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Richard Akresh
Daniel Halim
Marieke Kleemans

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Long-term and Intergenerational Effects of Education: Evidence from School Construction in Indonesia
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ABSTRACT

In 1973, the Indonesian government began one of the largest school construction programs ever. We use 2016 nationally representative data to examine the long-term and intergenerational effects of additional schooling as a child. We use a difference-in-differences identification strategy exploiting variation across birth cohorts and regions in the number of schools built. Men and women exposed to the program attain more education, although women's effects are concentrated in primary school. As adults, men exposed to the program are more likely to be formal workers, work outside agriculture, and migrate. Households with parents exposed to the program have improved living standards and pay more government taxes. Education benefits are transmitted to the next generation. Increased parental education has larger impacts for daughters, particularly if mothers are exposed to school construction. Intergenerational results are driven by changes in the marriage partner's characteristics, with spouses having more education and improved labor market outcomes.

Richard Akresh
Department of Economics
University of Illinois at Urbana-Champaign
1407 West Gregory Drive
214 David Kinley Hall
Urbana, IL 61801
and NBER
akresh@illinois.edu

Daniel Halim
Department of Economics
University of Illinois at Urbana-Champaign 1407
West Gregory Drive
214 David Kinley Hall
Urbana, IL 61801
dzhalim2@illinois.edu

Marieke Kleemans
Department of Economics
University of Illinois at Urbana-Champaign
1407 West Gregory Drive
214 David Kinley Hall
Urbana, IL 61801
kleemans@illinois.edu

An online appendix is available at http://www.nber.org/data-appendix/w25265

1. Introduction

The questions of which adult outcomes are affected by increases in educational attainment and whether these effects persists into the next generation are of great policy importance and broad research interest. Governments in developing countries spend approximately one trillion dollars annually on education, and households are estimated to spend hundreds of billions more on the education of their children (Glewwe and Muralidharan, 2016). While much of the government spending is motivated by the belief that increases in education will translate to higher economic development and growth, the causal effect of schooling on economic growth is not uncontested.[1] An extensive literature in macroeconomics and growth has pointed to a high correlation between cross-country differences in per capita income and in education, but some have argued that these may reflect reverse causality of increased educational attainment in anticipation of high rates of economic growth (Bils and Klenow, 2000).[2]

Microeconomic analyses of the returns to schooling date back to Gorseline (1932) and Walsh (1935) and have long recognized that without (quasi) exogenous variation in educational attainment, the causal impact of education is hard to estimate because the choice of how much education to obtain is correlated with a large number of individual, household, and community characteristics. In recent years, major strides forward have been made using randomized experiments, but reviewing 111 primary school interventions in developing countries, McEwan (2015) finds that only 10 percent had any evaluation taking place more than one month after the

[1] In the early nineties, theories endogenizing technology (such as Romer, 1990 and Grossman and Helpman, 1991) were motivated by the belief that cross-country differences in human capital could not quantitatively explain the differences in levels and growth rates of per capita output (Klenow and Rodriguez-Clare, 1997, 2005). Several later empirical papers challenge that belief showing that a Solow model augmented to include human capital can explain the lion's share of cross-country variance in output per capita (Mankiw, Romer, and Weil, 1992; Young, 1994, 1995; Barro and Sala-i-Martin, 1995).
[2] Foster and Rosenzweig (1996) also find evidence of this direction of causality by documenting that Indian provinces that benefited from the Green Revolution saw increases in returns to, and enrollment in, schooling.

intervention had ended.[3] While the focus on measuring early life outcomes is understandable given that primary education provides the foundation for subsequent educational attainment, the ultimate goal is improvements in later life outcomes and overall economic development.

In this paper, we study the causal impact of one of the largest primary school construction programs ever completed on a wide range of long-term and intergenerational outcomes, including those related to education, employment, migration, living standards, taxes, marriage health, housing and assets. Between 1973 and 1979, the Indonesian government constructed over 61,000 primary schools, averaging two schools per 1,000 children of primary school age. We use 2016 nationally representative Indonesian data to examine the long-term and intergenerational effects of additional schooling as a child. Following the seminal work by Duflo (2001) who studies the effects of this school construction program on men's education and earnings in 1995, we employ a difference-in-differences strategy, exploiting variation across geographic regions in the number of schools built and across birth cohorts in their exposure to the schools.

The paper makes the following contributions. First, we estimate the causal impact of the school construction program on an extensive range of outcomes, many of which researchers have not previously studied. Second, we do so at a time that those exposed to the program are in their forties and fifties, giving us a unique look at the persistence of the effects over time. This type of long-term analysis is important for policy evaluation, but is uncommon and existing evidence on the persistence of education interventions is mixed.[4] Third, the long time horizon and detailed

[3] Notable exceptions include Baird, Hamory Hicks, Kremer, and Miguel (2016) who show positive labor market impacts 10 years after a deworming intervention in Kenya, and Gertler et al. (2014) showing higher earnings 20 years after an early child stimulation program in Jamaica. Evidence from the U.S. indicates preschool and kindergarten programs lead to improved adult outcomes (Garces, Thomas, and Currie, 2002; Heckman et al., 2010; Chetty et al., 2011) as do health interventions (Bhalotra and Venkataramani, 2018).

[4] For example, Evans and Ngatia (2018) find that positive outcomes from a free school uniform program fade out over time and are no longer observable eight years after the intervention. Andrabi et al (2015) using data from Pakistan find that only one-fifth to one-half of student learning persists between grades. Jacob, Lefgren, and Sims (2010) find low persistence of teacher learning in the U.S. with three-quarters or more fading out within one year.

household-level data allow us to observe intergenerational effects on children whose parents were exposed to the program and study impacts on the children's educational attainment and basic measures of well-being.[5] Fourth, we extend the focus on working-age men in Duflo (2001) to also study the impact of school construction on women. This allows us to study gender differences for both the first and second generation outcomes and explore marriage market outcomes, which appear to play a crucial role in the intergenerational transmission of human capital. Finally, while most of the education research evaluates demand-side interventions, we study the impacts of a supply-side educational intervention, with large up-front costs and benefits dispersed over time. We perform a detailed cost-benefit analysis to calculate the internal rate of return, and using tax data, we evaluate whether school construction pays for itself with higher future government tax revenues.

Figure 1 provides an overview of our findings. Due to the richness of the data and the sheer number of outcomes we can explore, we want to be careful in avoiding an overemphasis on any single significant result and so we take two main approaches. First, following Kling, Liebman, and Katz (2007), we create an index for each family of outcomes where we aggregate all the individual outcomes in that family together. As described in more detail in Section 3, we then estimate standardized effects from exposure to the school construction program on a range of these outcome indexes (Banerjee et al., 2015). Second, since we examine multiple outcomes, we correct for the potential issue of simultaneous inference using multiple hypothesis testing. We

[5] Black and Devereux (2011) review the large literature on the intergenerational transmission of human capital that measures the persistence between parents' and children's educational attainment, while Currie (2011) and Almond and Currie (2011) provide a review of the long-term effect on education of negative shocks while in utero or early childhood. In addition to the focus on the estimation of correlations between parent and child educational outcomes, recently there is an increased emphasis on estimating causal relationships. Researchers have used changes in school compulsory laws (see Chevalier, 2004 for U.K; Black, Devereux, and Salvanes, 2005 for Norway; Oreopoulos, Page, and Stevens, 2008 for U.S.), other educational policies (Currie and Moretti, 2003; Maurin and McNally, 2008), and environmental shocks (Black et al., forthcoming) to estimate these effects. There is however limited evidence from developing countries.

calculate q-values using the Benjamini-Hochberg step-up method to control for the false discovery rate (Benjamini and Hochberg, 1995).

The consistent pattern seen in Figure 1 is that exposure to the school construction program improves almost every family of outcomes that we are able to explore in the data.[6] School construction, not surprisingly, leads to improved educational outcomes. Duflo (2001) previously showed this for men, and we are now able to confirm that it also improves women's education.[7] The education effects for women are concentrated in primary school only, while men also see significant increases in lower and upper secondary education. As adults, men who were exposed to the program are more likely to be employed, to work in the formal sector, and to work in the non-agricultural sector, while the likelihood of migration increases for both men and women. Households in which either parent were exposed to the program have higher living standards, better housing, more assets, and pay more government taxes. While nutrition and health investments increase, we do not observe any improvements in health outcomes. School construction leads to improved marriage market outcomes, with spouses being more educated, more likely to be literate, and healthier.

Parents transmit these effects to the next generation, who have more education, with larger impacts observed in secondary and tertiary education. Second generation children whose parents were exposed to the school construction program are less likely to be working, but as with the first generation results, we do not find any evidence of improved health outcomes. Increased parental education has larger impacts for daughters, particularly if the mother was

[6] An increase of one additional school built per 1,000 children would increase these indexes for those exposed to the school construction by 0.02 to 0.07 standard deviations relative to the control group.

[7] In addition to Duflo (2001) focusing on Indonesia, studies evaluating school construction projects have been carried out in Mozambique (Handa, 2002), Pakistan (Alderman, Kim, and Orazem, 2003), Afghanistan (Burde and Linden, 2013) and Burkina Faso (Kazianga et al., 2013). These studies focus on improvements in enrollment rates, as opposed to later-life outcomes, and all confirm large increases in school enrollment.

exposed to the school construction program. We perform a mediation analysis indicating that the intergenerational transmission of human capital appears to be driven by changes in the parents' marriage market outcomes, especially whether the spouse has completed primary school, is literate, works in the formal sector, and outside of agriculture.

