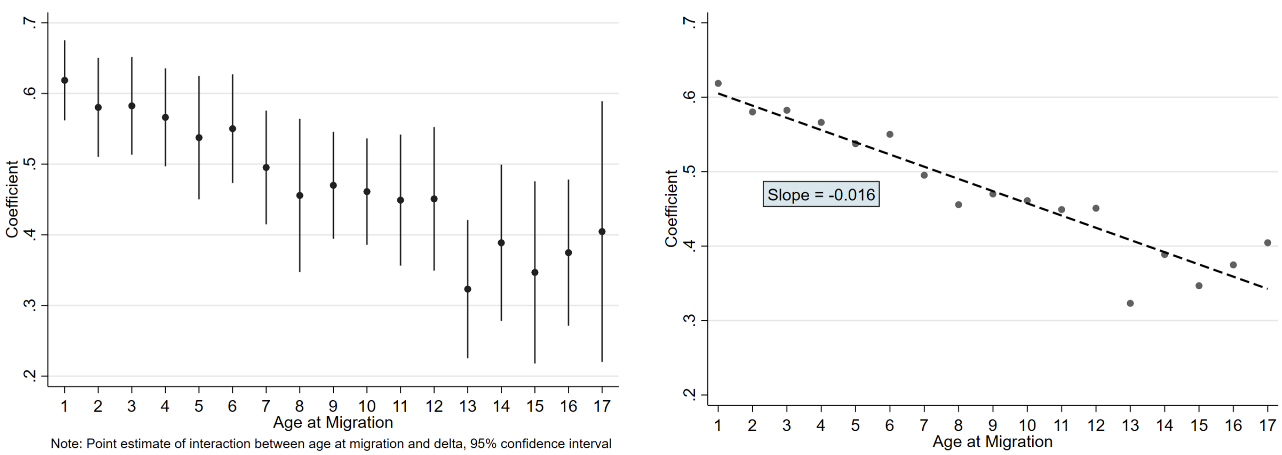
Figure 2: Location Exposure Effect on Primary School Graduation (Age 12 - 20)



(a) $\hat{\beta}_m$ (b) Linear Fit of $\hat{\beta}_m$

Note: Panel (a) shows the effect of location quality change $\Delta_{odcp}^{Primary12+}$ on primary school graduation by age at migration during childhood ($\hat{\beta}_m$ from Equation (1), with 95% confidence intervals, standard errors clustered two-way on origin and destination district). Panel (b) shows the linear fit of those regression coefficients, the location exposure effect $\hat{\gamma}$. The regression in Panel (a) controls for origin $\times$ birth-cohorts fixed effects, age-at-migration $\times$ parent-education group fixed effects, and highest completed degree of the household head interacted with gender of household head, and highest completed degree of their spouse interacted with the spouse's gender. Maximum age at migration is 11 years. Sample: one-time migrants aged 12 to 20 from 2000 and 2010 Indonesian Population Census.

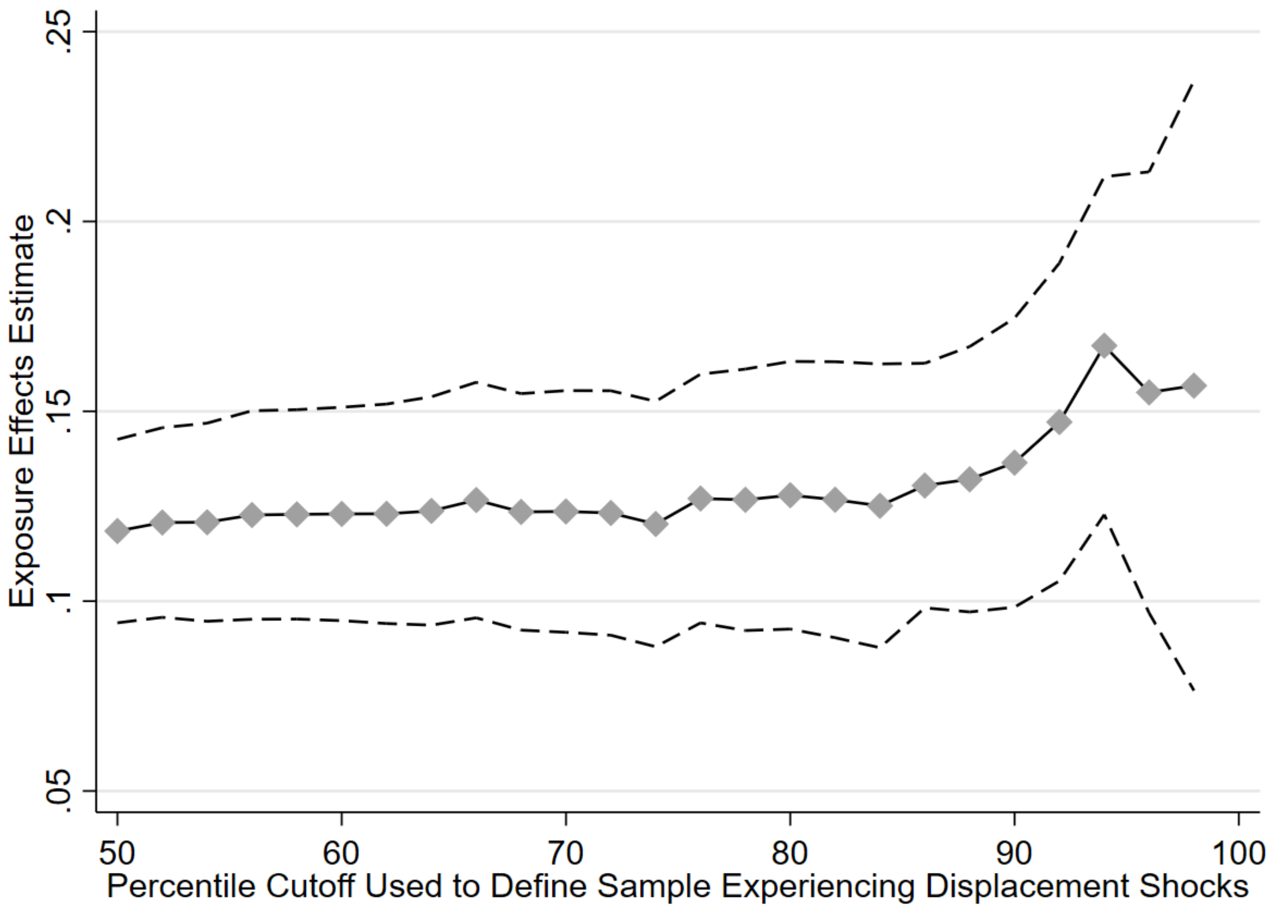
Figure 3: Location Exposure Effect on Senior Secondary School Graduation (Age 18 - 20)



(a) $\hat{\beta}_m$ (b) Linear Fit of $\hat{\beta}_m$

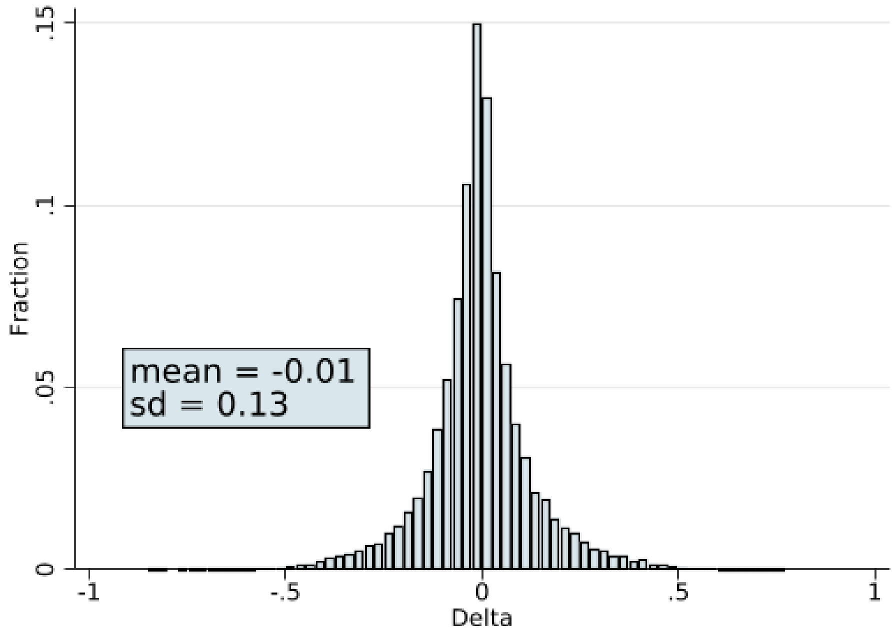
Note: Panel (a) shows the effect of location quality change Δ_{odcp}^{SMA18+} on primary school graduation by age at migration during childhood ($\hat{\beta}_m$ from Equation (1), with 95% confidence intervals, standard errors clustered two-way on origin and destination district). Panel (b) shows the linear fit of those regression coefficients, the location exposure effect $\hat{\gamma}$. The regression in Panel (a) controls for origin $\times$ birth-cohorts fixed effects, age-at-migration $\times$ parent-education group fixed effects, and highest completed degree of the household head interacted with gender of household head, and highest completed degree of their spouse interacted with the spouse's gender. Maximum age at migration is 11 years. Sample: one-time migrants aged 18 to 20 from 2000 and 2010 Indonesian Population Census.

Figure 4: Exposure Effects Estimates and Displacement Shocks



Note: This plots estimates of annual childhood exposure effects γ for a subset of individuals leaving districts with high population outflows in the respective years. Outflows are defined by dividing the number of individuals who leave district o in year t (net of year effects) by the mean outflow over the years 1980 to 2010 $z_{ot} = \frac{F'_{ot}}{F'_{o}}$. Each dot represents the exposure effect estimate on a subset of 25 displacement shock percentiles, starting with above-median values of z_{ot} and going in two-percentile increments. The last point shows the estimation result on the subset the highest two percentiles of z_{ot}. The exposure effect is estimated according to Equation (2), but instrumenting for the difference in district quality $\Delta_{odcp}^{JUNSEC15+}$ by $E[\Delta_{odcp}^{JUNSEC15+}|o,p]$, which is the mean district quality change for someone in origin district o and parental education p, averaged over all migration years. Controls for origin $\times$ birth-cohorts fixed effects, age-at-migration $\times$ parent-education group fixed effects, and highest completed degree of the household head interacted with gender of household head, and highest completed degree of their spouse interacted with the spouse's gender. Sample: one-time migrants aged 15 to 20 from 2000 and 2010 Indonesian Population Census.

Figure 5: Histogram Δ_{odcp}^{JUNSEC}, Sample of 15 - 20 Year Olds



Note: Figure shows the distribution of location quality change $\Delta_{odcp}^{JUNSEC15+}$ for all migrants age 15 - 20. $\Delta_{odcp}^{JUNSEC15+} = \bar{y}_{dcp} - \bar{y}_{ocp}$, where $\bar{y}_{dcp}$ and $\bar{y}_{ocp}$ are average junior secondary school graduation rates of permanent residents in destination district d and origin district o, respectively, of birth cohort c and parent education group p. Sample: one-time migrants aged 15 to 20 from 2000 and 2010 Indonesian Population Census.

Tables

Table 1: Location Exposure Effect on Graduation Rates

	Dependent Variable:		
	Primary12+	**JUNSEC15+**	**SENSEC18+**
	(1)	(2)	(3)
	(a) Baseline		
$m_i \times$ c.Delta	-0.022***	-0.020***	-0.017***
	(0.003)	(0.002)	(0.002)
Sample Mean	0.86	0.72	0.52
Observations	1,694,400	1,036,024	462,470
Max. Mig. Age	11	14	17
	(b) With Household Fixed Effects		
$m_i \times$ c.Delta	-0.026***	-0.029***	-0.023
	(0.007)	(0.007)	(0.014)
Sample Mean	0.87	0.71	0.50
Observations	545,689	266,976	54,110
Max. Mig. Age	11	14	17

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors clustered two-way on origin and destination district (in parentheses). Reports location exposure effect on primary school completion (Primary12+) for children aged 12 to 20, junior secondary school completion (JUNSEC15+) for children aged 15 - 20, and senior secondary school completion (SENSEC18+) for children aged 18 - 20. Panel (a) controls for origin $\times$ birth-cohorts fixed effects, age-at-migration $\times$ parent-education group fixed effects, and highest completed degree of the household head interacted with gender of household head, and highest completed degree of their spouse interacted with the spouse's gender (according to Equation (2)). Panel (b) replaces the household head and spouse variables with family fixed effects (according to Equation (3)). Max. mig. age reports the sample's respective maximum age of migration. Sample: one-time migrants from 2000 and 2010 Indonesian Population Census.

Table 2: Location Exposure Effect on Junior Secondary School Graduation, Heterogeneity by Parental Education and Gender

	Low Education	**High Education**	**Boys**	**Girls**
	(1)	(2)	(3)	(4)
		(a) Baseline		
$m_i \times \Delta_{odcp}^{JUNSEC}$	-0.019***	-0.009**	-0.018***	-0.023***
	(0.002)	(0.003)	(0.003)	(0.003)
Sample Mean	0.52	0.85	0.69	0.75
Observations	419,844	616,170	559,204	476,815
		(b) With Family Fixed Effects		
$m_i \times \Delta_{odcp}^{JUNSEC}$	-0.017*	-0.008	-0.040***	-0.041***
	(0.008)	(0.011)	(0.010)	(0.009)
Sample Mean	0.51	0.85	0.66	0.75
Observations	110,913	155,565	87,544	64,034

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors clustered two-way on origin and destination district (in parentheses). Reports location exposure effect on junior secondary school completion ($\hat{\gamma}^{lin}$ from Equations (2) and (3), respectively). Panel (a) controls for origin $\times$ birth-cohorts fixed effects, age-at-migration $\times$ parent-education group fixed effects, and highest completed degree of the household head interacted with gender of household head, and highest completed degree of their spouse interacted with the spouse's gender. Panel (b) replaces the household head and spouse variables with family fixed effects. Maximum age at migration is 14 years. Column (1) restricts to children from households with less educated parents, Column (2) restricts to households with more educated parents. Column (3) restricts to boys and Column (4) restricts to girls. Sample: one-time migrants aged 15 to 20 from 2000 and 2010 Indonesian Population Census.

Table 3: Alternative District Quality Change Measures

	Additional Quality Change Measure:			
	Urban	**Log Density**	**JUNSEC per capita**	**JUNSEC per child**
	(1)	(2)	(3)	(4)
	(a) Baseline			
$m_i \times \Delta_{odcp}^{JUNSEC}$	-0.021***	-0.025***	-0.020***	-0.020***
	(0.003)	(0.003)	(0.002)	(0.002)
$m_i \times$ c.Delta	0.000	0.000***	0.009	-0.086
	(0.001)	(0.000)	(0.087)	(0.076)
Sample Mean	0.72	0.72	0.72	0.72
Observations	1,036,024	1,036,024	1,009,261	1,036,024
	(b) With Family Fixed Effects			
$m_i \times \Delta_{odcp}^{JUNSEC}$	-0.038***	-0.036***	-0.032***	-0.029***
	(0.007)	(0.007)	(0.007)	(0.007)
$m_i \times$ c.Delta	0.007**	0.001**	-0.680**	-0.211
	(0.002)	(0.000)	(0.228)	(0.161)
Sample Mean	0.71	0.71	0.71	0.71
Observations	266,976	266,976	252,596	266,976

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors clustered two-way on origin and destination district (in parentheses). Reports location exposure effect on junior secondary school completion ($\hat{\gamma}^{lin}$ from Equations (2) and (3), respectively). Adds an additional measure of district quality change (measured in estimated year of migration): the share of individuals living in an urban area in Column (1), log density in Column (2), the number of junior secondary schools per 1000 inhabitants in Column (3), and the number of junior secondary schools per 1000 children age 7 to 15 in Column (4). See Section 3.2.2 for definition of variables. Panel (a) controls for origin $\times$ birth-cohorts fixed effects, age-at-migration $\times$ parent-education group fixed effects, and highest completed degree of the household head interacted with gender of household head, and highest completed degree of their spouse interacted with the spouse's gender. Panel (b) replaces the household head and spouse variables with family fixed effects. Maximum age at migration is 14 years. Sample: one-time migrants aged 15 to 20 from 2000 and 2010 Indonesian Population Census. Data source for urbanization, density and the number of children: Indonesian Population Census (1980, 1990, 2000, 2010), SUPAS (1985, 1995, 2005); data source for number of schools: PODES (1980, 1983, 1986, 1990, 1993, 1996, 2000, 2003, 2005, 2008, 2011).

Table 4: Number of Children in Household and Location Exposure Effect

	Sample Restriction: Number of Children			
	All	**$>$ 2**	**$<$ 4**	**4+**
	(1)	(2)	(3)	(4)
(a) Baseline				
$m_i \times \Delta_{odcp}^{JUNSEC}$	-0.020***	-0.020***	-0.017***	-0.021***
	(0.002)	(0.002)	(0.002)	(0.003)
Sample Mean	0.72	0.71	0.76	0.67
Observations	1,036,024	859,604	526,658	509,361
(b) With Family Fixed Effects				
$m_i \times \Delta_{odcp}^{JUNSEC}$	-0.029***	-0.029***	-0.021	-0.029***
	(0.007)	(0.007)	(0.011)	(0.007)
Sample Mean	0.71	0.71	0.79	0.68
Observations	266,976	266,976	60,570	205,582

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors clustered two-way on origin and destination district (in parentheses). Reports location exposure effect on junior secondary school completion ($\hat{\gamma}^{lin}$ from Equations (2) and (3), respectively). Panel (a) controls for origin $\times$ birth-cohorts fixed effects, age-at-migration $\times$ parent-education group fixed effects, and highest completed degree of the household head interacted with gender of household head, and highest completed degree of their spouse interacted with the spouse's gender. Panel (b) replaces the household head and spouse variables with family fixed effects. Maximum age at migration is 14 years. Column (1) reproduces baseline regression on the full sample. Column (2) excludes families with only two children, Column (3) restricts to families with less than four children, and Column (4) restricts to families with four or more children. Sample: one-time migrants aged 15 to 20 from 2000 and 2010 Indonesian Population Census.

Table 5: Precision of Age at Migration Estimate and Location Exposure Effects

	Sample Restriction:			
	Baseline	**Age 15 - 19**	**Age 15 - 18**	**Age 15 - 17**
	(1)	(2)	(3)	(4)
	(a) Baseline			
$m_i \times \Delta_{odcp}^{JUNSEC}$	-0.020***	-0.020***	-0.020***	-0.018***
	(0.002)	(0.002)	(0.002)	(0.002)
Sample Mean	0.72	0.71	0.69	0.66
Observations	1,036,024	894,945	750,656	580,607
	(b) With Household Fixed Effects			
$m_i \times \Delta_{odcp}^{JUNSEC}$	-0.029***	-0.032***	-0.043***	-0.031**
	(0.007)	(0.007)	(0.009)	(0.012)
Sample Mean	0.71	0.69	0.66	0.62
Observations	266,976	196,588	130,269	65,650

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors clustered two-way on origin and destination district (in parentheses). Reports location exposure effect on junior secondary school completion ($\hat{\gamma}^{lin}$ from Equations (2) and (3), respectively). Panel (a) controls for origin $\times$ birth-cohorts fixed effects, age-at-migration $\times$ parent-education group fixed effects, and highest completed degree of the household head interacted with gender of household head, and highest completed degree of their spouse interacted with the spouse's gender. Panel (b) replaces the household head and spouse variables with family fixed effects. Maximum age at migration is 14 years. Column (1) reproduces baseline regression with the outcome measured for children age 15 - 20, Column (2) restricts the age of the child to 15 - 19 years, Column (3) restricts to 15 - 18 years, Column (4) to 15 - 17. Sample: one-time migrants from 2000 and 2010 Indonesian Population Census.

Table 6: Do Child Characteristics Predict District Quality Change?

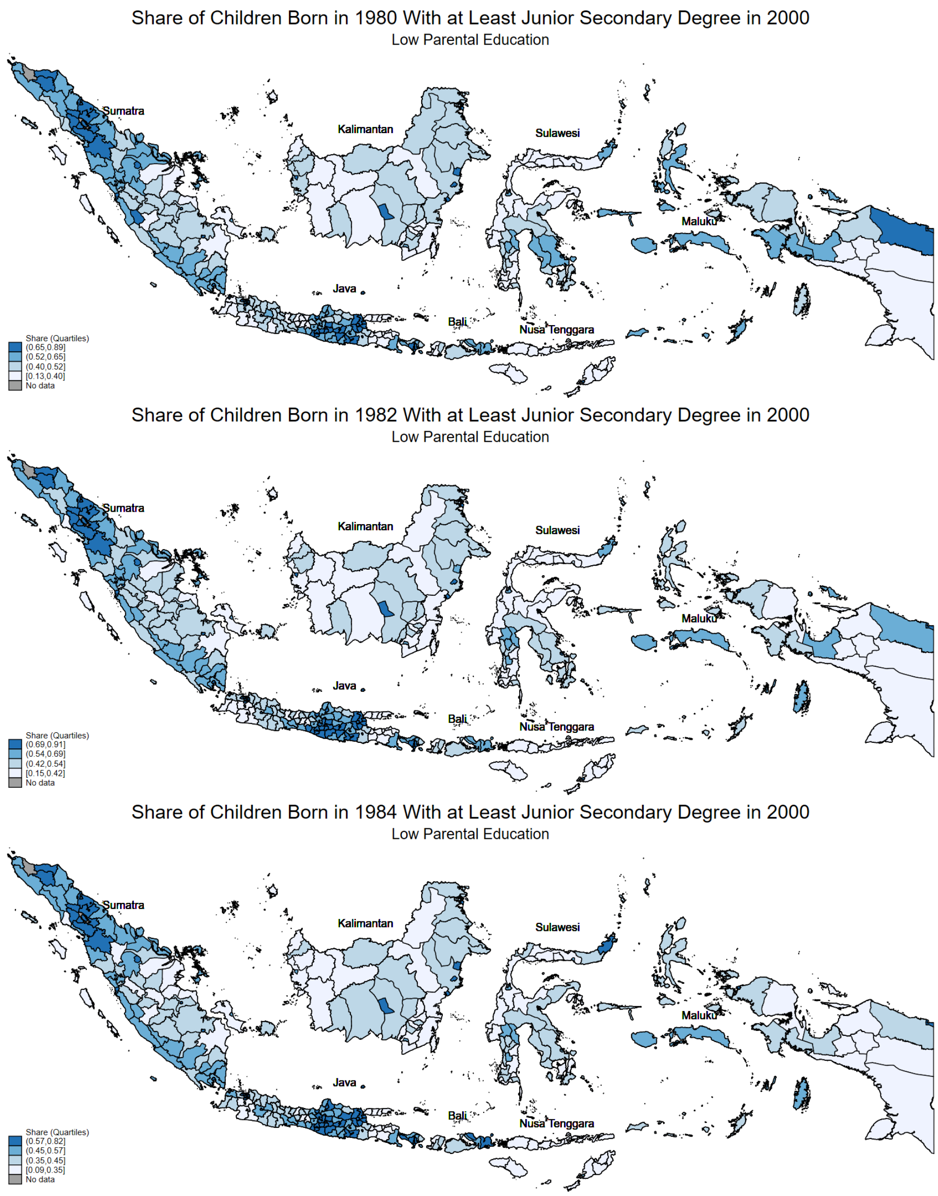
	Dependent Variable: $\Delta_{odcp}^{JUNSEC15+}$		
	Youngest: Boy	**Oldest: Boy**	**Number of Children**
	(1)	(2)	(3)
Youngest Child Male	-0.003***		
	(0.001)		
Oldest Child Male		-0.005***	
		(0.001)	
Number of Children			-0.002***
			(0.000)
Observations	277,639	665,306	665,306

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors clustered two-way on origin and destination district (in parentheses). Outcome variables are $\Delta_{odcp}^{JUNSEC15+}$ of the youngest child in (1) and of the oldest child in (2). Column (3) takes a household average of $\Delta_{odcp}^{JUNSEC15+}$ as the dependent variable. Explanatory variables are an indicator that takes the value of 1 if the youngest migrant child is a boy in Column (1) and if the oldest child is a boy in Column (3). The independent variable in Column (3) is the total number of children in the household. One observation per household. The sample in Column (1) is restricted to households with at least two migrant children. Controls for origin $\times$ birth-cohorts fixed effects, birth cohort fixed effects, highest completed degree of the household head interacted with gender of household head, and highest completed degree of their spouse interacted with the spouse's gender. Sample: one-time migrants aged 15 to 20 from 2000 and 2010 Indonesian Population Census.

Appendix

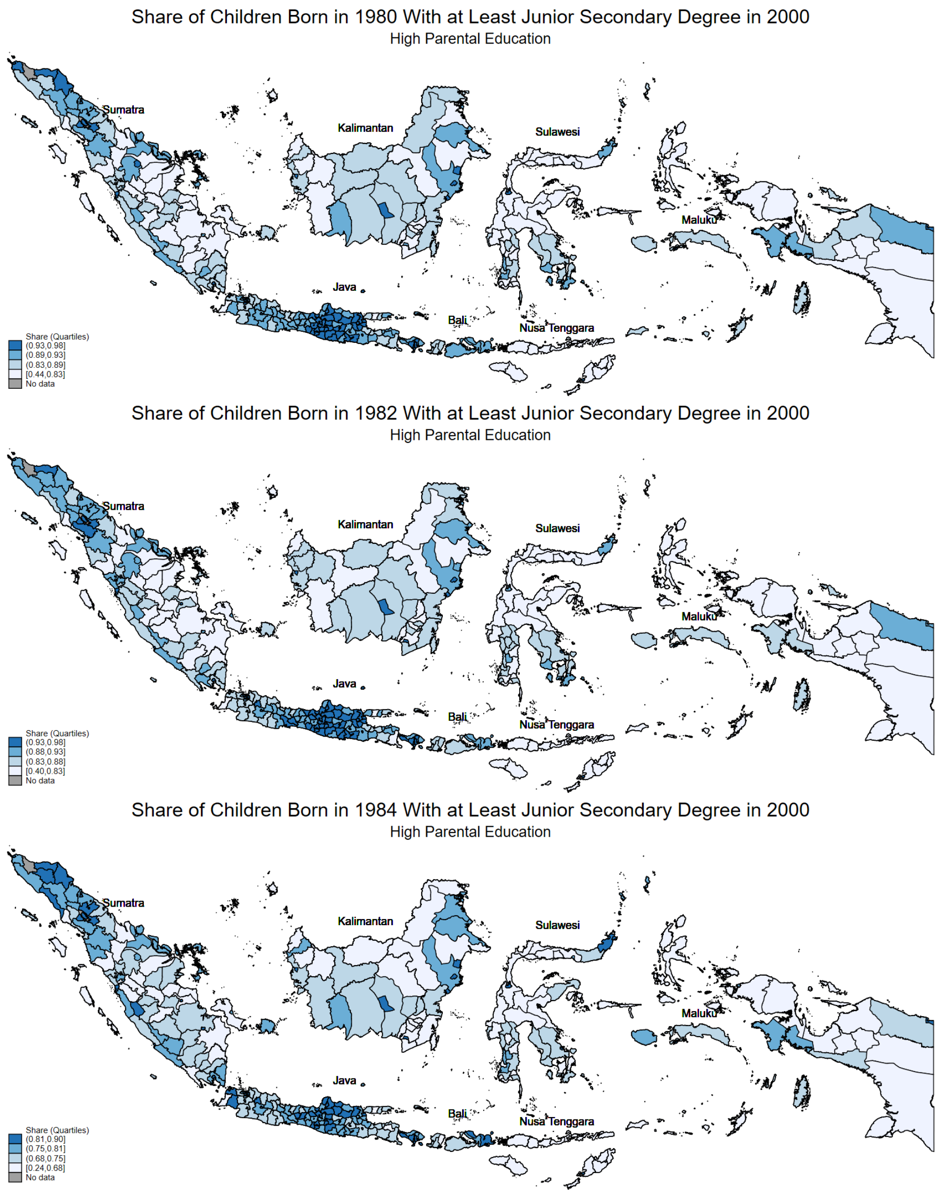
A Figures

Figure A.1: Share of Permanent Residents with Junior Secondary Degree (Low Parental Education) in 2000, by Birth Year