To quantify the policy implications, we conduct an extensive cost-benefit analysis in which we create an accounting model to calculate the discounted costs of school construction and subsequent benefits for the government in terms of increased tax revenues and overall improved living standards for the Indonesian population. Across a range of different parameter estimates, we find that the school construction program leads to increased government tax revenues that will directly offset school construction costs in most cases within 40 years. Furthermore, taking into account the improved welfare and living standards of the Indonesian population reveals high internal rates of return ranging from 13-21 percent and benefits surpassing costs within 17-30 years after the schools were built. These results provide strong support for the cost-effectiveness of supply-side interventions.[8]

The rest of this paper is organized as follows. Section 2 describes the institutional context and school construction program in Indonesia. Section 3 describes the empirical identification strategy and the data. Section 4 presents the results examining the effects of exposure to school construction on a range of long-term outcomes and Section 5 discusses the intergenerational

[8] Recent education research has typically focused on evaluating demand-side interventions that include either information-based interventions (see Jensen, 2010 for the first study of this type that provided information to parents about the returns to schooling), cash transfer programs (see Fiszbein et al. 2009 for an overview; Behrman, Parker, Todd, 2011 for evidence on the medium-term impacts of the Mexican conditional cash transfer program Progresa; Parker and Vogl, 2017 for evidence on Progresa's long-term impacts; Baird et al., 2011, Akresh, de Walque, and Kazianga, 2013, 2016; and Benhassine et al., 2015 for research that explores the role of conditionality in these cash transfer programs), scholarship programs (see Kremer, Miguel, and Thornton, 2009 for one of the first studies to examine the impact of merit-based scholarships), or other household level interventions (see Oster and Thornton, 2011 for evidence on providing female sanitary products to secondary school girls; and Muralidharan and Prakash, 2017 for evidence from providing bicycles to families).

effects. Section 6 shows results of a number of robustness checks. Section 7 presents the cost-benefit analysis and Section 8 concludes.

2. Institutional Context

Indonesia is the fourth most populous country in the world and the seventh largest economy in terms of total GDP at purchasing power parity. The country has experienced over 40 years of high economic growth. Beginning from Soeharto's rise to power in 1967, Indonesia's Ministry of National Development Planning (*Bappenas*) outlined their plans for national development and the reduction of poverty in a series of Five-Year Development Plans (*Repelita*). One important part of these plans included the establishment of the "presidential instructions" (INPRES) program, which set up a system for distributing revenues from the central government to lower administrative levels. Starting with the oil boom in 1973, the central government emphasized the explicit goal of reducing regional disparities (Ravallion, 1988).

As part of this redistribution goal, the government began a nationwide school construction program, the Sekolah Dasar INPRES, which was one of the first and largest INPRES programs. Between 1973 and 1979, around 61,800 primary schools were constructed. Enrollment rates in 1972 before the start of school construction were 71 percent among primary school-age children. By 1978, enrollment rates among this age reached 85 percent.[9] Prior to this program in 1973, capital expenditures in education were low and enrollment rates in the few years before school construction began were stagnant (World Bank, 1989).

School construction nearly doubled the stock of primary schools from a baseline of around 63,000 primary schools. On average, the program added over 200 schools per district or two schools for every 1,000 children of primary school age. There was large heterogeneity across

[9] World Bank Databank. 2018. "Adjusted Net Enrollment Rate, Primary (% of Primary School Age Children)" (Accessed on October 17, 2018: databank.worldbank.org)

districts in how many schools the government built as the government designed the school construction program to target regions in which enrollment was initially lower.[10] The government designed each school for 120 students, and they recruited teachers and paid their salaries for these newly constructed schools. During the same period, the government attempted to train new teachers, and the percentage of teachers who met the minimum qualification of having an upper secondary school degree did not change over this period (World Bank, 1989).

3. Empirical Strategy and Data

3.1 Difference-in-differences

Following Duflo (2001), we estimate a difference-in-differences specification in which an individual's region of birth and date of birth jointly determine their exposure to the INPRES school construction program. Children in Indonesia typically attend primary school between the ages of seven to twelve. INPRES school construction started during the 1973-1974 school year, so children who were born in or before 1962 were at least 12 years of age in 1974 and would not have benefited from the school construction.[11] Children younger than seven in 1974 would have been exposed to the full potential benefits of the newly constructed schools. Children who were of primary school age in 1974 might partially benefit from the new INPRES schools as some of them were induced to enroll, and their propensity to enroll likely decreased with the child's age.

In addition to variation across birth cohorts, there was considerable variation across geographical regions in the intensity of the school construction program. This was because the program intensity (how many schools were constructed) was linked to the regions' primary

[10] Figure 2 presents a map of Indonesia indicating the geographical distribution of the number of schools constructed in each district.

[11] The 1993 Indonesian Family Life survey indicates that less than 3 percent of individuals born between 1950 and 1962 were still in primary school in 1974. As a further check, we use the 1976 Intercensal Survey and find that only 4.3 percent of individuals born between 1950 and 1962 were still in primary school in 1976.

school enrollment rate in 1972 (prior to the school construction). Areas that had low prior enrollment rates benefited more from the program and had more schools built, while areas with high prior enrollment rates had fewer additional schools built.

Exploiting these two sources of variation (birth cohort and geographical), we estimate the effect of school construction in the following regression:

$$y_{ijt} = \alpha + \beta\, School_j \cdot Young_{it} + \left(\boldsymbol{X}_j \boldsymbol{B}'_t\right) \gamma_t + \mu_j + \delta_t + \varepsilon_{ijt} \quad (1)$$

where y_{ijt} is the outcome of individual *i* born in district j in year *t*, $School_j$ measures the number of schools constructed by the INPRES program between 1973 and 1979 per 1,000 children in the individual's birth district j. It is important to use an individual's birth district instead of current district of residence because the latter may be endogenous to program placement if households move in order to provide access to schools for their children.[12] $Young_{it}$ is an indicator variable for being born between 1968-1972 (ages 2-6 in 1974) and thus being young enough to have benefitted from the program. Individuals born between 1957 and 1962 (ages 12-17 in 1974) represent older birth cohorts that were not exposed to the construction program. Following Duflo (2001), we exclude individuals born between 1963 and 1967 (ages 7-11 in 1974) as they might have only partially benefited from the school construction. We perform several robustness checks to confirm our results are consistent across various definitions of exposed and unexposed cohorts.[13] μ_j are time-invariant district of birth fixed effects, δ_t are cohort of birth fixed effects,

[12] In the African context, child fostering, where the biological parents send their own child to live with another family, is quite common and often done to send the child to school (Akresh, 2004, 2009). In the Indonesian context, child fostering is much less common (Marazyan, 2012).

[13] One of the reasons for Duflo (2001) to restrict the young cohort to those born before 1972 is so that these cohorts would have completed schooling and begun participating in the labor market by 1995, the survey year of the data she uses. The 1972 cohorts turned 23 in 1995, which is old enough to have completed tertiary education. With our 2016 data, cohorts born after 1972 would have also been exposed to the school construction and had sufficient time to complete school and join the labor market. In the robustness checks discussed in Section 6, we explore the robustness of the results to alternative cohort definitions. In particular, we show that results are robust to adding in younger cohorts (born 1973-1980), older cohorts (born 1950-1956), and partially exposed cohorts (born 1963-1967).

and $\boldsymbol{X}_j \boldsymbol{B}_t'$ is intended to control for district-specific time-varying factors that might influence outcomes. Following Duflo (2001), we do this by interacting birth cohort indicators with the district enrollment rate in 1971 and with the presence of water and sanitation programs in the district.[14] Note that we closely follow Duflo (2001) with the only exceptions that, unlike Duflo, we cluster our standard errors, and we do so at the district level, and that our data allows us to estimate the effects of school construction on both men and women. To allow for gender heterogeneity, we estimate Equation (1) separately for men and women.[15]

We are able to explore both individual and household-level variables to examine impacts of exposure to the school construction program. For data collected at the household level, such as expenditures and assets, we use the birth cohort and region of birth of the household head or the spouse and present results in separate panels for men and women.[16] In Equation (1), j refers to the district of birth of the man or woman, while t refers to their year of birth.[17]

[14] We use the district enrollment rate in 1971 because school construction program intensity was tied to the 1972 district enrollment and not controlling for pre-program enrollment might bias the results as there could be mean reversion even in the absence of the INPRES program. In addition, the oil boom, which provided the financial resources for the school construction, could have also provided the resources for other government programs that were correlated with INPRES schools placement. Water and sanitation programs were the second largest set of INPRES programs delivered by the central government.

[15] Given the school construction program targeted less developed areas, we want to show that our effects are not explained by general catching up (or mean reversion) from those areas, as this would violate the parallel trends assumption. To test this identification assumption, we estimate placebo regressions in which we compare the old cohort (ages 12-17 in 1974) with an even older cohort (ages 18-24 in 1974). Results are discussed in Section 6 and show there are no differential time trends in outcomes prior to the school construction. Further, in Appendix Figure A.1, we estimate a regression where we interact the number of INPRES schools constructed in one's birth district with an indicator for age in 1974. We omit the age group 19-24 in 1974 from the regression so that we are comparing each age against this older cohort. For both men and women, we do not observe any differential trend effects for non-exposed ages.

[16] Female household heads represent 13.8 percent of the sample and are included in the regressions for women. Results are robust to estimating the regressions separately for household heads and spouses, irrespective of gender. Note that in the household level regressions the analysis for men does not condition on the women's exposure to school construction. Likewise, for women, the analysis does not condition on the men's exposure.

[17] This assignment is arguably the most natural way to define exposure for household-level outcomes as it is possible to have multiple individuals living in a household and these individuals could be in the old, young, and intermediate birth cohorts. For example, a household with the household head born in 1962, his wife born in 1968, his younger brother born in 1965, and his sister-in-law born in 1970 would yield potentially four individuals of which one is in the old birth cohort (1957-1962), one in the intermediate cohort (1963-1967), and two in the young cohort (1968-1972). Robustness checks discussed in Section 6 show that this assignment decision does not influence the results.