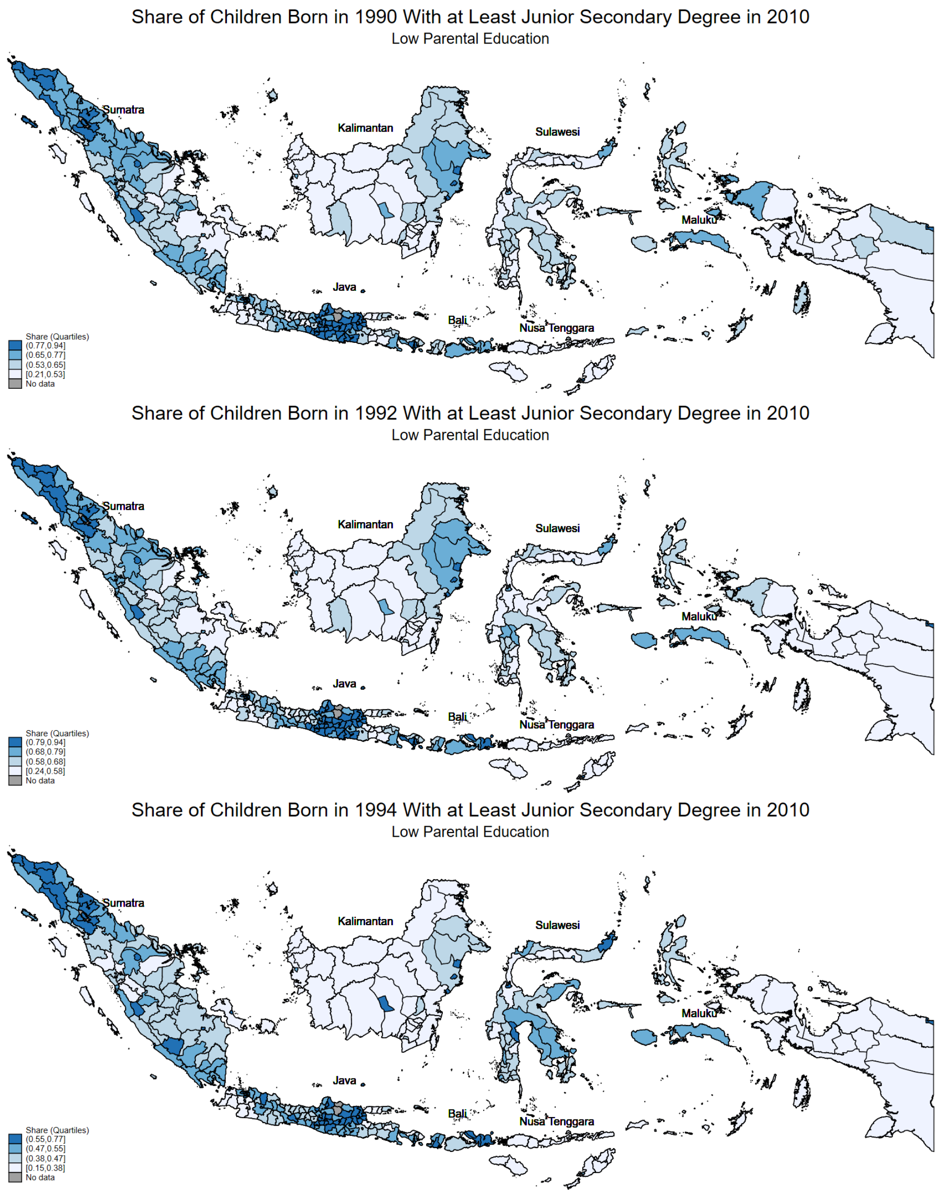
Note: Shows the share of permanent residents with junior secondary degree in 2000 Census, by birth year and district. Children from families with the highest parental education being less than junior secondary degree.

Figure A.2: Share of Permanent Residents with Junior Secondary Degree (High Parental Education) in 2000, by Birth Year



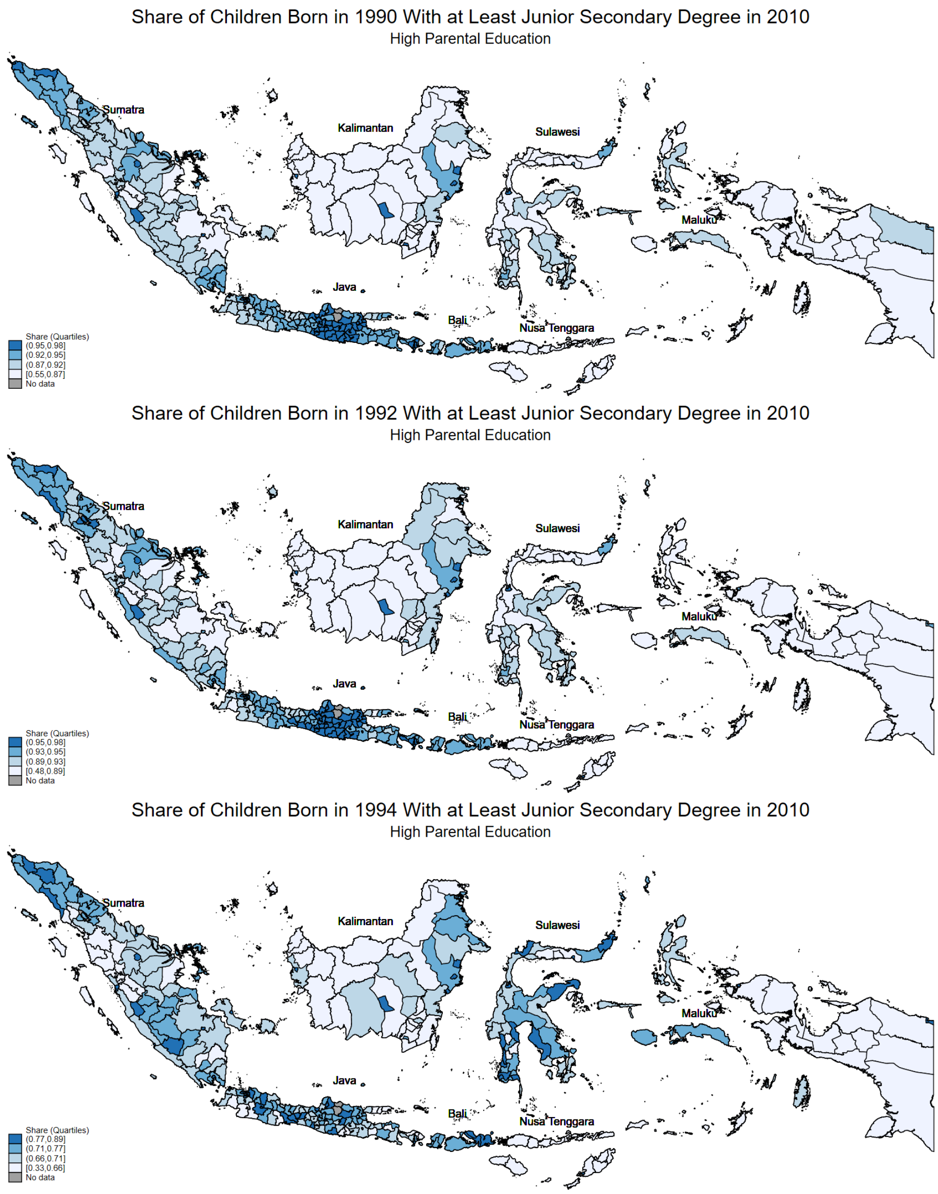
Note: Shows the share of permanent residents with junior secondary degree in 2000 Census, by birth year and district. Children from families with the highest parental education being at least junior secondary degree.

Figure A.3: Share of Permanent Residents with Junior Secondary Degree (Low Parental Education) in 2010, by Birth Year



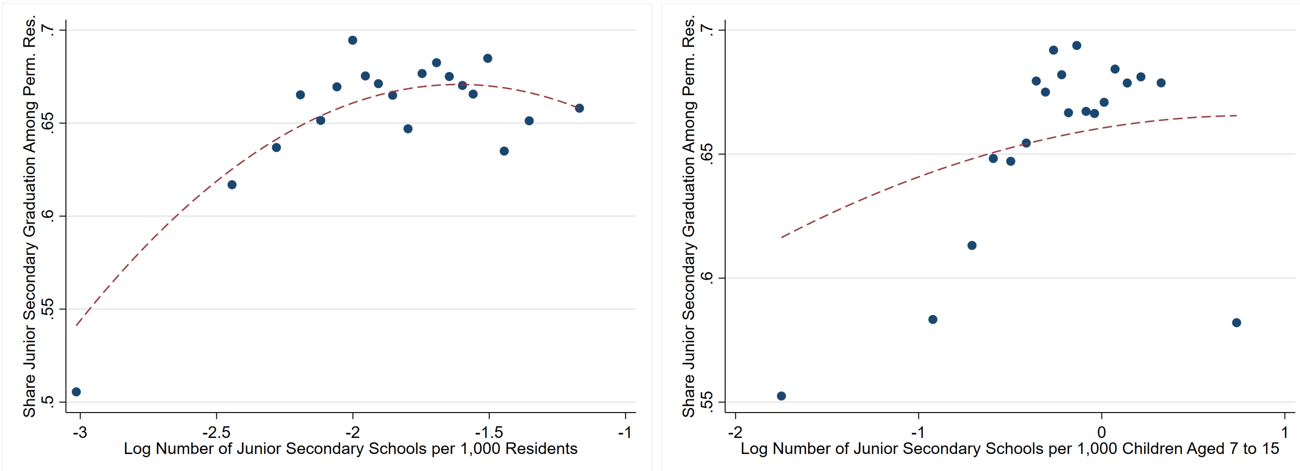
Note: Shows the share of permanent residents with junior secondary degree in 2010 Census, by birth year and district. Children from families with the highest parental education being less than junior secondary degree.

Figure A.4: Share of Permanent Residents with Junior Secondary Degree (High Parental Education) in 2010, by Birth Year



Note: Shows the share of permanent residents with junior secondary degree in 2010 Census, by birth year and district. Children from families with the highest parental education being at least junior secondary degree.

Figure A.5: Correlation of Junior Secondary School Graduation and Number of Schools

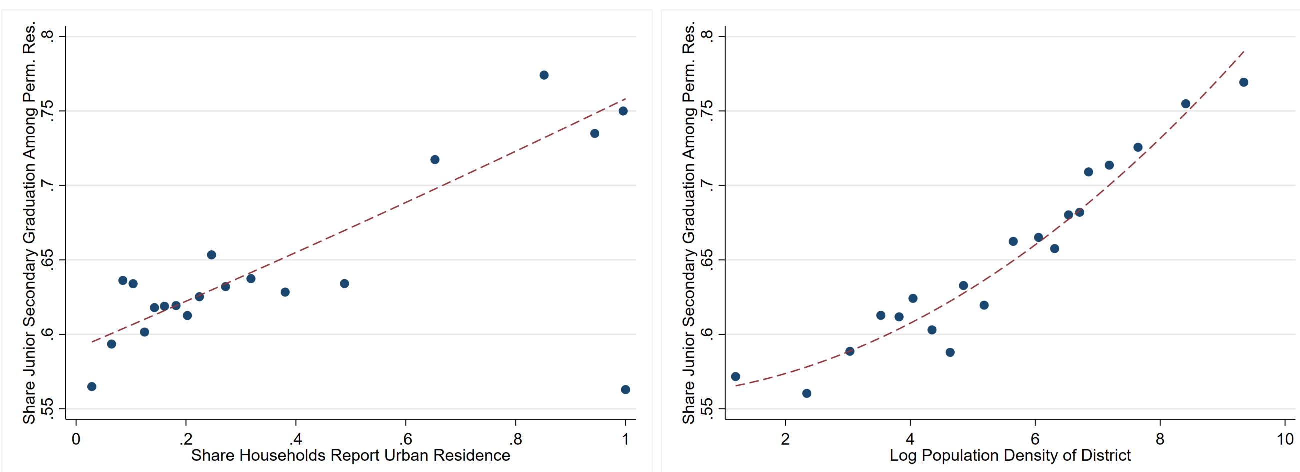


(a) Log Schools per 1,000 Residents

(b) Log Schools per Children 1,000 Aged 7 to 15

Note: This shows binned scatter plots of the district level junior secondary school graduation rates among permanent residents aged 15 to 20 with the log of the number of junior secondary schools per 1,000 residents in Panel (a), and per 1,000 children aged 7 to 15 in Panel (b), respectively. Data source for graduation rates: 2000 and 2010 Indonesian Population Census. Data sources for number of schools: PODES (1993, 1996, 2000, 2003, 2005, 2008).