The duration between the school construction that started in 1973 and the data collection that took place in 2016 allows us to not only study the long-term effects of exposure to the program but also to study the effects of school construction on the next generation's outcomes. Specifically, we can estimate the impact on children's schooling and other child outcomes based on whether their mother or father (or both) was exposed to the INPRES school construction program. We estimate the reduced-form relationship between second generation outcomes and the INPRES schools construction program in the following regression:

$$y_{ijtca} = \alpha + \beta\, School_j \cdot Young_{it} + \left(\boldsymbol{X}_j \boldsymbol{B}_t'\right) \gamma_t + \mu_j + \delta_t + \theta_a + \varepsilon_{ijtca} \qquad (2)$$

where y_{ijtca} denotes the outcome of child c who is age a, born to a parent i who was born in district j in year t, $School_j$ is the number of schools constructed in the father's or mother's birth district, $Young_{it}$ indicates if the father or mother belongs to the young cohort, and θ_a is child c's age fixed effect.[18] Standard errors are clustered at the father's or mother's birth district.[19]

3.2 Strategies to address the large number of outcomes

We adopt two main strategies to deal with the large number of outcomes that we examine in order to avoid overemphasizing any single significant result. First, as mentioned in the introduction, we create indexes for each family of outcomes following Kling, Liebman, and Katz (2007). These indexes combine all of the individual outcomes in each family of outcomes. To construct the indexes, we first define each outcome so that higher values correspond with better outcomes. Then we standardize each outcome into a Z-score by subtracting the mean and

[18] We include child age fixed effects because parents in the old cohort will mechanically have older children on average than parents in the young cohort and older children have more chance to complete more years of schooling than younger children. Therefore, the marginal benefit to the children's years of schooling has to be estimated across different households but among children of the same age.

[19] As is common in household surveys, Susenas 2016 identifies all household relationships with respect to the household head. If a child is not the biological or adopted child of the household head and spouse, the child will be recorded as 'other household member'. Therefore, our intergenerational analysis is restricted to children of the household head and spouse.

dividing by the standard deviation of the older cohort born in low intensity regions. We then average all of the Z-scores and then standardize the average relative to the older cohort born in the low intensity regions.[20] We then estimate the effect of exposure to the school construction program on these standardized outcome indexes.

Second, since we examine multiple outcomes, we correct for the potential issue of simultaneous inference using multiple hypothesis testing. Following Benjamini and Hochberg (1995), we use the concept of a false discovery rate (FDR) to allow inference when we are conducting many tests. Intuitively, the FDR allows the researcher to tolerate a certain number of tests to be incorrectly discovered. An FDR adjusted q-value of 0.05 implies that 5 percent of significant tests will result in false positives, compared with an unadjusted p-value of 0.05 that implies that 5 percent of all tests will result in false positives. In all of the regression tables, we present standard errors (and stars indicating statistical significance) based on the regular unadjusted p-values and also FDR adjusted q-values that address the multiple hypotheses being tested in a given family of outcomes.

3.3 Data

To measure the impact of this school construction program, we use Duflo's data of the Sekolah Dasar INPRES program that reports the number of schools constructed in each district between 1973 and 1979.[21] We combine the data on school construction with the National Socioeconomic Survey conducted in 2016, henceforth Susenas 2016, which is administered by Indonesia's Central Statistics Bureau, Badan Pusat Statistik. Susenas 2016 is a nationally representative

[20] This is the approach used by Banerjee et al. (2015) in evaluating the effect of poverty graduation programs across six different countries on a range of outcomes. Ajayi and Ross (2017) who are not evaluating a randomized control trial modify this standardization approach to use with a difference-in-difference empirical identification strategy that does not have a randomly assigned control group.

[21] We are grateful to Esther Duflo for sharing these data.

household survey that covers all 34 provinces and 511 districts of Indonesia.[22] The data combines a large sample size of 291,414 households and 1,048,575 individuals with a wide range of variables, including on education, employment, migration, living standards, taxes, housing and assets, nutrition, health, marriage market and demographic outcomes, welfare program participation, and educational outcomes for the next generation.[23]

Summary statistics are presented for each family of outcomes in Tables 1 to 12, which will be discussed in the next section together with the estimated results of the INPRES school construction program. For the birth cohorts that our analysis will focus on (born 1957-1962 for the old cohort and 1968-1972 for the young cohort), households have on average just over four members and the sample is evenly split between men and women. Average completed years of schooling for individuals in these cohorts is 8.0 years for men and 7.1 years for women. Approximately 81 percent of men and 73 percent of women have completed primary school. These individuals have lower rates of lower and upper secondary school completion (39 and 34 percent for men respectively and 31 and 26 percent respectively for women). Tertiary completion rates are only 9.5 and 7.7 percent for men and women.

These individuals would be ages 44 to 48 (young cohort) and 54 to 59 (old cohort) at the time of the survey in 2016. Most men are working (95 percent), while women have lower labor force attachment (64 percent). Conditional on working, only 33 percent of men and 24 percent of women are in the formal labor market. Just over half of men and women work in the non-agricultural sector and around one-quarter of them have migrated from their birth district.

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[22] The smallest geographical unit in the Susenas 2016 is the Indonesian '*kabupaten*', loosely translated as district.

[23] Susenas 2016 is particularly suitable to study the effects of the school construction program because it includes information on the individual's district of birth and because the sample is large enough to be able to precisely estimate the observed relationships. Appendix B provides further rationale for the choice of data, in particular showing that the sample for the Indonesia Family Life Survey (IFLS) is not large enough to detect the effects of school construction. This is confirmed by Bharati, Chin, and Jung (2018) who use the most recent round of the IFLS and argue it is underpowered to estimate the effect of school construction on education.

4. Results

This section describes the impact of the INPRES school construction program on long-term and intergenerational outcomes. Following the estimation strategy outlined in the previous section, the main explanatory variable is an interaction of the number of schools constructed per 1,000 children in a person's birth district with an indicator variable for being young enough to have benefitted from the program. As briefly discussed in the introduction, Figure 1 reveals broad positive impacts of the school construction program across ten indexes that measure impact on individuals exposed to the program, and across two indexes that capture second generation effects on their children. In Tables 1 to 12, we present the family of outcomes that each of the 12 indexes is based on and we discuss these in more detail.

4.1. Impact on educational attainment

Table 1 studies the relationship between school construction and educational attainment.[24] On average, the program increases years of education for men by 0.27 years and for women by 0.23 years. At the mean number of schools built per 1,000 children (1.98), these estimates imply an increase in years of schooling of 0.53 and 0.46 for men and women, respectively. The analysis by Duflo (2001) is restricted to men, and the comparable point estimate in her study (0.19 years) is lower than ours. We can only speculate about the source of this difference, but both estimates are modest in size, given that the number of primary schools almost doubled.[25]

The next four rows break the education effects down by completed level of education and show considerable gender differences. For men, the program caused a 2.6 percentage point increase in the likelihood of having completed primary school. Even though the INPRES

[24] Educational outcomes are recorded for household members aged five and older, and are missing otherwise.
[25] Similar to Duflo (2001), we also estimate the impact on average years of education for the sample of wage earners and for all those employed. Results are broadly similar in magnitude and significance.

program targeted primary schools only, effects for men continue through lower and upper secondary education at 2.3 and 2.6 percentage points. These represent larger percentage increases than for primary school because the average completion rates for lower and higher secondary education are lower.

The results for women on the other hand are concentrated in primary school only, which they were 4.1 percentage points more likely to complete, and we are able to reject the equality of this coefficient with the male effect. The effects on lower and higher secondary completion rates are considerably smaller and indistinguishable from zero. For both men and women, the school construction program did not affect tertiary education completion rates. As shown in row 6, literacy rates are high on average at 95 percent for men and 91 percent for women, and the program raised these by 1.5 and 3.3 percentage points, respectively. The FDR q-values (in brackets in the table) that correct for multiple hypothesis testing across all of the outcomes in the education table show that the coefficients remain statistically significant.

The last row in Table 1 creates an index using all other rows combined, following Kling et al. (2007) as discussed in the previous section. The point estimates correspond with those shown in Figure 1 and confirm broad increases in education attainment for men and women. Building two additional schools in an individual's birth district would increase the educational outcomes for those exposed to the school construction by approximately 0.13 standard deviations relative to the control group.

The gender dynamics and patterns by grade are explored in further detail in Figure 3 showing the impact of school construction on the likelihood of completing at least a certain number of years of education. For example, it shows that the program increased the likelihood of completing at least one year of school by 0.95 percentage points for men and 2.3 percentage points for women. For both men and women, the effects are significantly different from zero throughout all primary school years and show an increasing pattern by grade, which explains the large effects on primary school completion rates. Consistent with Table 1, effects for men continue throughout lower and upper secondary school and seem fairly stable across grades. While positive, the effects for women are not distinguishable from zero, nor are the effects on tertiary education for either gender.[26]

4.2. Long-run labor market impacts

Having observed large increases in education in response to the INPRES school construction program, Table 2 studies subsequent labor market and migration outcomes.[27, 28] As shown in row 1, 95 percent of men are working and the school construction program raises this by 0.6 percentage points. The effect for women is half as large and insignificant, but allows for an economically meaningful increase within its confidence bounds, especially considering a lower average employment rate of 64 percent. Conditional on working, row 2 explores the intensive margin of employment, namely number of hours worked. Point estimates indicate increases of 0.26 hours for men and 0.16 for women, but neither are significant. In response to the school construction, men move to jobs that are generally deemed more desirable: they are 1.1 percentage points more likely to work in the formal sector that tends to offer higher quality and more stable jobs. Given an average formal sector employment rate of 33 percent for men,

[26] While the school construction program could be used as an instrument for years of education, we prefer to study later-life outcomes using OLS in order to capture broad impacts and because the exclusion restriction could be violated if the program caused community-level changes that affect long-term outcomes in ways other than through increased schooling. There is a strong first stage relationship with the F-statistic being 32.3 for men and 31.8 for women. That said, for scaling purposes, the coefficients on long-term outcomes can be multiplied by approximately four to calculate the effect of an extra year of education, given that the program increased years of schooling by approximately 0.25 years.

[27] Employment outcomes are recorded for household members aged ten and older, and are missing otherwise.