Figure A.6: Correlation of Junior Secondary School Graduation and Urbanization

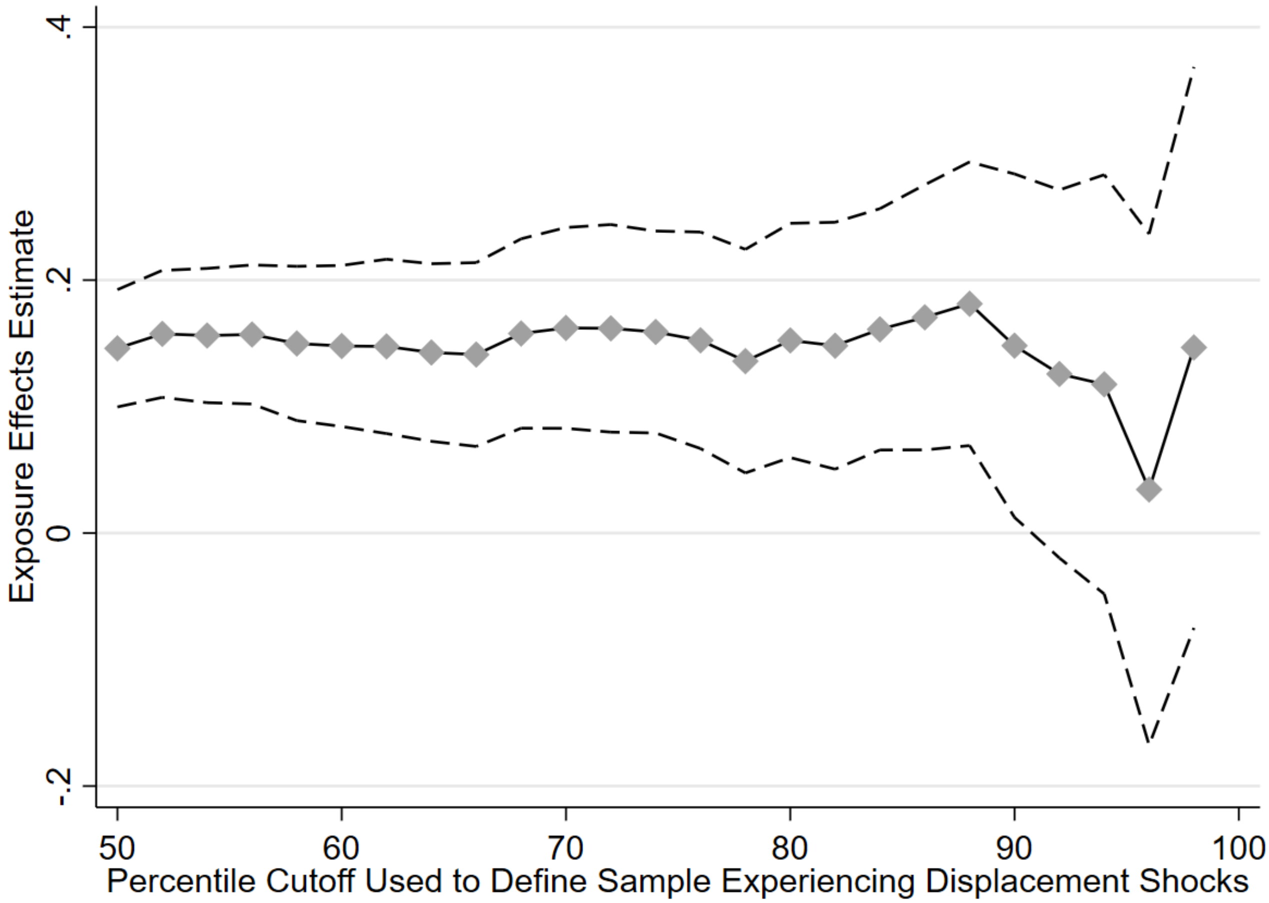


(a) Share Urban

(b) Log Density

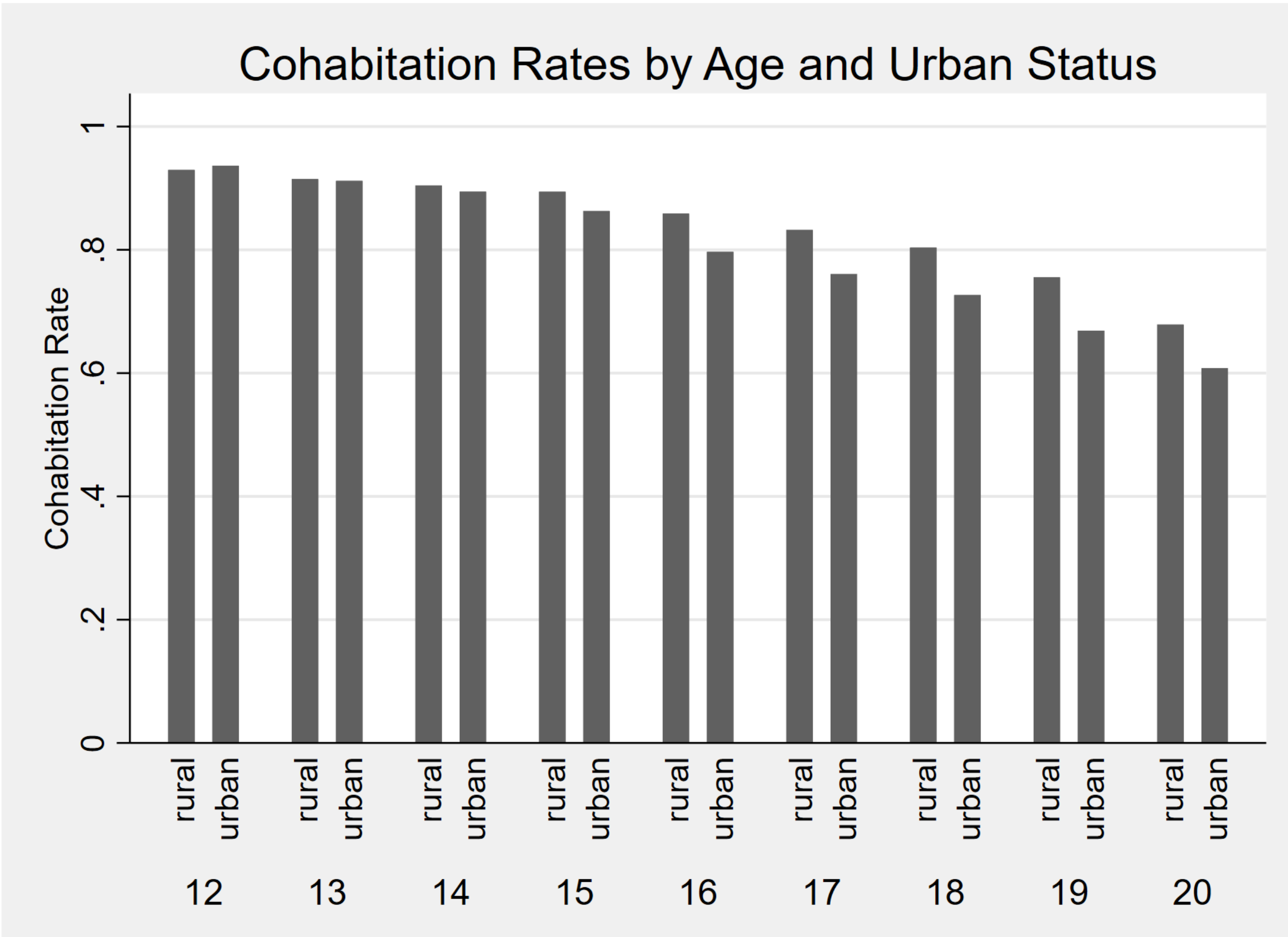
Note: This shows binned scatters plot of the district level junior secondary school graduation rates among permanent residents aged 15 to 20 with share of households reporting an urban residence in Panel (a), and log population density in Panel (b), respectively. shows permanent residents in families with high parental education (highest degree at least junior secondary) and Panel (b) shows permanent residents in families with high parental education. Data source for graduation rates: 2000 and 2010 Indonesian Population Census. Data sources for number of schools and total population: 2000 and 2010 10% Sample of Indonesian Population Census retrieved from **?**. Data sources for number of children: Population Census (2000, 2010) and SUPAS (1995, 2005).

Figure A.7: Exposure Effects Estimates and Displacement Shocks, Fixed Effects Specification



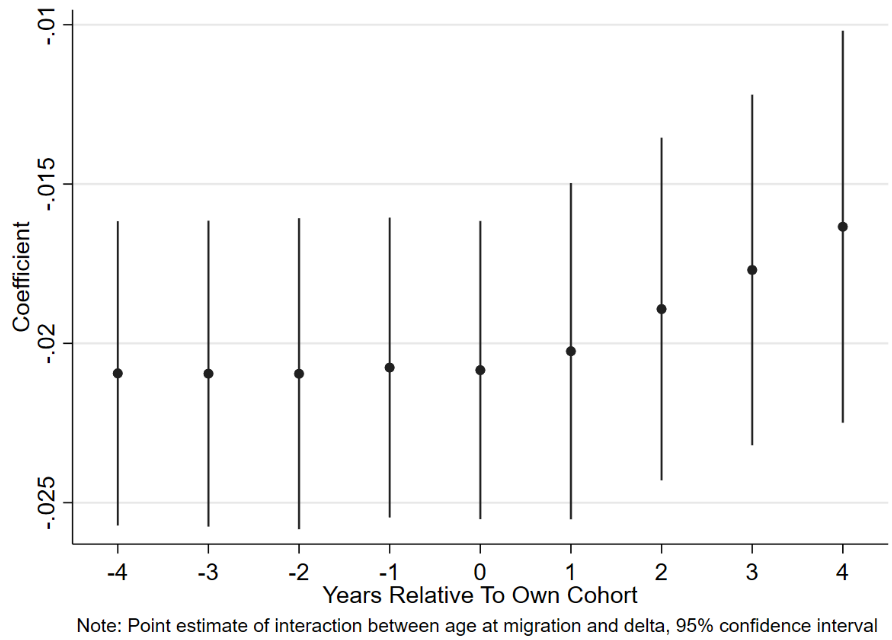
Note: This plots estimates of annual childhood exposure effects γ for a subset of individuals leaving districts with high population outflows in the respective years. Outflows are defined by dividing the number of individuals who leave district o in year t (net of year effects) by the mean outflow over the years 1980 to 2010 $z_{ot} = \frac{F'_{ot}}{F'_{o}}$. Each dot represents the exposure effect estimate on a subset of 25 displacement shock percentiles, starting with above-median values of z_{ot} and going in two-percentile increments. The last point shows the estimation result on the subset the highest two percentiles of z_{ot}. The exposure effect is estimated according to Equation (3), but instrumenting for the difference in district quality $\Delta_{odcp}^{JUNSEC15+}$ by $E[\Delta_{odcp}^{JUNSEC15+}|o,p]$, which is the mean district quality change for someone in origin district o and parental education p, averaged over all migration years. Controls for origin $\times$ birth-cohorts fixed effects, age-at-migration $\times$ parent-education group fixed effects, and household fixed effects. Sample: one-time migrants aged 15 to 20 from 2000 and 2010 Indonesian Population Census.

Figure A.8: Cohabitation Rates

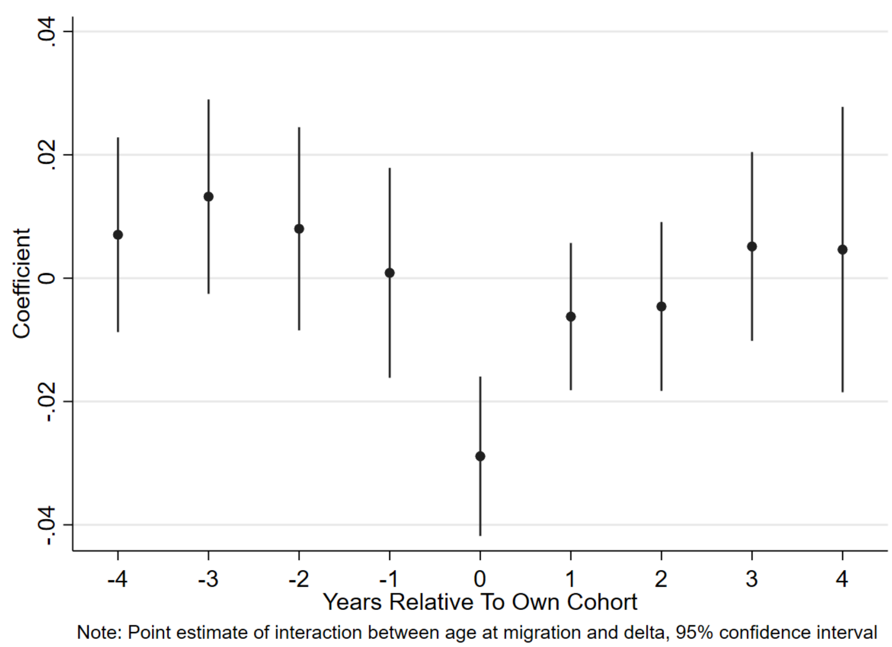


Note: This plots cohabitation rates by age of the child and urban status of the household. Data: 10% subsample of 2000 and 2010 Indonesian Census, respectively, retrieved from **?**. Averages reweighed with provided sample weights.