[28] Heckman, Humphries, and Veramendi (2018) provide a recent overview of the extensive literature examining the relationship between education and labor market outcomes. Duflo, Dupas, and Kremer (2017) is one of the few education sector randomized control trials that follows individuals over eight years and finds that secondary school scholarships improved labor market outcomes.

increasing the number of schools in an individual's birth district by the sample mean raises the likelihood of men being in the formal sector by almost 7 percent. They furthermore move away from agricultural work, which they are 1.2 percentage points less likely to hold, compared to 44 percent on average, and shift towards service sectors. We do not find any evidence of occupational shifts for women.[29]

There is a large literature on the relationship between education and migration that has generally focused on the selection into migration in terms of educational attainment.[30] However, little is known about the causal relationship between education and migration, in particular whether an exogenous shift in education leads to more or less migration. In our situation, on average, 27 percent of men and 25 percent of women have migrated away from their district of birth. The school construction program increases migration rates by 0.7 and 0.8 percentage points respectively, and at the mean level of school construction, this would represent an increase of 5.1 and 6.5 percent for men and women, respectively. Row 7 indicates that the increase in migration is concentrated in shorter distance moves within—rather than between—provinces. Correcting for multiple hypothesis testing across all of the outcomes in the work/migration table shows that the FDR q-values are somewhat larger but coefficients generally remain statistically significant. Finally, aggregating the seven outcomes in the work/migration table into an index following Kling et al. (2007) shows a positive and significant impact for men with an increase of 0.076 standard deviations due to an increase of two additional schools built in the district.

4.3. Long-term impacts on living standards, taxes, housing, and assets

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[29] The only occupation-related variable that shows up as statistically significant for women is whether they are self-employed in their own micro-enterprise, which almost a quarter of women are. They are 1.1 percentage points more likely to do so. Given the large number of outcomes variables, we decided not to report all subcategories separately, but instead combine them into the 'formal worker' variable, which is not statistically significant.

[30] Empirical evidence for Indonesia (Hicks et al., 2018) and for developing countries in general (Young, 2013) shows positive selection from rural to urban areas and negative selection from urban to rural.

Susenas 2016 collects detailed data on expenditure at the household level, which we use as a proxy for living standards.[31] Table 3 shows the effects of exposure to the school construction program on five aggregated living standard measures. Row 1 shows that households in which males are exposed experience a 2.1 percent increase in total expenditure and households in which females were exposed increase total expenditure by 3.2 percent, and we are able to reject the equality of these coefficients. The increase is larger for non-food expenditure than for food expenditure as shown in rows 2 and 3 and, as a result, the ratio of non-food to total increases (row 4). Households where the household head or spouse was exposed to the school construction program in the 1970s spend 16 to 19 percent more on education in 2016 (row 5). All results remain statistically significant even after correcting for multiple hypothesis testing. The last row in Table 3 combines the expenditure data from rows 1, 2, and 5 into a living standards index, showing an overall increase of 0.03 and 0.05 standard deviations for men and women, respectively, for each additional school built in an individual's birth district.[32]

In addition to increases in expenditure, we study whether tax payments increase. This is an important input for the cost-benefit analyses in Section 7 allowing us to study whether a program as large as the Sekolah Dasar INPRES program could pay for itself from increased tax payments over time. Table 4 shows broad increases in total tax payments and the three main tax

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[31] Susenas 2016 does not include information on income, unlike the 1995 Intercensal survey that Duflo (2001) used to measure the returns to education. After the 1995 round, the earnings question was discontinued so we do not have access to more recent income data. That said, Rizky, Suryadarma, and Suryahadi (2018) argue that expenditure is a better measure of living standards because income data tends to suffer from under-reporting in developing countries. All expenditure values refer to average monthly expenditure measured in 10,000 Indonesian rupiah (IDR). In 2016, the exchange rate was 1 USD=13,308 IDR. Expenditure categories that were reported in weekly or annual amounts are converted to monthly expenditure. In regression analyses, we apply an inverse hyperbolic sine transformation to the nominal values since consumption data tends to be skewed and a log transformation would not be defined for zero expenditures. The inverse hyperbolic sine is approximately equal to log (2y) or log (2) + log (y), so in most cases it can be interpreted the same way as a standard logarithmic dependent variable.

[32] The variables shown in rows 3 and 4 of Table 3 can be derived from those shown in rows 1 and 2, so to avoid double counting these are excluded from the living standards index.

payment sub-components that Susenas 2016 collects data on.[33] Total tax payments, shown in row 1, increase by 7.8 percent in households in which the man is exposed to the school construction program and by 12.3 percent if the woman was exposed, and we are able to statistically reject the equality of these coefficients. Total tax expenditures are comprised of a rich set of tax data that is analyzed in more detail in rows 2, 3, and 4, revealing increases in land and building taxes, taxes on motorized and non-motorized vehicles, and local community taxes.

Table 5 explores effects on housing and assets starting with the likelihood of living in an urban area. On average 43 percent of the sample lives in urban areas and even though exposure increased migration, especially over short distances, the school construction program does not increase the share of people living in urban areas. They do appear to move to more valuable and larger housing. Row 2 shows an increase of 2.8 percent in the monthly rent payments if the women is exposed to school construction, and a smaller and insignificant effect if males are exposed. If either is exposed, we observe increases in floor area of 1.2–1.5 square meters (row 3) and increases in utility usage of 5.1 to 8.5 percent (row 4). In order to approximate for household wealth, row 5 studies the impact of school construction on an asset index that is created as a principle components index over household ownership of all durable assets that the Susenas 2016 asked about.[34] The school construction program leads to a 3.0 percent increase if men are exposed and 4.0 percent increase if women are exposed in the household asset index. Aggregating all five housing and asset outcomes into an index following Kling et al. (2007) (row 6) confirms broad increases for men and women in response to school construction.

4.4. Long-run impacts on nutrition and health

[33] All tax payments refer to average monthly values measured in 10,000 Indonesian rupiah (IDR).

[34] Asset index is a PCA index of ownership of motorcycle, car, home phone, computer/laptop, television, gold/jewelry (≥ 10 g), refrigerator, water heater, LPG gas tube (≥ 5.5 kg), boat, motorized boat, and air conditioner.

There exists a strong correlation between more education and better health, although research estimating a causal relationship has found mixed evidence. Lleras-Muney (2005) finds positive effects of education on mortality in the U.S., while Clark and Royer (2013), Malamud, Mitrut, and Pop-Eleches (2018), and Meghir, Palme, and Simeonova (2018) find no effects of education on mortality in the U.K., Romania, and Sweden, respectively. Tables 6, 7, and 8 show three main components of health effects in response to the INPRES school construction program. Table 6 focuses on nutrition and finds increases in food intake, particularly for women exposed to the program. Overall calories increase by 1.8 percent for women while the effect for men is smaller and not significant. Patterns are similar for consumption of protein, fats, and carbohydrates with respective increases of 1.8, 2.3, and 1.7 percent when women are exposed to the program, and smaller increases for men, and we are able to reject the equality of coefficients in all cases. The data do not allow us to answer definitively (although Table 8 examines self-reported health outcomes) whether these changes in nutrition for women are health improving as additional protein is likely to be beneficial for individuals in developing countries, but additional fats can be indicative of a worsening diet.

Table 7 studies investments in health at the household level. Overall health expenditures appear to increase by 7.1 percent for exposed men and 5.5 percent for exposed women, but only the men's coefficient is marginally significant. The effects are particularly large for investments in preventative health, including medical check-ups, family planning, and immunizations, which increase by 24 percent if the father is exposed to school construction and 19 percent if the mother is exposed. Breaking this down further, we see large increases in expenditures related to family planning, including contraceptives and consultations, of 32 and 23 percent for exposed men and women, respectively. On the curative side, households with either the man or woman exposed are 4.8-7.5 percent more likely to use a private hospital instead of a public one, which generally provide higher quality and more expensive health care. Finally, row 5 shows an increase of 14 percent in health insurance expenditures if women are exposed to the program, and an 8 percent increase if men are exposed, but the latter cannot be distinguished statistically from zero. Taken together, the health investment index in row 6 shows broad increases for both men and women exposed, with an improvement in health investments of 0.13 standard deviations when an additional two schools are built in the individual's birth district.

A natural follow-up question is whether increases in nutrition and health investments result in improved health outcomes. Table 8 reveals that overall such improvements are not observed. While we see increases in not reporting a health complaint in the last month (0.4 and 0.3 percentage points for exposed men and women, respectively) and the number of days uninterrupted by health complaints (row 2), neither are statistically significant. Considering severe health complaints only, we observe a 0.5 percentage point decrease in reports from exposed men. The aggregated health index in row 4 shows an improvement in health outcomes for men exposed to school construction but is insignificant for exposed women.

4.5. Long-run marriage and fertility effects

Evidence estimating the causal relationship between education and demographic outcomes has generally been mixed and nuanced. Osili and Long (2008) find evidence of increased education reducing fertility in Nigeria. On the other hand, using U.S. data, McCrary and Royer (2011) find only a small fertility effect but a larger effect on the quality of the marriage partner. In the Kenyan context, education subsidies reduce women's likelihood of teenage marriage and pregnancy (Duflo, Dupas, and Kremer, 2015). Looking at a larger age range of women, Geruso and Royer (2018) find increased education lowered teen fertility and increased the education of the spouse, but had no impact on total completed fertility.

In our setting, we also find nuanced evidence of the impacts of exposure to the school construction program. Table 9 explores marriage and fertility outcomes for those exposed to the program in the 1970s. In general, women marry on average almost five years younger than men (row 1), but there is no effect of exposure to the school construction program on the age of first marriage. Coefficients are small and statistically insignificant. On the other hand, we do observe improvements in marriage partners, with spouses having more years of schooling. Program exposure for men raises their spouse's years of schooling by 0.18 years, while program exposure for women raises their spouse's years of education by 0.12 years. Note that there is an overall increase in years of education attained in communities exposed to the program, so the increase in the level of education of a person's spouse may be due to improved selection on the marriage market and/or an overall increase in the level of education in the local marriage market. We also observe a 1 percentage point increase in the likelihood that the women's spouse is still alive in 2016, which may be indicative of improved health of the spouse. We do not have complete birth histories for each women that would allow us to measure the relationship between increased education and fertility. However, we are able to test if there is a change in the number of children aged 0–14 living in the household at the time of the survey in 2016. Exposure to school construction reduces the number of children for women. All of these results remain statistically significant after correcting for multiple hypothesis testing. The marriage market index that aggregates these four outcomes shows that for women there is a significant improvement in her marriage market if she is exposed to the school construction program. Having an additional two schools built in her home district raises this index by 0.10 standard deviations.