Figure A.9: Outcome-Based Placebo Test: Birth Cohorts



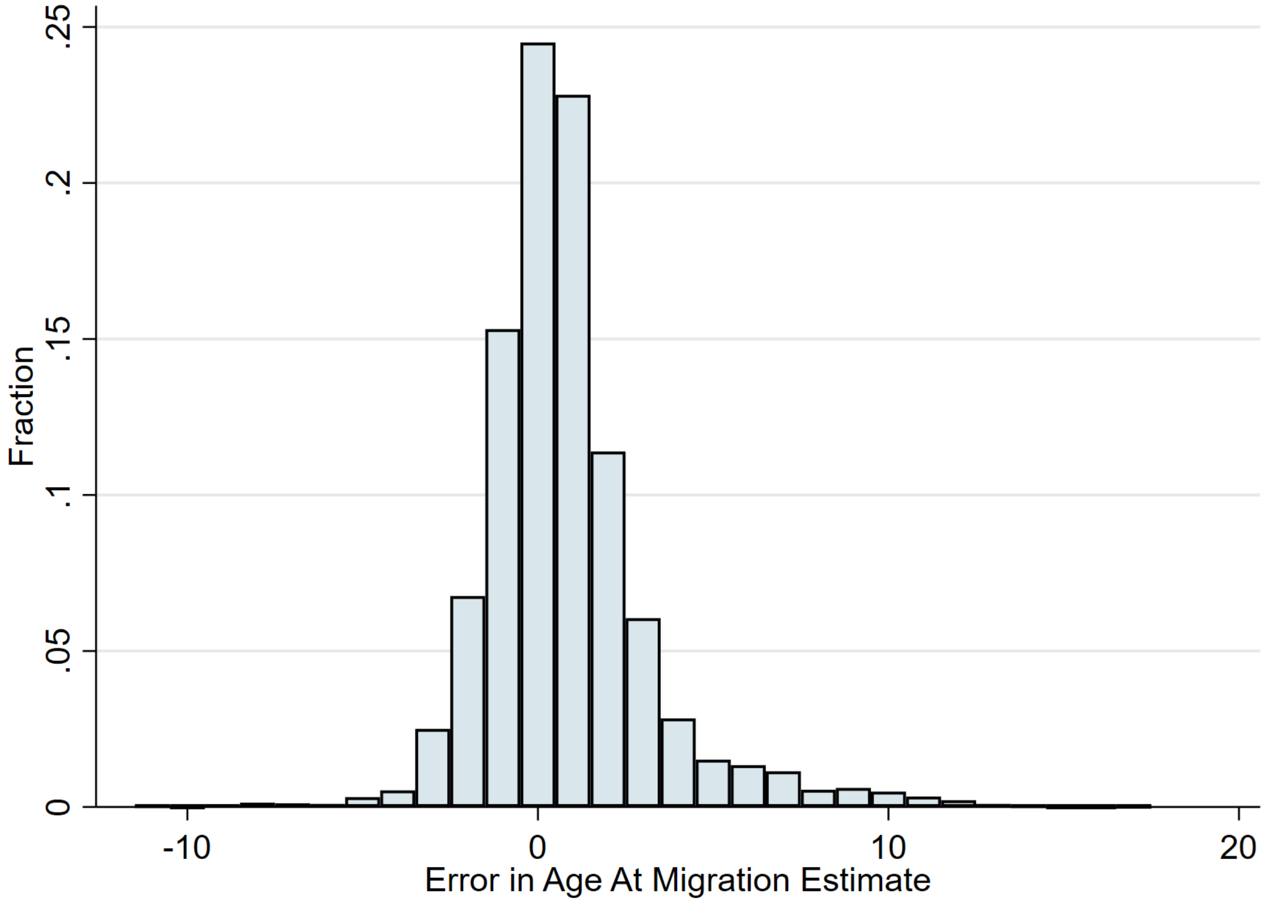
Panel (a) Baseline Specification



Panel (b) Household Fixed Effects

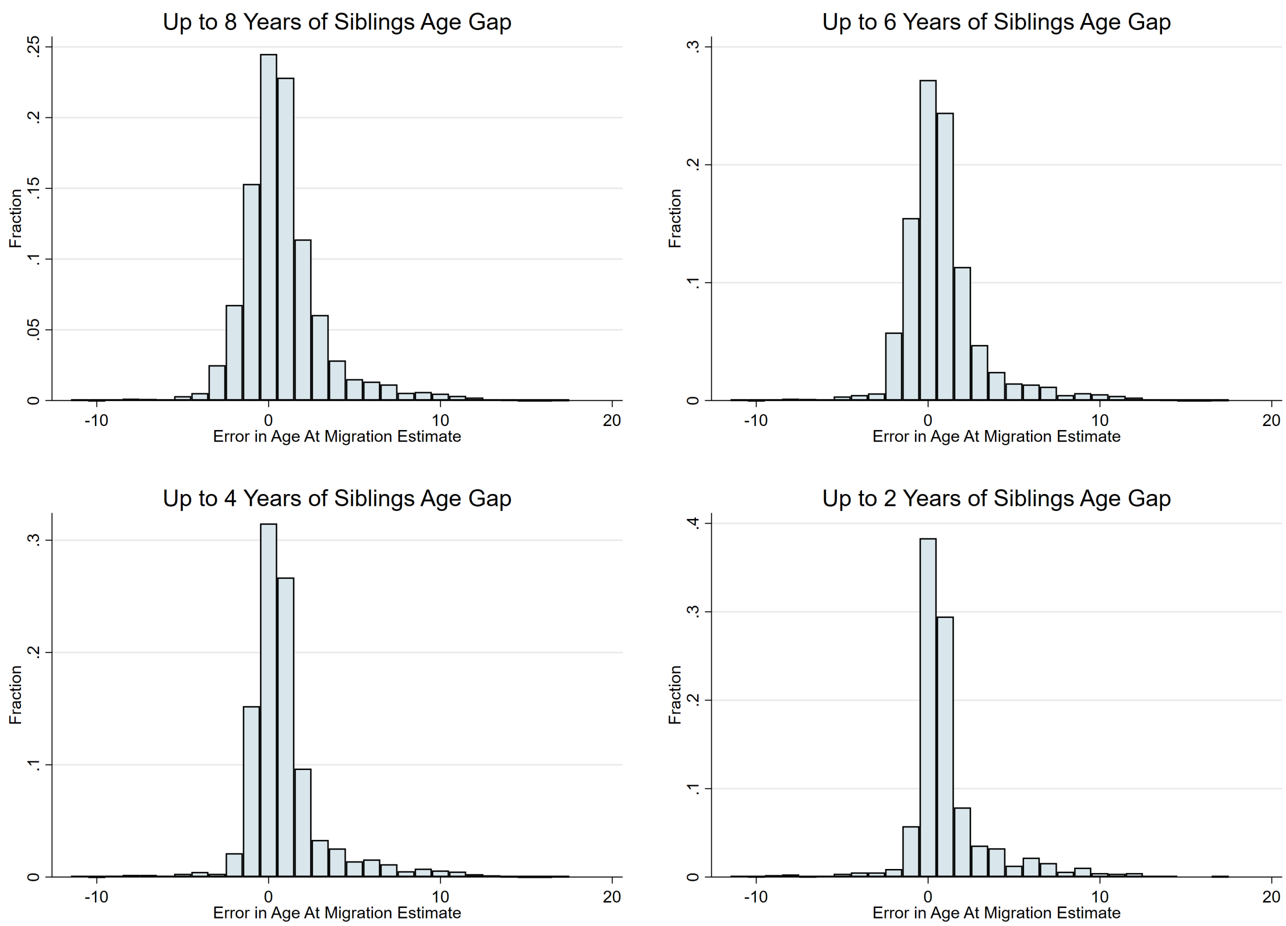
Note: This plots coefficients of exposure effects on junior secondary school completion for children age 15 - 20 from nine separate regressions according to Equations (2) and (3), respectively, where $\Delta^{JUNSEC15+}_{odcp}$ is replaced by $\Delta^{Junsec}_{pod,c+t}$ with t = -4, ..., 4. Standard errors clustered two-way on origin and destination district (in parentheses). Panel (a) controls for origin × birth-cohorts fixed effects, age-at-migration × parent-education group fixed effects, and highest completed degree of the household head interacted with gender of household head, and highest completed degree of their spouse interacted with the spouse's gender. Panel (b) replaces the household head and spouse variables with family fixed effects. Maximum age at migration is 14 years. Sample: one-time migrants aged 15 to 20 from 2000 and 2010 Indonesian Population Census.

Figure A.10: Difference in Age of Migration Estimate between the SUPAS- and the Siblings-Based Approach



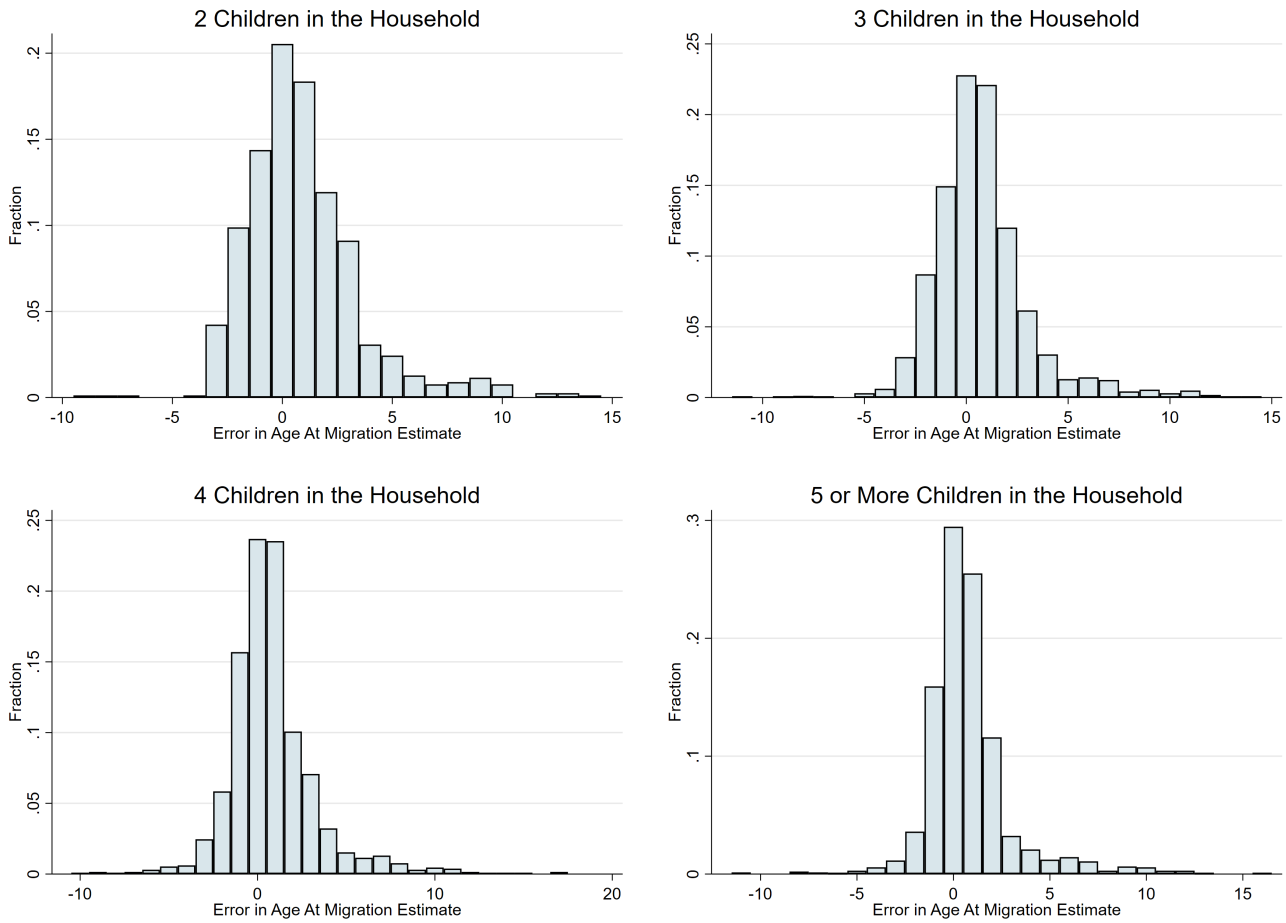
Note: This plots a histogram of the difference between the age at migration estimate based on the information provided by SUPAS and the siblings-based approach. Positive values indicate that the siblings-based approach estimates earlier migration. The sample includes children born in 1980 to 1988 and 1990 to 1993 who are between 12 and 20 years old and live with their parents in the 1995 and 2005 SUPAS waves. Sample: 1-time migrants according to both the SUPAS- and the siblings-based approach from SUPAS1995 and SUPAS2005.

Figure A.11: Difference in Age of Migration Estimate between the SUPAS- and the Siblings-Based Approach, by Siblings Age Gap



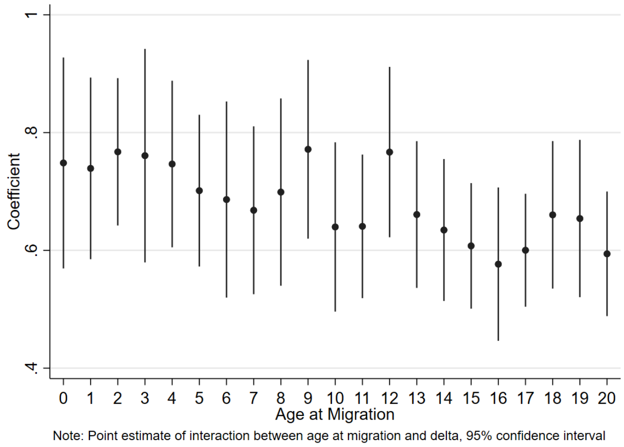
Note: This plots a histogram of the difference between the age at migration estimate based on the information provided by SUPAS and the siblings-based approach. Positive values indicate that the siblings-based approach estimates earlier migration. The sample includes children born in 1980 to 1988 and 1990 to 1993 who are between 12 and 20 years old and live with their parents in the 1995 and 2005 SUPAS waves. The restriction on the maximum age gap that the siblings-based approach is using is eight years in the baseline sample (upper left panel), and six, four, and two years subsequently. Sample: 1-time migrants according to both the SUPAS- and the siblings-based approach from SUPAS1995 and SUPAS2005.

Figure A.12: Difference in Age of Migration Estimate between the SUPAS- and the Siblings-Based Approach, by Number of Children in the Household

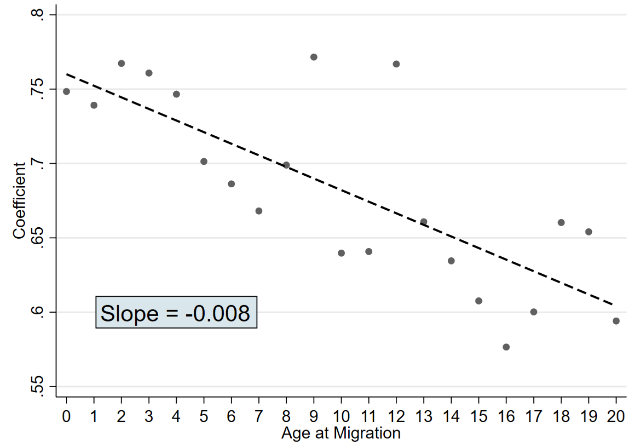


Note: This plots a histogram of the difference between the age at migration estimate based on the information provided by SUPAS and the siblings-based approach. Positive values indicate that the siblings-based approach estimates earlier migration. The sample includes children born in 1980 to 1988 and 1990 to 1993 who are between 12 and 20 years old and live with their parents in the 1995 and 2005 SUPAS waves. Panels are separated by the number of children present in the household. Sample: 1-time migrants according to both the SUPAS- and the siblings-based approach from SUPAS1995 and SUPAS2005.