4.6. Long-term impacts on welfare program utilization

Lastly, we explore the effects of exposure to school construction on the first generation's utilization of government welfare programs. Susenas 2016 collects data on four countrywide programs that aim to reduce poverty and inequality. Ex-ante it is unclear if increased take-up of welfare programs reflects higher needs due to increased poverty, or whether it is indicative of increased awareness of existing programs. Table 10 reveals few changes in response to the INPRES school construction program, and this is confirmed by the last row that combines the four welfare program outcomes into a welfare program index.

5. Second generation effects of school construction

Having observed large long-term effects of Indonesia's school construction program on a wide range of outcomes, including education, employment, migration, and living standards, we now investigate whether the effects extend to the next generation and affect the children of those parents who were exposed to the program. As explained in Section 3, second generation impacts are measured using the same difference-in-differences framework as first generation effects. The main explanatory variable is an interaction of the intensity of school construction in a parent's birth district with an indicator of whether the parent was young enough to have benefitted from the program. Outcomes of all children living in the parent's household are considered and age fixed effects are included to ensure comparisons take place across children of the same age.

5.1. Second generation effects on education and wellbeing

Table 11 shows the effect of parental exposure to the school construction program on the education attainment of their children. Row 1 confirms that the effects of the school construction program persist into the next generation.[35] Children whose fathers were exposed to the program obtain an additional 0.10 years of education, while children whose mothers were exposed obtain 0.17 years more. We are able to reject the equality of these coefficients. The magnitudes have decreased compared to the first generation results of 0.27 years for men and 0.23 years for women, but are still economically meaningful. In the next sub-section, we explore potential channels through which these effects persist into the next generation.

Unlike the first generation education results, no effects are observed on children for primary school completion rates (row 2) because primary school by 2016 has become almost universal.[36] There are large effects on completing lower and upper secondary for children whose parents were exposed to the school construction program, with the effect for exposed mothers being statistically larger than for exposed fathers. Also, unlike the first generation education results, increases in educational attainment now extend to tertiary education completion rates. Children whose mothers were exposed are 0.8 percentage points more likely to have completed tertiary education, compared to a 0.4 percentage point increase for children whose fathers were exposed. In terms of effect sizes, an increase of the mean number of schools in a mother's birth district would lead to a 25 percent increase in the likelihood her child completes tertiary education, relative to average tertiary education levels.

To account for the fact some second generation children may still be attending school, we study the effects on age-for-grade (row 6), loosely defined as an indicator variable for whether the child is on track to complete the appropriate grades on time.[37] Results confirm that having

[35] Related research explores the production function for children's human capital (Behrman and Rosenzweig, 2002; Attanasio, Meghir, and Nix, 2017) as well as focuses on how parents or teachers respond to inequalities across children (Akresh et al., 2012, Pop-Eleches and Urquiola, 2013).
[36] UNICEF data indicate that net enrollment rates in primary education in Indonesia from 2008-2012 were 100 and 98 percent for boys and girls, respectively.
[37] More specifically, the indictor variable is zero for those who have not yet started primary school by age 7 as well as for those who had to repeat one or more grades before completing upper secondary education, which is

parents exposed to school construction in the 1970s increases the likelihood of being on track by 1.1 percentage points if the father is exposed and 1.8 percentage points if the mother is exposed.

All of the education results for mothers remain statistically significant after correcting for multiple hypothesis testing, while lower secondary and tertiary completion rates for children when the father is exposed are not statistically significant. We aggregate the six outcomes into a second generation education index, and it shows broad increases for children when their father or mother was exposed to the school construction. An increase of two additional schools built in the father or mother's birth district increases their children's educational attainment by 0.06 or 0.11 standard deviations, respectively, relative to parents who were not exposed to the program.

Having observed broad increases in educational attainment for the children whose parents were exposed to the INPRES school construction program, in Table 12, we explore effects on the children's general well-being. Despite having limited information on these second generation children, we are able to explore employment and self-reported health outcomes. Rows 1 and 2 examine their likelihood of being employed. We consider it welfare improving for children not to be engaged in employment so we define the employment-related variables as the number of days and hours they are not engaged in work. For a child whose father was exposed to school construction, we see a slight reduction in the days and hours worked, but for a child whose mother was exposed the effects are indistinguishable from zero. However, none of these results remain statistically significant after correcting for multiple hypothesis testing. Rows 3, 4, and 5 study second generation health effects. Children whose mothers were exposed to the school construction show no effects on their health indicators. On the other hand, children whose fathers were exposed appear to self-report worse health outcomes. We are unable to determine if these

compulsory in Indonesia. The indicator variable is one for those who are on track to complete upper secondary education in a timely manner and for those who have already completed upper secondary education.

children are actually less healthy or whether their better educated parents have an understanding of health that makes them more likely to report their child as ill. Aggregating these employment and health indicators into a second generation wellbeing index shows no effect for children whose fathers were exposed but does show a positive impact if the child's mother was exposed.

We next explore two dimensions of heterogeneity in the second generation education results. First, we examine if school construction had different second generation effects at different grade levels. Second, we examine, within a household, if paternal or maternal exposure to school construction had differential impacts on their children and if those impacts differed by whether the child was a son or daughter.

In Figure 4, we estimate the likelihood of a second generation child completing at least a certain number of years of school. We explore the effects depending on whether the father or mother was exposed to school construction and whether their child is a son or daughter. Results highlight that effects are small and indistinguishable from zero during primary school. Consistent with Table 11, for all other grades, exposure to school construction by mothers has a larger effect than fathers on their children's education. For grades in lower secondary, upper secondary, and tertiary, we observe effects that are significantly different from zero for daughters when either their mother or father was exposed to school construction. Effect sizes for second generation daughters are approximately of the same magnitude as those of the first generation's men exposed to the program (see Figure 3 for this comparison). Effect sizes are largest for daughters when the mother was exposed to the program and lowest for sons when the father was exposed. While we cannot statistically distinguish the results by gender of the parent or child when examining each grade separately, in Table 13 we investigate this issue in more detail.

Table 13 examines if the impact of parental exposure within a given household varies when controlling for the partner's exposure and if those impacts differ by whether the child was a son or daughter. We face several challenges in this situation if we want to strictly follow our identification strategy of focusing on young (born 1968-1972) and old (born 1957-1962) cohorts. For a household to be included in the regression, we need both the father and the mother to be in these specific birth cohorts.[38] Given this selected sample in which both parents are in either the young or old cohort, we observe that for these households the impact of mother's exposure to the school construction program has a much larger effect on the child than the father's exposure. An additional school built in the mother's birth district raises her child's education by 0.16 years of school, while there is no effect if the father is exposed.

In column 2, we attempt to address this selection issue by expanding the range of birth cohorts that are included in the regressions. We now include all individuals who were born between 1950 and 1980. All birth cohorts born 1968-1980 could be exposed to the school construction that began in 1973. This will address the sample selection issue as both parents no longer need to be part of the young and old cohorts as previously defined. The sample size expands to 246,466 second generation children with parents in this extended birth cohort range. Results are consistent, with mother's exposure to school construction increasing her child's education more than the father's exposure. We are able to reject the equality of coefficients in both the restricted (column 1) and the extended birth cohort samples (column 2).

[38] For instance, if the mother was born in 1968, but the father was born in 1965, then that household would be excluded from the regressions because the father is in neither the young nor old cohort. There are 120,838 children in the regressions in Table 11 exploring the impact of father's exposure on second generation years of schooling. However, with this additional sample restriction that the mother must also be in the young or old cohort, there are now only 44,105 children, a loss of almost two-thirds of the sample.

We are further able to explore whether parental exposure has a different effect for sons and daughters. Panel A, as previously discussed, focuses on all children. In Panel B, we restrict the analysis to sons only and in Panel C to daughters only. Results for sons and daughters show consistently larger education effects for the second generation child if the mother was exposed to school construction, and in all cases, mother exposure is statistically significant. Focusing on the extended cohort sample (column 2), we can reject the equality of the mother and father exposure coefficients in the case of daughters but not sons. The benefit to daughters is three times larger if their mother was exposed to the INPRES school construction program rather than their father.

5.2. Channels for intergenerational persistence of education

To gain insight into the mechanisms through which parents' exposure to school construction affects their children's education, we perform a mediation analysis shown in Appendix Table A.1. Column 1 repeats the effects of parent's exposure to school construction on the child's years of education and subsequent columns add the indexes shown in Figure 1 as control variables that may function as mediators through which parental exposure manifests itself.[39]

Column 2 shows that adding the work and migration index leads to a 15 percent reduction of the effect of father's exposure, which is a substantial decrease, but the effect remains large at 0.082 additional years of education for his children. Controlling for the work and migration index in the analyses of mother's exposure barely affects the coefficient. This is not surprising since we found few labor market effects for women in response to school construction. A larger drop of 23 percent occurs for women when we control for the living standards index (column 3), which is in line with Section 4.3 that shows large increases in expenditure for women exposed to school construction. Despite this drop, children are still

[39] Note that the column headings show which index is included as an explanatory variable. The dependent variable for all columns is second generation's years of schooling.

getting an additional 0.13 years of education even if we hold expenditure constant. The comparable point estimate for father's exposure is 0.082 years. Exploring if there are other variables that may mediate the direct effect of parent exposure on their children's schooling, we control for the index of taxes and housing/assets in columns 4 and 5 respectively. The point estimates remain the same as when we control for living standards.

In columns 6 and 7, we explore whether controlling for increased nutrition and health investments reduces the estimated effect of program exposure on second generation's schooling. We do not find support for this as the point estimates for fathers and mothers are similar to column 1 without any mediators as control variables. We take this as suggestive evidence that increased health investment and nutrition are not relevant channels through which children of those exposed to school construction gain additional education. Similarly, when controlling for reported health outcomes in column 8, no mediating effect appears, which is expected given the small direct effect of exposure on health outcomes. Finally, controlling for the marriage index in column 9, which includes spouse characteristics and household size, leads to a reduction of 11-15 percent in the effect of exposure.