Figure A.13: Location Exposure Effect on Years of Education (Adults Aged 21+)



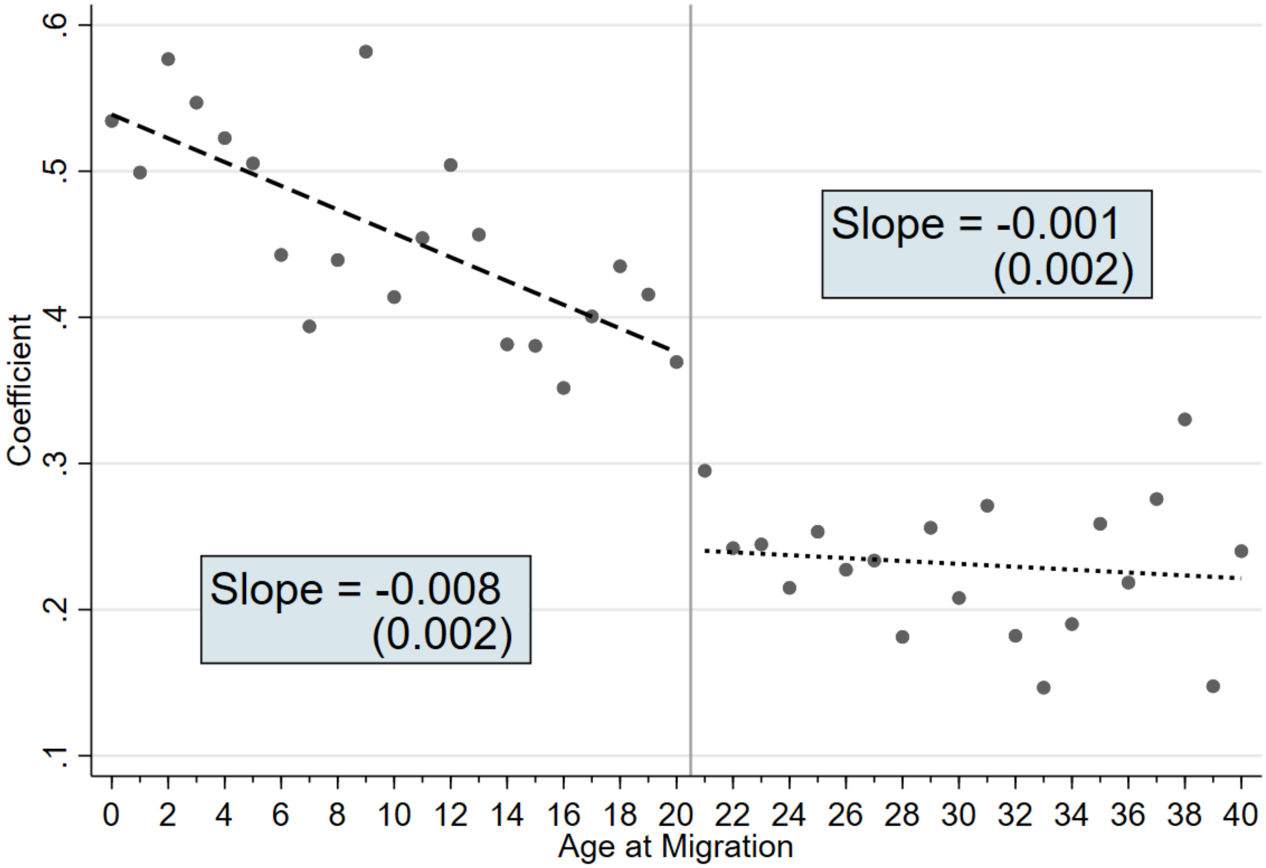
(a) $\hat{\beta}_m$



(b) Linear Fit of $\hat{\beta}_m$

Note: Panel (a) shows the effect of location quality change Δ^{YR}_{odc} on years of education by age at migration during childhood ($\hat{\beta}_m$ from Equation (4), with 95% confidence intervals, standard errors clustered two-way on origin and destination district). Panel (b) shows the linear fit of those regression coefficients, the location exposure effect $\hat{\gamma}$. The regression in Panel (a) includes birth cohort fixed effects and origin quality interacted with birth cohort fixed effects. Maximum age at migration is 20 years. Sample: one-time migrants aged 21+ from SUPAS1985, SUPAS1995, SUPAS2005.

Figure A.14: Location Exposure Effect on Years of Education (Adults Aged 21+)
Selection into Migration



Note: Panel (a) shows the effect of location quality change Δ^{YR}_{odc} on years of education by age at migration during childhood ($\hat{\beta}_m$ from Equation (4), with 95% confidence intervals, standard errors clustered two-way on origin and destination district). The regression in Panel (a) includes birth cohort fixed effects and origin quality interacted with birth cohort fixed effects. Panel (b) shows the linear fit of those regression coefficients, the location exposure effect $\hat{\gamma}$, which is estimated separately for migrations until the age of 20, and between 21 and 40, respectively. Maximum age at migration is 40 years. Sample: one-time migrants aged 21+ from SUPAS1985, SUPAS1995, SUPAS2005.

B Tables

Table B.1: Characteristics of Positive and Negative Moves

	Delta SMP Negative (1)	**SD** (2)	**Delta SMP Positive** (3)	**SD** (4)	**Difference** (5)	**SE of Difference** (6)
Head: Age	46.025	6.821	45.654	6.641	0.371***	(0.013)
Head: Female	0.065	0.246	0.065	0.247	-0.000	(0.000)
Head: Primary Education	0.860	0.347	0.884	0.320	-0.024***	(0.001)
Head: Junior Secondary Education	0.549	0.498	0.567	0.496	-0.018***	(0.001)
Head: Senior Secondary Education	0.394	0.489	0.408	0.491	-0.013***	(0.001)
Destination Denser Than Origin	0.219	0.413	0.645	0.479	-0.426***	(0.001)
Destination More Urban Than Origin	0.238	0.426	0.691	0.462	-0.453***	(0.001)
Share Urban in Origin	0.610	0.377	0.387	0.317	0.222***	(0.001)
Share Urban in Destination	0.405	0.320	0.667	0.359	-0.262***	(0.001)
More Jun. Sec. Schools PC	0.553	0.497	0.491	0.500	0.062***	(0.001)
More Jun. Sec. Schools PK	0.487	0.500	0.478	0.500	0.008***	(0.001)
Observations	562,110		483,149		1,045,259	

Note: Descriptive statistics on the household heads of individuals in the regression sample, the siblings, and of the origin and destination district. Columns (1) and (2) show means and standard deviations of moves to worse destination, measured by share of permanent residents with at least completed junior secondary education, Columns (3) and (4) show means and standard deviations of moves to better destination, measured by share of permanent residents with at least completed junior secondary education. Column (5) shows difference in point estimates (standard error in parentheses in column (6)). PC = per capita (Podes population), PK = per child aged 7 to 15 (Census population). See Section 3.2.2 for definition of variables. Data: Indonesian Population Census (1980, 1990, 2000, 2010), SUPAS (1985, 1995, 2005), Podes (1980, 1983, 1986, 1990, 1993, 1996, 2000, 2003, 2005, 2008, 2011).

Table B.2: Number of Children at Time of Migration and Location Exposure Effects

	Sample Restriction: Number of Children			
	All	**1**	**2**	**3+**
	(1)	(2)	(3)	(4)
		(a) Baseline		
$m_i \times \Delta_{odcp}^{JUNSEC}$	-0.020***	-0.021***	-0.018***	-0.017***
	(0.002)	(0.005)	(0.003)	(0.003)
Sample Mean	0.72	0.75	0.72	0.66
Observations	1,036,024	424,032	378,072	233,888
		(b) With Family Fixed Effects		
$m_i \times \Delta_{odcp}^{JUNSEC}$	-0.029***		-0.026**	-0.032***
	(0.007)		(0.009)	(0.008)
Sample Mean	0.71	.	0.75	0.67
Observations	266,976		132,440	134,074

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors clustered two-way on origin and destination district (in parentheses). Reports location exposure effect on junior secondary school completion ($\hat{\gamma}^{lin}$ from Equations (2) and (3), respectively). Panel (a) controls for origin × birth-cohorts fixed effects, age-at-migration × parent-education group fixed effects, and highest completed degree of the household head interacted with gender of household head, and highest completed degree of their spouse interacted with the spouse's gender. Panel (b) replaces the household head and spouse variables with family fixed effects. Maximum age at migration is 14 years. Column (1) reproduces baseline regression on the full sample. Column (2) restricts to families with only one child at the time of migration, Column (3) restricts to families who migrated with exactly two children, and Column (4) restricts to families with at least three children at the time of migration. Sample: one-time migrants aged 15 to 20 from 2000 and 2010 Indonesian Population Census.

Table B.3: Robustness Location Exposure Effect on Junior Secondary School Graduation: Year of Migration Estimate

	Sample Restriction:			
	Error $\leq$ 4	**Error $\leq$ 3**	**Error $\leq$ 2**	**Error $\leq$ 1**
	(1)	(2)	(3)	(4)
	(a) Baseline			
$m_i \times \Delta_{podc}^{JUNSEC}$	-0.020***	-0.020***	-0.020***	-0.019***
	(0.002)	(0.002)	(0.003)	(0.003)
Sample Mean	0.72	0.71	0.70	0.70
Observations	1,036,024	852,125	588,843	256,996
	(b) With Household Fixed Effects			
$m_i \times \Delta_{podc}^{JUNSEC}$	-0.029***	-0.029***	-0.031***	-0.032***
	(0.007)	(0.007)	(0.007)	(0.010)
Sample Mean	0.71	0.70	0.69	0.69
Observations	266,976	217,742	151,641	64,771

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors clustered two-way on origin and destination district (in parentheses). Reports location exposure effect on junior secondary school completion ($\hat{\gamma}^{lin}$ from Equations (2) and (3), respectively). Panel (a) controls for origin $\times$ birth-cohorts fixed effects, age-at-migration $\times$ parent-education group fixed effects, and highest completed degree of the household head interacted with gender of household head, and highest completed degree of their spouse interacted with the spouse's gender. Panel (b) replaces the household head and spouse variables with family fixed effects. Maximum age at migration is 14 years. Column (1) reproduces baseline regression with a maximum error of 4 years, Column (2) restricts to maximum year of migration estimate error of 3 years, Column (3) restricts to maximim error of 2 years, Column (4) to 1 year. Sample: one-time migrants from 2000 and 2010 Indonesian Population Census.

Table B.4: Parental Education and Spacing of Births

	Year of Migration Bounds	**Year of Migration Bounds**	**Average Age Gap of all Children**	**Average Age Gap of all Children**
	(1)	(2)	(3)	(4)
High Education	0.311***	0.094***	-0.178***	-0.087***
	(0.004)	(0.003)	(0.002)	(0.001)
No. of Children FE	No	Yes	No	Yes
Observations	1,426,230	1,426,230	1,426,230	1,426,230

Note: Table presents household level regressions with the span between the upper and the lower bound of the timing of migration estimate as the dependent variable in Columns (1) and (2) and the average age gap between all children in the household in Columns (3) and (4). Independent variable is an indicator that equals 1 if highest parental education is at least a junior secondary degree. Even columns add number of children fixed effects .

Table B.5: Observations by Age at Migration, Sibling Age Gap, and Number of Children

	Error ≤ 4 (1)	**Error ≤ 3** (2)	**Error ≤ 2** (3)	**Error ≤ 1** (4)	**Error ≤ 4** (5)	**Error ≤ 3** (6)	**Error ≤ 2** (7)	**Error ≤ 1** (8)	**Error ≤ 4** (9)	**Error ≤ 3** (10)	**Error ≤ 2** (11)	**Error ≤ 1** (12)
	(a) Two Children				**(b) Three Children**				**(c) Four or More Children**			
1	41,476	41,476	41,476	41,476	80,593	80,593	80,593	80,593	143,755	143,755	143,755	143,755
2	80,424	80,424	80,424	0	133,140	133,140	133,140	1,569	165,592	165,592	165,592	6,507
3	101,422	101,422	0	0	123,168	123,168	7,504	4,341	112,959	112,959	25,427	16,442
4	120,998	0	0	0	116,914	16,538	11,809	3,858	99,157	41,019	34,457	13,186
5	0	0	0	0	31,338	26,611	13,516	2,910	50,512	46,099	29,994	10,019
6	0	0	0	0	34,973	25,976	14,490	3,922	48,677	40,079	27,117	9,997
7	246	246	246	246	27,866	20,521	11,278	4,229	41,114	34,334	23,906	10,862
8	866	866	866	866	19,612	14,158	8,081	3,896	32,680	27,478	19,661	9,784
9	1,346	1,346	1,346	639	13,308	9,839	6,515	2,612	25,964	22,069	16,462	7,898
10	1,363	1,363	985	493	9,223	7,390	4,638	1,974	20,541	17,930	13,129	6,414
11	1,073	1,073	798	409	5,895	4,983	3,344	1,547	15,650	13,993	10,559	5,230
12	917	917	705	353	3,740	3,506	2,447	1,102	10,451	9,826	7,552	3,792
13	768	768	570	296	2,408	2,407	1,798	904	6,840	6,834	5,572	2,911
14	663	663	517	242	1,599	1,599	1,272	693	4,134	4,134	3,635	2,121
15	598	598	466	225	1,124	1,124	889	483	2,598	2,596	2,154	1,232
16	427	427	301	143	688	688	509	251	1,517	1,517	1,152	552
17	210	210	131	0	356	356	232	7	893	892	638	109

Note: Shows the number of observations in each cell defined by the age at migration estimate, the maximum age gap between the siblings defining the age at migration estimate, and the number of children in the family at the time of the Census. Children in sample are born between 1980 and 1988, and 1990 and 1998. Columns (1) - (4) show families with exactly two children, Columns (5) - (8) show families with exactly three children, and Columns (9) - (12) show families with four or more children.

Table B.6: Robustness Location Exposure Effect on Primary School Completion: Age of Child

	Sample Restriction:			
	Baseline	**Age 12 - 18**	**Age 12 - 16**	**Age 12 - 14**
	(1)	(2)	(3)	(4)
	(a) Baseline			
$m_i \times \Delta_{podc}^{primary}$	-0.022***	-0.022***	-0.022***	-0.019***
	(0.003)	(0.003)	(0.004)	(0.004)
Sample Mean	0.86	0.84	0.80	0.72
Observations	1,694,400	1,419,411	1,073,427	687,175
	(b) With Household Fixed Effects			
$m_i \times \Delta_{podc}^{primary}$	-0.026***	-0.027***	-0.036***	-0.050**
	(0.007)	(0.008)	(0.009)	(0.018)
Sample Mean	0.87	0.85	0.80	0.69
Observations	545,689	403,378	229,737	70,902

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors clustered two-way on origin and destination district (in parentheses). Reports location exposure effect on primary school completion ($\hat{\gamma}^{lin}$ from Equations (2) and (3), respectively). Panel (a) controls for origin $\times$ birth-cohorts fixed effects, age-at-migration $\times$ parent-education group fixed effects, and highest completed degree of the household head interacted with gender of household head, and highest completed degree of their spouse interacted with the spouse's gender. Panel (b) replaces the household head and spouse variables with family fixed effects. Maximum age at migration is 11 years. Column (1) reproduces baseline regression with the outcome measured for children age 12 - 20, Column (2) restricts the age of the child to 12 - 18 years, Column (3) restricts to 12 - 16 years, Column (4) to 12 - 14. Sample: one-time migrants from 2000 and 2010 Indonesian Population Census.