In a final attempt to explore whether mediating variables can serve as channels through which parents' exposure affects their children's education, we include all indexes from columns 2 to 9 as control variables. This leads to a 29 and 34 percent decrease in the direct effect of father's and mother's exposure, respectively. The school exposure effects remain large and significant at 0.069 additional years of schooling if fathers are exposed and 0.111 if mothers are exposed. Holding constant many of the variables that were effected by school construction, there remains a direct effect of parents' exposure to school construction on their children's education. There are many channels through which these effects could manifest themselves, for example through increased encouragement to go to school or help with homework. We cannot distinguish between these channels, but based on this table, we conclude that there remains a direct effect from parents to their children that is not explained by the variables we observe in the data.

5.3 Selection of second generation individuals

There are two issues about the survey data structure that are relevant for our second generation analysis and the selection of which individuals are in the regression samples. First, there is a tradeoff between the selection of which individuals remain in the household, and are therefore in our survey, and what age they would need to be to finish different levels of school. Focusing only on younger children ages 0-15 reduces the selection bias as few of them leave the household by that age. However, those young children are not old enough to have completed higher levels of schooling (lower secondary, upper secondary, or tertiary), which are important to include given that average years of schooling has increased since 1973 and primary school is almost universal in 2016. As we increase the age range to focus on older children, they have had time to complete higher levels of schooling, but a larger percentage of them have left the household. In all second generation analyses thus far, we include all children who still live with their parents, regardless of their age. We do, of course, include child age fixed effects. The rationale of not imposing any age restrictions is illustrated in Appendix Figure A.2. In the top panel, this figure shows the coefficient of the school construction exposure on years of education of second generation children when we limit our analyses to individuals under a certain age, and on the x-axis we vary the upper-bound to the ages included. Given that lower levels of education are near universal by 2016, it is not surprising we do not find an effect if we only look at children age 0-15. As we move to higher age limits, the increase in sample size is shown on the bottom panel, and the effect size increases as individuals are given sufficient time to complete their education.

Second, Susenas 2016, like most household surveys, only includes information on individuals who currently reside in a given household but not on family members living in different households. Therefore, in our case, for second generation children who are no longer living with their parents, perhaps because they started a new household, we cannot link them to their biological parents and we do not know whether the parents of these children were exposed to the program. For this reason, all second generation analyses thus far are based on those still living with their parents. We next explore the robustness of the results under various assumptions about the children who have left the household.

To do this, we conduct three main types of bounding analyses. First, we estimate extreme bounds in which all non-co-resident children are assumed to have parents who were either exposed or not exposed (Manski, 1990). The intuition behind the extreme bounds analysis is to re-assign individuals living apart from their parents back into the sample.[40] In Appendix Table A.2, we compare our baseline estimate for the second generation's years of schooling (Table 11, row 1) to the first bounding strategy. Including all individuals in the data under age 40 in the second generation's education regressions, we increase the sample size from 120,838 and 105,523 in the father's and mother's regressions to 644,675 observations. In these bounding exercises, we maintain the exposure status of children who still live with their parents. In regressions measuring the effect of a father's exposure to school construction on his child's years

[40] For children who live apart from their parents, we need to assume their parent's birth district and birth year in order to determine the parent's exposure status. Because we have no other information, the best assumption for the parent's birth district is to assume that it is the same as the child's. To test the robustness of this assumption, we estimate the regressions in Table 11 using the child's birth district instead of the parent's birth district to measure exposure, and results are consistent. In the main regressions, we include birth year fixed effects, but it is harder to predict parent's birth year given only a child's age, so we instead include an indicator variable for whether the parent is in the young cohort. Estimating the regressions in Table 11 replacing birth year dummies with a young cohort dummy yield consistent results. Further, to minimize the probability of including individuals who are unlikely to be children of a parent in our young or old birth cohorts, we impose an upper age restriction of 40 because that would imply parents in the old cohort who were 14-19 years old at the time of birth.

of schooling, results are no longer statistically significant with these extreme bounds in which we assume all non-co-resident children are born to non-exposed fathers (column 2) and then all non-co-resident children are born to exposed fathers (column 3). However, the education effects for second generation children whose mothers were exposed to school construction remain statistically significant despite these extreme assumptions. Results when we assume all non-co-resident children are born to mothers who were not exposed to school construction show that these children obtain an additional 0.05 years of school. On the other extreme, when we assume all non-co-resident children are born to mothers who were exposed to school construction, results show that children still obtain an additional 0.03 years of school.[41]

Aside from the extreme assumption that parents of non-co-resident children are either all exposed or all not exposed, we are also likely adding too many individuals to the regression. Our second bounding exercise attempts to address these issues. The bounding regressions for second generation children should not include children born to parents who are not in the old (born 1957-1962) or young (born 1968-1972) cohorts. To improve our bounds in these two dimensions, we use the Indonesia Family Life Survey (IFLS) data that does notably well in tracking individuals over time and matching parents to children who remain at home and who have moved away.[42] We use these data to obtain the fraction of children at each age who are

[41] Note that the effects using these extreme bounds are smaller than our estimates reported in Table 11. This is to be expected if parental exposure to school construction leads to an increase in their children's years of schooling. The reason for this is that the children we add to our sample are a combination of children whose parents were exposed and whose parents were unexposed. So in the case we assume all parents were exposed, part of these children actually had non-exposed parents and thus no increased educational attainment due to their parent's exposure to school construction, but we incorrectly assign them to the group of exposed parents, which biases the estimates downwards. Similarly, if we assume all parents were unexposed, part of these children actually had exposed parents so increased educational attainment, but we incorrectly assign them to the group of unexposed parents, which again leads to a downward bias of the estimates.

[42] 87.8 percent of individuals surveyed in the first wave (1993) were tracked or confirmed dead in the fifth and last wave (2014/2015). We match 91 percent of children in the last wave's household roster to their co-resident or non-co-resident parents. Non-co-resident parents who never completed a detailed individual survey in IFLS were not asked for their birthplace, so we assume their birthplace is the same as their child's.

born to old and young cohort parents among all children no longer living with their parents. We then use these IFLS-based fractions to randomly assign at each age non-co-resident children in the Susenas data to either old or young cohort parents and to exclude the rest from the regression.[43] We then simulate this randomization assignment procedure 1,000 times and estimate the second generation years of schooling regression.

Appendix Figure A.3 shows the distribution of coefficients from these 1,000 repetitions for father's and mother's exposure to the school construction. The effect sizes for father's exposure on their children's years of schooling range from 0.011 to 0.047 (at the 5th and 95th percentiles) with a median coefficient of 0.028. The effect sizes for mother's exposure are larger, ranging between 0.018 and 0.065 with a median coefficient of 0.043.[44]

Third, we repeat the second generation analysis directly using the IFLS itself. The last three columns in Appendix Table A.2 show estimates for all individuals (column 4), for children who live with their parents ("Stayers", column 5), and for children who have moved away from their parents' household ("Movers", column 6). The IFLS sample of stayers provides us with the closest comparison to our Susenas sample of stayers. Across the sample of stayers, movers, and all second generation children, we find no statistically significant effect for fathers exposed to the school construction. However, the estimated effect of mother's exposure is 0.538 in the sample of children still living with their parents compared to 0.196 in the sample of all children. This suggests an effect only 36 percent as large if we are unable to include non-co-resident

[43] Results are also consistent if we use the IFLS to obtain the fraction of children at each age and gender who are born to old and young cohort parents among all children no longer living with their parents and then use these age-gender IFLS-based fractions to draw random samples in the Susenas data.

[44] Coefficients for father's exposure are statistically significant at the 10, 5, and 1 percent levels in 47, 27, and 5 percent of the regressions, respectively. Coefficients for mother's exposure are more likely to be statistically significant. We observe that in 63, 42, and 13 percent of the regressions the coefficients are statistically significant at the 10, 5, and 1 percent levels, respectively.

children in the analysis. Scaling down our estimates of mother's exposure in column 1 by this magnitude would yield an estimated effect of 0.062 additional years of school.[45]

6. Threats to identification and robustness checks

6.1 Possible general equilibrium effects

The analysis presented so far has exploited variation across geographic regions and birth cohorts to identify the "partial equilibrium" effects of the school construction program. This raises the concern that "general equilibrium" effects might undo the direct effect of the program (Heckman, Lochner, and Taber, 1998). The concern in our specific situation is that the school construction program increased the education levels of the young cohort in the high intensity regions, and this increase in educated young cohorts could have affected individuals who were not exposed to the school construction (either the older cohorts or the young cohorts in the low intensity regions). Depending on how these general equilibrium effects worked, they could potentially bias our results leading to either an over- or under-estimate of the true effect. Whether the general equilibrium effects have a negative or positive effect depends on the substitutability or complementarity between the old and young cohorts.

School construction led to many more educated, young workers. If those young workers are substitutes for the older cohorts, then this increase in educated young workers could have driven down the wages for the older cohorts who were competing with them for jobs in those locations. If that happened, then the effects we observe for improved living standards for the young cohort relative to the older cohort might be biased. In our difference-in-differences specification, if school construction negatively affected the older cohorts, we would be over-estimating the true effect of the program. Duflo (2004) provides some evidence that these

[45] Note that this is in line with the range of coefficient values shown in Appendix Figure A.3

general equilibrium effects might have occurred in the Indonesian context, although the magnitudes of the bias appear to be rather small. Focusing on the instrumental variables specification that she estimates, she finds that an increase of 10 percentage points in the share of primary school graduates would lead to a decrease of 2.9-3.8 percent in the wages for the old cohorts. Given we observe in our data an increase of only 2.6 percentage points in the likelihood of completing primary school for men, the subsequent old cohort wage decreases would be less than 1 percent. Adjusting our estimates by that magnitude does not significantly alter our results. In addition, if we adjust by this magnitude the cost-benefit calculations discussed in Section 7, it would not affect our overall interpretation of the benefits of the school construction program.[46]

Alternatively, if the young cohorts were complements for the older cohorts (so for instance, they start more businesses and hire older cohort individuals or they spend more money on goods and services produced by the older cohorts), then in this case the older cohorts actually benefit by having more educated younger cohorts in their location. Therefore, if the general equilibrium effects act in this way, we would be underestimating the true effect.