Table B.7: Education- and Gender-Specific Convergence

	Dependent Variable: Junior High School Graduation					
	(1)	(2)	(3)	(4)	(5)	(6)
	(a) Education					
	No family fixed effects			**With family fixed effects**		
$m_i \times \Delta_{odcp}^{JUNSEC}$ (own)	-0.020***		-0.022***	-0.029***		-0.015
	(0.002)		(0.003)	(0.007)		(0.008)
$m_i \times \Delta_{odcp}^{JUNSEC}$ (other)		-0.009***	0.002		-0.033***	-0.013*
		(0.002)	(0.002)		(0.005)	(0.006)
Sample Mean	0.72	0.72	0.72	0.71	0.71	0.71
Observations	1,036,024	1,036,024	1,036,024	266,976	266,976	266,976
	(b) Gender					
	No family fixed effects			**With family fixed effects**		
$m_i \times \Delta_{podcg}^{JUNSEC}$ (own)	-0.020***		-0.012*	-0.023***		-0.009
	(0.002)		(0.005)	(0.006)		(0.008)
$m_i \times \Delta_{podcg}^{JUNSEC}$ (other)		-0.020***	-0.009*		-0.025***	-0.021*
		(0.002)	(0.004)		(0.006)	(0.009)
Sample Mean	0.72	0.72	0.72	0.71	0.71	0.71
Observations	1,035,305	1,035,305	1,035,305	266,695	266,695	266,695

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors clustered two-way on origin and destination district (in parentheses). Reports location exposure effect on junior secondary school completion (JUNSEC15+) for children age 15 - 20 ($\hat{\gamma}^{lin}$ from Equations (2) and (3), respectively). In Panel (a), Δ_{podc}^{JUNSEC} (own) is the change in district quality based on educational outcomes of permanent residents of own parental education group and Δ_{podc}^{JUNSEC} (other) is the change in district quality based on educational outcomes of permanent residents of the other education group. In Panel (b), Δ_{podcg}^{JUNSEC} (own) is the change in district quality based on educational outcomes of permanent residents of the own gender, and Δ_{podcg}^{JUNSEC} (other) is the change in district quality based on educational outcomes of permanent residents of the other gender. Columns 1 and 2 control for origin $\times$ birth-cohorts fixed effects, age-at-migration $\times$ parent-education group fixed effects, and highest completed degree of the household head interacted with gender of household head, and highest completed degree of their spouse interacted with the spouse's gender. Columns 3 and 4 replace the household specific variables with household fixed effects. Maximum age at migration is 14 years. Sample: one-time migrants aged 15 to 20 from 2000 and 2010 Indonesian Population Census.

Table B.8: Age at Migration and Gender of the Youngest and the Oldest Child

	Dependent Variable: Age at Migration in Years	
	Gender of Oldest	**Gender of Youngest**
	(1)	(2)
Male	0.026***	0.006
	(0.008)	(0.003)
Sample Mean	4.01	2.44
Observations	1,077,706	500,359

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors clustered two-way on origin and destination district (in parentheses). Table shows the results of a regression of the age at migration of the oldest and the youngest child, respectively, on an indicator for their gender. The specifications control for origin $\times$ birth-cohorts fixed effects, highest completed degree of the household head interacted with gender of household head, and highest completed degree of their spouse interacted with the spouse's gender. Column (2) replaces the household head and spouse variables with family fixed effects. Maximum age at migration is 19 years. Sample: one-time migrants age 12 to 20 from 2000 and 2010 Indonesian Population Census.

Table B.9: Descriptive Statistics: Household Heads in the Census

	All (1)	Ever Migrated (2)	Migrated in Past 5 Years (3)	Regression Sample (4)
	(a) 2000 Census			
Age	39.40	37.73	33.26	43.65
Female	0.10	0.10	0.13	0.05
Any Children	0.81	0.74	0.59	1.00
Number of Children (if any)	2.31	2.30	2.07	3.67
Number of Children	1.87	1.70	1.23	3.67
Primary Education	0.78	0.89	0.91	0.82
Junior Secondary Education	0.38	0.62	0.67	0.45
Senior Secondary Education	0.24	0.45	0.51	0.31
Ever Migrated	0.27	1.00	1.00	0.67
Migrated in Past 5 Years	0.07	0.27	1.00	0.05
Observations	44,020,253	11,765,317	3,169,358	660,808
	(b) 2010 Census			
Age	41.18	39.42	32.02	43.52
Female	0.11	0.11	0.19	0.05
Any Children	0.81	0.74	0.49	1.00
Number of Children (if any)	2.09	2.08	1.82	3.18
Number of Children	1.69	1.54	0.89	3.18
Primary Education	0.87	0.94	0.97	0.95
Junior Secondary Education	0.50	0.73	0.83	0.73
Senior Secondary Education	0.33	0.55	0.67	0.56
Ever Migrated	0.29	1.00	1.00	0.78
Migrated in Past 5 Years	0.05	0.19	1.00	0.03
Observations	51,041,423	14,684,946	2,759,972	725,274

Note: Sample means of Indonesian household heads age 15 to 60. Panel (a) restricts to 2000 Census, Panel (b) to 2010. Column (1) includes all individuals, column (2) restricts to those who ever migrated. Column (3) restricts to those who migrated in the past five years, and column (4) includes the household heads in the regression sample that meet the age restriction.

Table B.10: Robustness Destination Effect on Junior Secondary School Graduation: Population Size, Inter-Province Migration

	Sample Restriction:					
	Baseline	**Pop $\geq$ 100**	**Pop $\geq$ 200**	**Pop $\geq$ 300**	**Migration Across Provinces**	**Migration Within Provinces**
	(1)	(2)	(3)	(4)	(5)	(6)
	(a) Baseline					
$m_i \times \Delta_{podc}^{JUNSEC}$	-0.020***	-0.020***	-0.020***	-0.021***	-0.020***	-0.018***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Sample Mean	0.72	0.72	0.72	0.72	0.72	0.71
Observations	1,036,024	1,033,637	1,023,606	1,006,262	569,434	466,563
	(b) With Household Fixed Effects					
$m_i \times \Delta_{podc}^{JUNSEC}$	-0.029***	-0.030***	-0.031***	-0.030***	-0.033***	-0.030**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.011)
Sample Mean	0.71	0.71	0.71	0.71	0.71	0.71
Observations	266,976	266,155	262,075	255,725	152,617	113,833

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors clustered two-way on origin and destination district (in parentheses). Reports location exposure effect on junior secondary school completion ($\hat{\gamma}^{lin}$ from Equations (2) and (3)). Panel (a) controls for origin $\times$ birth-cohorts fixed effects, age-at-migration $\times$ parent-education group fixed effects, and highest completed degree of the household head interacted with gender of household head, and highest completed degree of their spouse interacted with the spouse's gender. Panel (b) replaces the household head and spouse variables with family fixed effects. Maximum age at migration is 14 years. Column (1) reproduces baseline regression with Δ_{podc}^{JUNSEC} based on at least 30 permanent resident children in each birth cohort $\times$ parent education group $\times$ district cell, Column (2) restricts to at least 100 permanent resident children in each cell, Column (3) restricts to at least 200 children in each cell, Column (4) to 300 children. Column (5) restricts to migrations within the same province, and Column (6) restricts to migration across province borders. Sample: one-time migrants from 2000 and 2010 Indonesian Population Census.

Table B.11: Robustness Location Exposure Effect on Junior Secondary School Graduation: Birth Order and Number of Children

	Additional Control:				
	Baseline	**Birth Order**	**Family Size**	**Birth Order + Family Size**	**Birth Order**
	(1)	(2)	(3)	(4)	(5)
	No Household Fixed Effects				**With HH FE**
$m_i \times \Delta_{podc}^{JUNSEC}$	-0.020***	-0.020***	-0.020***	-0.020***	-0.029***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.007)
Sample Mean	0.72	0.72	0.72	0.72	0.71
Observations	1,036,024	1,036,024	1,036,024	1,036,024	266,976

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors clustered two-way on origin and destination district (in parentheses). Reports location exposure effect on junior secondary school completion ($\hat{\gamma}^{lin}$ from Equations (2) and (3), respectively). Columns (1) to (4) control for origin × birth-cohorts fixed effects, age-at-migration × parent-education group fixed effects, and highest completed degree of the household head interacted with gender of household head, and highest completed degree of their spouse interacted with the spouse's gender. Column (5) replaces the household head and spouse variables with family fixed effects. Maximum age at migration is 14 years. Column (1) reproduces baseline regression, Column (2) additionally controls for birth order, Column (3) controls for number of children in the household fixed effects, Column (4) controls for both, Column (5) controls for birth order in the household fixed effects specification. Sample: one-time migrants from 2000 and 2010 Indonesian Population Census.

Table B.12: Destination Effect on Junior Secondary School Graduation Bounding Exercise: Age At Migration

	Baseline (1)	**Version 1** (2)	**Version 2** (3)
(a) Baseline			
$m_i \times \Delta_{podc}^{JUNSEC}$	-0.020***		
	(0.002)		
$m_i^{v1} \times \Delta_{podc}^{JUNSEC}$		-0.010***	
		(0.003)	
$m_i^{v2} \times \Delta_{podc}^{JUNSEC}$			-0.027***
			(0.003)
Sample Mean	0.72	0.72	0.72
Observations	1,036,024	1,036,024	1,036,024
(b) With Family Fixed Effects			
$m_i \times \Delta_{podc}^{JUNSEC}$	-0.029***		
	(0.007)		
$m_i^{v1} \times \Delta_{podc}^{JUNSEC}$		-0.026***	
		(0.007)	
$m_i^{v2} \times \Delta_{podc}^{JUNSEC}$			-0.029***
			(0.007)
Sample Mean	0.71	0.71	0.71
Observations	266,976	266,976	266,976

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors clustered two-way on origin and destination district (in parentheses). Reports location exposure effect on junior secondary school completion ($\hat{\gamma}^{lin}$ from Equations (2) and (3), respectively). Panel (a) controls for origin $\times$ birth-cohorts fixed effects, age-at-migration $\times$ parent-education group fixed effects, and highest completed degree of the household head interacted with gender of household head, and highest completed degree of their spouse interacted with the spouse's gender. Panel (b) replaces the household head and spouse variables with family fixed effects. The *Version 1* speficiation increases the age at migration estimate by one year if the destination is of better quality than the origin, and decreases it otherwise. The *Version 2* specification decreases the age at migration estimate by one year if the destination is of better quality than the origin and increases it otherwise. Sample: one-time migrants from 2000 and 2010 Indonesian Population Census.

Table B.13: Location Exposure Effect on Marriage, Health, and Work

	Dependent Variable:			
	Ever Married	**Ever Married Male**	**Ever Married Female**	**Health Issue**
	(1)	(2)	(3)	(4)
	(a) Baseline			
$m_i \times \Delta_{podc}^{JUNSEC}$	0.001	-0.001	0.002	-0.000
	(0.002)	(0.002)	(0.002)	(0.000)
Sample Mean	0.05	0.05	0.05	0.01
Observations	503,016	268,283	234,732	503,016
	(b) With Household Fixed Effects			
$m_i \times \Delta_{podc}^{JUNSEC}$	0.021	0.031	0.003	0.001
	(0.015)	(0.017)	(0.016)	(0.002)
Sample Mean	0.05	0.05	0.06	0.01
Observations	117,127	37,482	28,083	117,127

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors clustered two-way on origin and destination district (in parentheses). Presents linear exposure effect coefficients from Equation (2) and Equation 3, respectively. The measure of quality change is with respect to junior secondary school completion. Dependent variable in Column (1) to (3) is whether a child was ever married, where Column (2) restricts to boys and Column (3) restricts to girls. The dependent variable in Column (4) is an indicator that takes the value 1 if a child has at least one of the following issues: problems seeing, hearing, concentrating, selfcare. Panel (a) controls for origin × birth-cohorts fixed effects, age-at-migration × parent-education group fixed effects, and highest completed degree of the household head interacted with gender of household head, and highest completed degree of their spouse interacted with the spouse's gender. Panel (b) replaces the household head and spouse variables with family fixed effects. Sample: one-time migrants aged 15 to 20 from 2010 Indonesian Population Census.

Table B.14: Location Exposure Effect on Completed Education And Labor Market Outcomes

	Dependent Variable:		
	Years of Schooling	Formal Employment	Log Income
	(1)	(2)	(3)
Age Migrated × Δ^{YR}_{odc}	-0.007*	-0.001**	-0.001
	(0.003)	(0.000)	(0.001)
Sample Mean	8.76	0.47	12.01
Observations	50,254	30,092	7,131

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors clustered two-way on origin and destination district (in parentheses). Reports location exposure effect ($\hat{\gamma}^{lin}$ from Equation 5) with district quality change measured in average years of education of permanent residents. Controls for birth cohort fixed effects and linearly for origin quality interacted with birth cohort fixed effects. Maximum age at migration is 20 years. The dependent variable in Column (1) is years of schooling, in Column (2) an indicator for employment in the formal labor market, and in Column (3) log income. Sample: one-time migrants aged 21+ from SUPAS1985, SUPAS1995, SUPAS2005.

C Intercensal Population Surveys (SUPAS)

A supplementary sample is drawn from the 1985, 1995, and 2005 waves of the Intercensal Population Survey (SUPAS). These data offer a detailed education measure and several questions on migration, which allow me to both identify one-time migrants as well as derive the age at migration for adults. While these data enable me to examine the impact of childhood neighborhoods on completed education without introducing cohabitation bias, it also means that I lack information on parental education.[34] In addition, the sample size is small, limiting the scope for heterogeneity analyses.[35]

The SUPAS, carried out for the first time in 1976, are household surveys with a small set of individual-level questions conducted midway between two population censuses. Due to data availability[36], I am using the 1985, 1995 and 2005 waves[37]. I fix districts to the 1985 borders to obtain harmonized neighborhoods. This results in 281 districts for the years 1985 and 1995, and 265 districts for the year 2005.[38]

Taking into account the trade-off between power and comparability, I restrict the data to the birth cohorts 1960 to 1984. I restrict the sample to all individuals who migrated exactly once before their 21^{st} birthday. To allow completion of their education, the individuals have to be at

[34] A sample consisting of adults who are still living with their parents and siblings would potentially be very selective.

[35] SUPAS interviews up to 0.5 percent of the Indonesian population. The sample of respondents is representative at the district level, but not necessarily representative of the migrant population.

[36] In 1976, only a smaller set of migration questions was included.