Unfortunately, the data we have does not allow us to distinguish between these competing stories of complementarity and substitutability among older and younger cohorts. Furthermore, the evidence on this question of the general equilibrium effects from developed countries (Angrist, 1995; Crepon et al., 2013; Bianchi, 2018) is unlikely to be helpful in understanding the developing country, Indonesian context over the past four decades. In our case, we can show that there are not systematic trend breaks when comparing the older cohort

[46] Recent research focusing on large-scale government investments in education in India finds that the general equilibrium effects could be much larger, with these effects working to depress the returns to education by 32% (Khanna, 2018). However, the analysis of the Indian policy highlights that skilled workers are worse off while unskilled workers are better off. In our Indonesian context, this evidence about the unskilled workers being better off would imply that the older cohorts who were more likely to be lower educated and unskilled would have benefited from these general equilibrium effects, thus providing some suggestive evidence that our difference-in-differences specification might underestimate the true effect.

with an even older cohort. While this is certainly not definitive, the results for household expenditures in Appendix Figure A.4 highlights that there is not a differential trend when comparing the old cohort (ages 12-17 in 1974) with an even older cohort (ages 18-24 in 1974). If general equilibrium effects were negatively impacting the older cohorts we would expect to see the oldest cohorts to be the worst off and that is not something we observe.

6.2 Robustness checks

In this sub-section, we present a set of specification checks highlighting the robustness of the main results. For all of the results presented so far, we have exploited the variation across birth cohorts and geographical regions in the number of schools built. The identification assumption is that the change in outcomes across birth cohorts in the regions that built many schools (high intensity) would have been the same as the change across birth cohorts in the regions that did not build many schools (low intensity). However, the educational patterns between birth cohorts could vary systematically across regions because of issues such as mean reversion. To test this assumption, we estimate placebo regressions in which we compare old cohorts (ages 12-17 in 1974) and even older cohorts (ages 18-24 in 1974). If the assumption is correct, then any change in outcomes between cohorts in these groups, both of whom were not exposed to the program, should not differ across geographic regions. Appendix Figure A.5 presents the results from estimating placebo regressions for each of the indexes for every family of outcomes (similar to Figure 1). In these regressions, we now compare individuals from an old cohort born between 1957 and 1962 and an even older cohort of individuals born between 1950 and 1956. Across all of the first generation outcomes for both females and males, the placebo regressions show no statistically significant effects.[47] This is suggestive evidence that the main difference-in-differences results are not driven by a failure of the identification assumption to hold.

In the main results, we define school exposure in an extremely conservative way. Individuals born between 1968 and 1972 (young cohort) would have been 2-6 years old in 1974 when the schools were built and would have benefited from full exposure to the program. Those born between 1957 and 1962 (old cohort) would have been 12-17 years old in 1974 when the schools were built and were too old to benefit from the construction of a primary school in their location. This is also the approach and cohort definitions used by Duflo (2001). However, there are other birth cohorts, both exposed and not exposed, that could be included in the analysis.

Appendix Table A.3 re-estimates the years of schooling education regressions discussed in Table 1 but now examines how the results change with alternative birth cohort definitions. Column 1 repeats the results defining the sample as individuals born in 1957-1962 (old cohort) or 1968-1972 (young cohort) as in Table 1, row 1. Columns 2-5 start with that baseline sample and then include additional birth cohorts in the regressions. The sample in column 2 adds in additional older cohorts born between 1950 and 1956 (and who were not exposed to the school construction). Column 3 adds in the individuals born between 1963 and 1967. These individuals would have been primary-school aged in 1974 (ages 7-11) when the schools were built. To be conservative, we assume that all of these cohorts were not exposed to the school construction, although in Appendix Figure A.1, it appears that some of the younger children ages 7-8 probably did benefit from the program. Column 4 extends the baseline sample by including children born

[47] We do observe a statistically significant effect in the placebo regressions for the second generation education index. This implies that there may have been a time trend across regions that could have influenced the educational outcomes for second generation children. However, if children whose parents born 1957-1962 are experiencing more education compared with children whose parents are born 1950-1956, then that likely means we are underestimating the true effect.

between 1973 and 1980. These children were born during and just after the schools were built and so they would have received full exposure to the program.[48] Finally, in column 5, we include all individuals born between 1950 and 1980. Results using these different sample definitions are consistent, showing that exposure to the school construction increased years of schooling for both men and women. In Appendix Figure A.6, we use the extended cohort definition (all individuals born between 1950 and 1980) and re-estimate the effect of school construction on indexes for families of outcomes (as we did in Figure 1). Results are consistent, showing large positive benefits for men and women who were exposed to the school construction.

All of the previous regressions using expenditure data as the outcome are estimated using an inverse hyperbolic sine transformation for the nominal values. While this is typically how expenditure data are analyzed, in Appendix Table A.4, we present robustness checks using alternative transformations for the expenditure data. The first four columns focus on total expenditures while the next four columns focus only on education expenditures. For a comparison with earlier results, columns 1 and 5 present the previous results from Table 3 rows 1 and 5 using the inverse hyperbolic sine transformation. We then present three alternative ways to estimate these regressions. Column 2 presents results from a log transformation of the nominal expenditure data and results are consistent in terms of magnitude and statistical significance. Column 3 presents the results using the nominal expenditure data and results are similar. Finally, in column 4, we estimate household per capita expenditures instead of total expenditures (again using the inverse hyperbolic sine transformation). This allows us to also capture potential changes in household structure that could be correlated with exposure to school construction.

[48] These Indonesian primary schools were initially expected to last for 20 years so the last cohort that could have gained the full six years of primary school education and completed their primary schooling by 1993 would have been born in 1980. Note that with these 1973-1980 cohorts it is possible that parents could have moved in order to give their children access to the schools, although the results appear to be consistent with the earlier ones.

The effect size is slightly smaller but the story remains the same that male and female exposure to school construction increases household expenditures (both total and per capita). Results for education expenditures (columns 5-8) shows that using a log transformation (column 6) or nominal values (column 7) would lead to different results than the inverse hyperbolic sine (column 5). This is predominantly due to the large number of zeroes for education expenditures and because the education data tends to be heavily skewed.[49]

Finally, we re-estimate the main results presented in Figure 1 measuring the effect of school construction on indexes of long-run outcomes using alternative control variables. Appendix Figure A.7 presents these new results in which we now exclude the interaction of birth year dummies and water and sanitation programs from the control variables. The magnitudes and levels of statistical significance are not significantly altered in this case.

7. Rate of return and fiscal impacts of school construction

Regression results highlight the many beneficial impacts for individuals exposed to the school construction program and for the intergenerational transmission of those benefits. In this section, we formally conduct a cost-benefit analysis to evaluate whether the school construction program was cost efficient for the Indonesian government.[50] Most cost-benefit analyses compare a program's costs with the overall welfare benefits of that program for the entire affected population, in effect asking if the economy would benefit from improved living standards. We are able to do that in our case as well. However, what is exceptional in our situation is that because the school construction program had a direct effect on increasing tax revenues collected

[49] Over 20,000 observations are dropped in the regressions using a log transformation (column 5).
[50] Appendix C discusses in more detail the assumptions made in our cost-benefit analysis and the specific parameters we include in the model and then tests the robustness of the results to alternative assumptions.

by the government, we are also able to measure whether these increases in government taxes collected offset the government's costs of building the schools.

We start by first measuring the costs of the school construction. The total costs include the initial investment to build the schools and train the teachers plus the recurring commitments to pay the teachers' salaries each year. The key point for the costs is that there were large and upfront costs at the beginning of the school construction program in 1973 and then subsequent smaller, but annual costs every year for the teacher salaries. The school construction cost approximately 782 million 2016 US dollars or around 1.5 percent of the Indonesian GDP in 1973 (Duflo, 2001). Schools were expected to recruit three teachers and to accommodate 120 students. Using survey estimates by Daroesman (1971), training three teachers across 61,800 schools would have cost the government 11.7 million in 2016 dollars.[51]

In our cost-benefit analysis, we will focus on two main benefit outcomes. The first are taxes paid directly to the government. We have information on taxes each household paid directly, and we have information on total household expenditures that we can use to estimate the 10 percent Value-Added-Tax (VAT) that the government would have collected on those purchases. The second main benefit is improvements in the first generation's overall living standards.[52] The key issue for the benefits side is that the government or the individual earns the

[51] On the cost side of the ledger, there are a number of parameters that are relevant in our model and all of them can be adjusted to see how the cost-benefit calculations respond. These parameters include: the discount rate, the number of years the school is expected to last, the number of teachers per school, the number of students per school, whether there is real growth in the teacher's salary, and the level of recurrent school administrative costs in addition to the teacher's salaries.

[52] With additional assumptions in the model, it would also be possible to measure the benefits accruing to the next generation. The regression results indicate that those individuals receive more education due to the school construction program, and presumably later in their lifetimes, they will subsequently pay more taxes and have higher living standards. Including these benefits in the model would further increase the benefit side of the ledger.

benefits each year and they accrue over many years, but these benefits do not start until long after the schools are built.[53]

We set up a cost-benefit accounting model to take all of these costs and subsequent benefits into account in the specific years they would have been realized and then trace out the arc of when the discounted benefits would offset the discounted costs. Table 14 summarizes the results and highlights how different assumptions about the relevant parameters influence the level of costs and benefits, the internal rate of return, and impact the breakeven year for the program when benefits first outweigh the costs. Column 1 starts with a less conservative approach.[54] Using these baseline values of parameters yields a total cost of school construction (school building, initial teacher training, and recurrent teacher salaries) of 2.55 billion in 2016 USD and a total tax benefit (direct taxes paid plus VAT taxes collected) of 9.00 billion in 2016 USD. This gives a project net benefit of 6.56 billion, a breakeven year in 1998, and an internal rate of return of 10.48 percent. Moving beyond government tax receipts and focusing on the program's impact on improving living standards substantially raises the level of net benefits to 59.24 billion with an internal rate of return of 20.68 percent.

From this initial set of parameters in column 1, we then modify parameters and trace out how those changes impact costs and benefits. Column 2 introduces real salary growth for teachers into the model and subsequently costs are higher and net benefits slightly smaller. Column 3 adjusts for the lifetime curvature in an individual's tax payments and the fact they tend

[53] As on the cost side, there are a number of parameters that are relevant for measuring the benefits in our model and all of them can be adjusted to see how the calculations respond. These benefit-side parameters include: the discount rate, the number of years the school is expected to last, the age individuals start paying taxes, an individual's life expectancy, the Indonesian economy's GDP growth rate, and the overall lifetime curvature in average taxes paid at each age across an individual's lifetime.