[37] I downloaded the 1985 and 2005 waves from IPUMS international (?), the data excludes the province of Aceh in 2005. I use a version of SUPAS 1995 provided to researchers by BPS (Central Bureau of Statistics) in Indonesia.

[38] The districts of the province Aceh are missing in the 2005 data.

least 21 years old at the time of the survey. 9.3 percent of this population has migrated exactly once during childhood.[39]

The focus on adults in this data comes with a limitation, which is the lack of parental information: most adults do not co-reside with their parents, and SUPAS does not record information on parents who live in other households. Therefore, the measure of district quality change Δ_{odc} is defined as the difference in average years of schooling of permanent residents in origin o and destination d, separately calculated for each birth cohort c.[40]

In the SUPAS data, I do not have to rely on siblings' information to construct year of migration estimates. Specifically, the 1985, 1995, and 2005 SUPAS waves contain a person's district (*kabupaten*) of birth, the district of residence five years prior, the previous district, and the current district of residence. This information allows me to directly identify individuals who have moved exactly once in their life. If the previous district is different to the current district but the previous district equals the birth district, I assume that a person only moved once.[41] Using a variable measuring years in current residence, I calculate the age at migration for one-time migrants.

D Representativity of the Regression Sample

To assess the representativity of the regression sample, Table B.9 shows descriptive statistics for Census household heads, with the 2000 Census in panel (a) and the 2010 Census in panel (b). Column (1) includes all household heads in the Census wave that are between 15 and 60 years old, column (2) restricts to those who have migrated at least once (as indicated by a district of residence that is different to the birth district), column (3) restricts to those who have migrated in the past five years, and column (4) shows the regression sample.

In 2000, household heads in the regression sample are on average 44 years old, which is a few years above the average age of all households, and in particular of those who have migrated in the past. This is not surprising given that household heads have to have at least two children to be included in the regression sample. This condition also explains a low share of female

[39]The rate of one-time migrants is higher in this sample than in the Census data. The reason is that the SUPAS allows to infer migration during the entire life time. In the Census, I measure migration that occurred between the birth of the oldest and the youngest child in the family, see Section 3.3.

[40]Permanent residents in this sample are those individuals who have not left their birth district before the age of 21.

[41]This classification is imperfect as it will over count the number of individuals who moved once, and under count those who moved more than once. For example, it might be the case that someone moved several times, then moved back to their birth district, and then moved away from it to their current district. I am able to identify some of these moves by taking into account the information recorded in the variable measuring district five years prior.

household heads (about five percent in the regression sample and about twice as much overall). The pattern is similar in the 2010 sample.

Migrants are on average less likely to have any children, in particular if they migrated recently. However, conditional on having children, average fertility is remarkably similar across groups (2.3 children in 2000 for all household heads and those who migrated). Mechanically, the regression sample has a higher average fertility (3.7 children). The patterns in the 2010 sample are remarkably similar, with fewer children across all groups.

There is clear positive selection into migration with respect to education. While in 2000, 78 percent of all household heads have at least primary and 38 percent have at least junior secondary education, these shares are 89 percent and 62 percent, respectively, for migrants (87 and 50 vs 94 and 73, respectively, in 2010). Recent migrants are even more selected. The regression sample is less educated than the average migrant in 2000, but remarkably similar in 2010.

E Validation of the Siblings-Based Approach With SUPAS Data

How good is the age at migration estimate based on siblings' births, and which fraction of childhood migration do I miss? To assess this question, I replicate the siblings-based migration estimate in a sample of children born in 1980 to 1988 and 1990 to 1993 who live with their parents in the 1995 and 2005 SUPAS waves.[42] In 97.5 percent of cases in this sample of 206,297 children, both the SUPAS-based approach (using current district, last district, district five years ago, and birth district) and the siblings-based approach (without last district, but taking into account siblings' birth districts) leads to the same number of migrations. However, conditional on being a migrant according to the SUPAS-based approach, this share falls to 84.4 percent. This is mostly driven by cases where the siblings-based approach misses migration spells (10.3 percent of cases). However, in 5.3 percent of cases, it is the other way around: the siblings-based approach reveals an intermediate location which cannot be detected with the information provided by current district, last district, district five years ago, and birth district. Interestingly, for migrants according to the SUPAS-based approach, the share of cases with disagreements between the approaches is slightly higher for children with better educated families: It is 16.4 percent in families where at least one parent has at least finished junior secondary school, while it is 14.8 percent in families where the highest parental education is less than junior secondary

[42]Since I want to have a comparison to the Census sample, I select the children who are between 12 and 20 in the survey, which implies that I cannot observe children born after 1993.

graduation. While statistically significant at the 1% level, the difference of 1.5 percentage points is quite small and therefore does not raise concerns of systematic selection into the regression sample. In addition, among one-time migrants according to the SUPAS-based approach, the share observations with an age at migration estimate from the siblings-based approach is 2.8 percentage points higher in more educated families.

By how much do the age at migration estimates of the two approaches differ? Figure A.10 plots a histogram of the difference between the age at migration estimate based on the information provided by SUPAS and the siblings-based approach, where positive values indicate that the siblings-based approach estimates earlier migration. Reassuringly, the histogram is centered around zero, with some more mass on the right hand side. In 24.5 percent of the cases, both estimates are exactly the same, and in an additional 38.1 percent of cases, they only differ by a year. Large differences are rare: In 92.7 percent of observations, the absolute difference is four years at most. Figure A.11 subsequently tightens the maximum age gap between the two defining siblings in the siblings-based approach from eight years in the baseline, to six, four, and two years, respectively. Evidently, the difference in age at migration estimates decreases with a tightening of the siblings' age bounds. Figure A.12 reveals that the difference in age at migration estimates decreases with more children in the household. This is not surprising given a negative relationship of the defining sibling age gap and the number of children per household. Again, the systematic differences along the dimension of parental education are small: While the average absolute difference is 1.60 years in families with highest parental education that is below junior secondary completion, the average difference is 1.67 years in households with more parental education. The difference between these two means is significant at the 10% level.

F Additional Robustness Checks

Precision of $\Delta_{odcp}^{JUNSEC15+}$. The measure of district quality change $\Delta_{odcp}^{JUNSEC15+}$ is based on the average educational outcomes of children in parental education group × birth cohort × district cells. In the baseline results of Section 4.1, I exclude all observations where either the parental education group × birth cohort × district cell of the origin or the destination includes less than 30 permanent residents. To improve precision, I increase this size cut-off to 100, 200, and 300 permanent residents for each cell. Both the point estimates and the confidence intervals move very little (Table B.10, Columns (2) to (4)).

Migration within and across provinces. If children migrate to an adjacent district, it is possible that they still have access to the same schools. To test whether the exposure effect is different for farther away migration, I repeat the analysis for a sample of children who moved across province borders (Table B.10, Column (5)) and within a province (Column (6)), respectively. Indeed, the latter coefficient is somewhat attenuated relative to the one from across-province migration in both specifications, but they are statistically indistinguishable from each other.

Birth Order and Family Size. Another way to test whether the siblings-based approach to infer age at migration has an impact on the estimation is to control for birth order. Appendix Table B.11 shows that this does not have an impact, neither in the baseline specification (Column (2)) nor in the household fixed effects specification (Column (5)). Similarly, controlling for family size (Column (3)) or family size and birth order (Column (4)) does not impact the coefficient.[43]

G Bounding Exercise

To further explore the impact of non-classical measurement error, I implement a bounding exercise which estimates the bias from systematically misidentifying the actual age at migration. That is, I might systematically overestimate the age at migration in families that move to better districts, and underestimate it in families that move to worse districts. To estimate the extent of this bias, I generate a new variable that increases the age at migration estimate by one year if the destination is better than the origin, and decreases it by one year otherwise:

$$m_i^{v1} = \begin{cases} m_i + 1 & \Delta_{odcp}^{JUNSEC} > 0 \\ m_i - 1 & \text{otherwise} \end{cases}$$

Equivalently, I define m_i^{v2} for the opposite case:

$$m_i^{v2} = \begin{cases} m_i - 1 & \Delta_{odcp}^{JUNSEC} > 0 \\ m_i + 1 & \text{otherwise} \end{cases}$$

Table B.12 reports results of this exercise. Compared to the baseline estimate of $\gamma^{lin} = -0.02$, the estimate using m_i^{v1} is biased downwards in magnitude to -0.10 while using m_i^{v2} results in a larger estimate of -0.027. Similarly, the magnitude of the exposure effects estimate in the household fixed estimation is somewhat smaller when using m_i^{v1}, but is unchanged when using m_i^{v2}. While classical measurement error attenuates the location exposure effects estimation, the

[43] I control for family size by including number of children fixed effects.

sign of non-classical measurement error is ambiguous.

H Other Outcomes

Part of the causal location effect could be factors that improve educational outcomes by reducing the likelihood of child marriage, child labor, or leading to better health. However, I do not find evidence for any of these explanations. Table B.13 repeats the analysis for different outcomes measured in 2010. While the baseline specification finds no evidence for the effect to work through child marriage, the fixed effects specification indicates that there is a negative exposure effect on child marriage, in particular for boys, indicating that spending more time in a better district decreased the likelihood to be married. However, none of the estimates are statistically significant. In comparison, **?** finds no urban exposure effects on marriage or fertility in Africa. I do not find any effect on severe health issues.[44]

So far, the analysis focused on young adults, and outcomes are measured just a few years after migration. How persistent are these effects and are they relevant later in life? I complement the analysis by constructing a second sample of one-time childhood migrants, based on the Intercensal Population Survey (SUPAS). This sample consists of adults who migrated exactly once before the age of 21. In this data I observe both completed education measured and several labor market outcomes. Unfortunately, parent characteristics are not available, and the sample size is by an order of magnitude smaller than the Census sample.

The estimation equations are variants of Equations (1) and (2), respectively:

$$y_i = \sum_{m=1}^{M} \beta_m \mathbb{1}(m_i = m)\Delta_{odc}^{YR} + \sum_{c=1}^{C} \mathbb{1}(c_i = c)\big(\theta_c^1 + \theta_c^2 \bar{y}_{oc}\big) + \ \ \epsilon_i \tag{4}$$

$$y_i = \beta^0 \Delta_{odc}^{YR} + \gamma^{lin} m_i \times \Delta_{odc}^{YR} + \sum_{c=1}^{C} \mathbb{1}(c_i = c)\big(\theta_c^1 + \theta_c^2 \bar{y}_{oc}\big) + \ \ \epsilon_i \tag{5}$$

where I define location quality change as $\Delta_{odc}^{YR} = \bar{y}_{dc} - \bar{y}_{oc}$, which is equivalent to Δ_{odcp}^{S}, with the exception that it does not distinguish between the two groups of parental education. In this sample, education is always measured in terms of years of schooling, denoted by the superscript YR. Considering the sample size, which includes at most 50,000 observations depending on the outcome variable, controlling for origin $\times$ birth-cohort fixed effects would be

[44] These are defined as having at least one of the following issues: problems seeing, hearing, concentrating, self care.

too demanding. Therefore, I control for origin by linearly including origin quality interacted with birth cohort fixed effects.

Figure A.13 Panel (a) shows $\hat{\beta}_m$ at every age at migration m, and the 95% confidence interval. The outcome here are years of schooling, which likely reflects completed education since individuals in my sample are at least 21 years old. The coefficients $\hat{\beta}_m$ decline with age at migration, confirming positive destination exposure effects from childhood migration. Panel (b) implies that migrating a year earlier to a destination where permanent residents of the same cohort have on average a year more schooling compared to the origin is associated with 0.008 more years of education for the migrant. Migrating to a one standard deviation better district yields 0.017 more years of school.

Compared to my findings on graduation probabilities in Section 4.1, the effects are attenuated. However, the two analyses are not directly comparable since individuals in the SUPAS sample obtained education in earlier decades[45], when the supply of schools was much lower. The individuals in the SUPAS sample were born in 1960-1984, so that about 18 percent of the sample was twelve or older when the first primary school under the school expansion program INPRES was constructed in 1974. Only about 10 percent were born in or after 1978, so that they would enter school after the majority of primary schools had been constructed (**?**).[46] The lack of schools for those early-born cohorts could limit the scope for positive location effects.

Do these positive location effects on education translate into better labor market outcomes? Unfortunately, data availability is a limiting factor as salary/wages are only included in the 1995 sample. All waves, however, contain information on the "status" of a worker with the alternatives "self-employed" "wage/salary worker" and "unpaid worker"[47]. To obtain insights on labor market effects, I will again estimate the linear specification from Equation (5) with Δ^{YR}_{odc} defined as before, but y_i being a binary variable which takes the value 1 if a person is employed in the formal sector (i.e. reports being a wage/salary worker) and 0 if she is working in the informal sector (i.e. reports being self-employed or an unpaid worker). About half of the individuals in my sample are employed in the formal sector. Additionally, I estimate the effect on log monthly income for the 1995 sample.

The reduced form results (reported in Table B.14) provide suggestive evidence for positive destination exposure effects in the labor market. The coefficient on formal employment is significant, but very small in magnitude (Column (2)). It implies that migrating a year earlier to

[45] Only the birth cohorts 1980 to 1985 overlap in both samples.

[46] Individuals in the Census samples were born in 1980-1988 and 1990-1998, and entered primary school after the vast majority of schools under the INPRES program had been constructed.

[47] Additionally, there is "unknown/missing" and "not in universe". The latter is an important category, since the variable is only identified on individuals 10+ who are working or are employed. The majority of inactive individuals are women.

one standard deviation better destinations increases the likelihood of being employed in the formal sector by about 0.2 percentage points or 0.4 percent. The effect on log monthly income is small and not significant, which could be due to little statistical power in the relatively small data set. However, the sign is consistent with positive exposure effects.

The SUPAS data allow me to estimate the extent of selection into migration. Figure A.14 plots $\hat{\beta}_m$ for individuals who migrated not only during childhood, but until the age of 40.[48] While migration during childhood has a causal effect on education, migration later in life is unlikely to have such an effect, since most individuals complete education in their late teens or early twenties.[49] The figure therefore reveals the extent of selection into migration: the average value of $\hat{\beta}_m$ for $m \geq 21$ is $\delta = 0.23$. By assumption, selection does not vary with age at migration.

[48]Very few individuals in my sample migrated between the ages of 41 and 45, which is why I omit them from this figure.

[49]In rare cases an individual might attend college at a higher age, therefore it is not entirely impossible for migration to have an effect later in life.