[54] In this column, we assume a 5.0 percent discount rate, no real growth in teachers' salary, no adjustment for the lifetime curvature of an individual's earnings (and subsequent taxes), no real growth in GDP per capita, 120 students per classroom, schools last 20 years, individuals start paying taxes after age 18, school administration costs 1.25 times the teachers' salaries, 3 teachers per school, and an individual's life expectancy is 60.

to peak around ages 40-50. Subsequently, the observed tax and living standards benefits are smaller. Column 4 now adds an adjustment for real GDP per capita growth in the economy of 3.25 percent. Taxes and living standards are measured in 2016, but there would have been real growth in those measures in the years prior to 2016 and this real growth rate adjustment takes that into account and further reduces net benefits. Column 5 represents what we believe to be a reasonable baseline case. In this scenario, all of the previous parameter values are maintained, and we increase the average number of students per school from 120 (20 per grade) to 180 (30 per grade), which is closer to what actually happened in these schools after they were built. Tax benefits are higher than costs with an internal rate of return of 8.10 percent, while living standards are substantially larger than costs with an internal rate of return of 16.84 percent.

Column 6 extends the school lifetime to 40 years. Original government plans in 1973 called for schools to last 20 years, but since most schools are still operating today, this seems like a reasonable assumption to test. Benefits increase substantially because there are more cohorts exposed to the program, but at the same time there are more years that teacher salaries are being paid so the cost side also increases. Net benefits are higher, but the internal rate of return only increases slightly because of the timing of when the additional costs are incurred. Column 7 increases the age after which individuals start to pay taxes from 18 to 22, while column 8 raises the recurrent cost multiplier from 1.25 to 1.5. Both changes have minor impacts on the net benefits observed. Column 9 further adjusts the number of teachers per school from 3 to 6 and this substantially raises the cost side of the ledger. Lastly, column 10 adjusts the life expectancy, which was increasing significantly over this period. Both tax and living standards benefits substantially outweigh costs.[55]

[55] Starting with the parameters from column 10, in order to observe a situation in which the net tax benefits do not outweigh the costs, it would be necessary to adjust those parameters so that recurrent costs must be greater than 1.9,

Appendix Figure A.8 graphs the discounted net tax and living standards benefits over time and highlights the breakeven years for when tax receipts and living standards benefits outweigh school construction program costs. Using the parameters adopted in columns 5 and 10 from Table 14, we show two highly realistic scenarios that the government would have faced. Overall net tax benefits are not that different across the two scenarios (5.42 and 7.76 billion), but the breakeven point in the scenario with more teachers, higher recurrent costs, and a longer school lifetime is much later. The net overall benefit to improved living standards is also much higher the longer the schools last (133.5 billion) and the improvement to the population's welfare offsets the program costs by 2003.

Across a range of different parameter estimates, school construction leads to increased government tax revenues that will offset school construction costs in most cases within 40 years. Even larger net benefits are observed when we include the population's improved living standards with net benefits ranging from 40 to 136 billion USD. Internal rates of return range from 13-21 percent and benefits outweigh costs within 17-30 years after the schools are built.

8. Conclusion

This paper studies the long-term and intergenerational effects of one of the largest school construction programs in history. We use a difference-in-differences estimation strategy exploiting variation across birth cohorts and regions in the number of schools built. We combine this with nationally representative data from Indonesia that contain information on a wide range of outcomes related to education, employment, migration, living standards, taxes, and marriage outcomes. We find that men and women exposed to the program attain more education, with

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the number of students must be less than 145, or the discount rate must be larger than 5.7 percent. However, net benefits from living standards would still remain positive until the following more drastic parameter adjustments are made: discount rate larger than 12 percent, or increases in recurrent costs to 2 plus reductions in number of students per school to 120 plus an increase in the discount rate to 10 percent.

men's education effects continuing beyond primary school. As adults, men exposed to school construction are more likely to be formal workers and work in a non-agricultural sector. Both men and women exposed to the program are more likely to have migrated from their birth district, although evidence points to increases in local migration within the province. Households in which either parent was exposed to school construction have higher living standards, more assets, and pay more government taxes. Exposure to school construction substantially alters marriage market outcomes with spouses being more educated and more likely to have migrated.

These benefits are transmitted to the next generation. Children with fathers or mothers who were exposed to the school construction program obtain more education. Significant effects are observed at all levels of schooling beyond primary school, but the largest impacts are seen in tertiary education with effect sizes indicating a 20 to 25 percent increase in the likelihood of the second generation child completing university. These second generation effects are significantly larger if the mother, as opposed to the father, was exposed to the program, with additional benefits accruing to daughters. We perform a detailed mediation analysis to explore the mechanisms that drive the intergenerational transmission of schooling. Marriage market outcomes appears to a play a crucial role, particularly whether the spouse has completed primary school, is literate, works in the formal sector, or works outside of agriculture.

Our cost benefit analysis highlights that under all reasonable assumptions the school construction program would pay for itself in terms of additional expected government tax revenues, not to mention the additional benefits of improved living standards. Furthermore, given the observed intergenerational transmission of education, the likely long-run benefits are vast. To gain additional insight into the intergenerational transmission of education, we perform an exploratory analysis calculating the intergenerational elasticity (IGE) of education between children and parents. Comparing the IGE across high and low program intensity areas and between young and old cohorts, we find there is an increase in mobility for children whose parents were exposed to the school construction program (see Mazumder, 2015 for a discussion of this literature). The broader societal impacts and changes in intergenerational transmission of human capital warrant further research.

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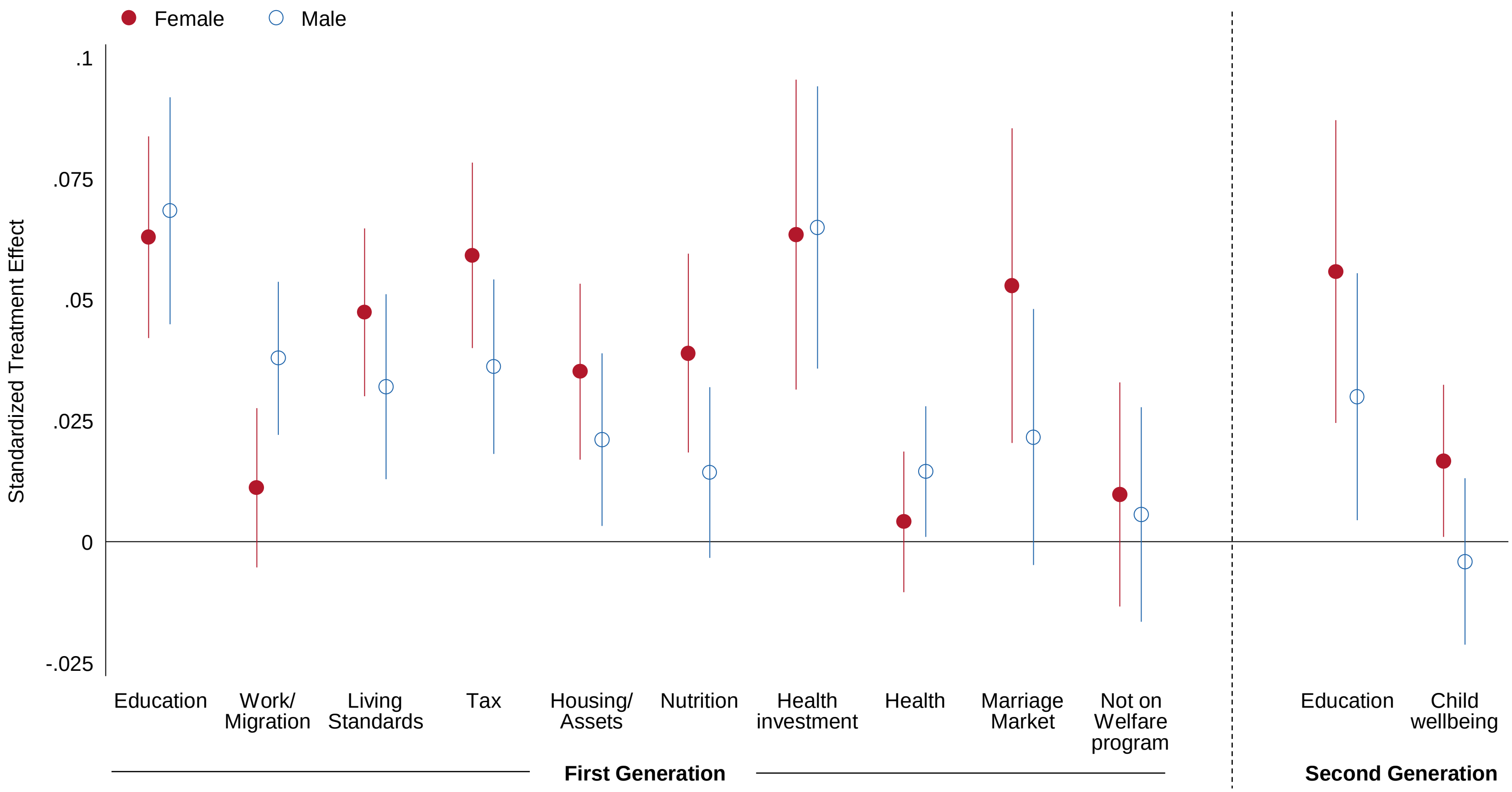

Figure 1. Effect of school construction on indexes of long-run outcomes

Notes: Following Kling, Liebman, and Katz (2007), we define indexes for families of outcomes by defining a Z-score for each outcome relative to the control group (defined in this case as the old cohort in low program intensity regions). Then, we average the Z-scores across all outcomes in the same family to get an index, such as "Education". Following Banerjee et al. (2015) to get standardized treatment effects, we then standardize the Kling indexes relative to the mean and standard deviation of the control group. In the figure, we present estimated regression coefficients and their respective 95% confidence intervals. The individual outcomes making up the index for each family are listed in Tables 1-12.

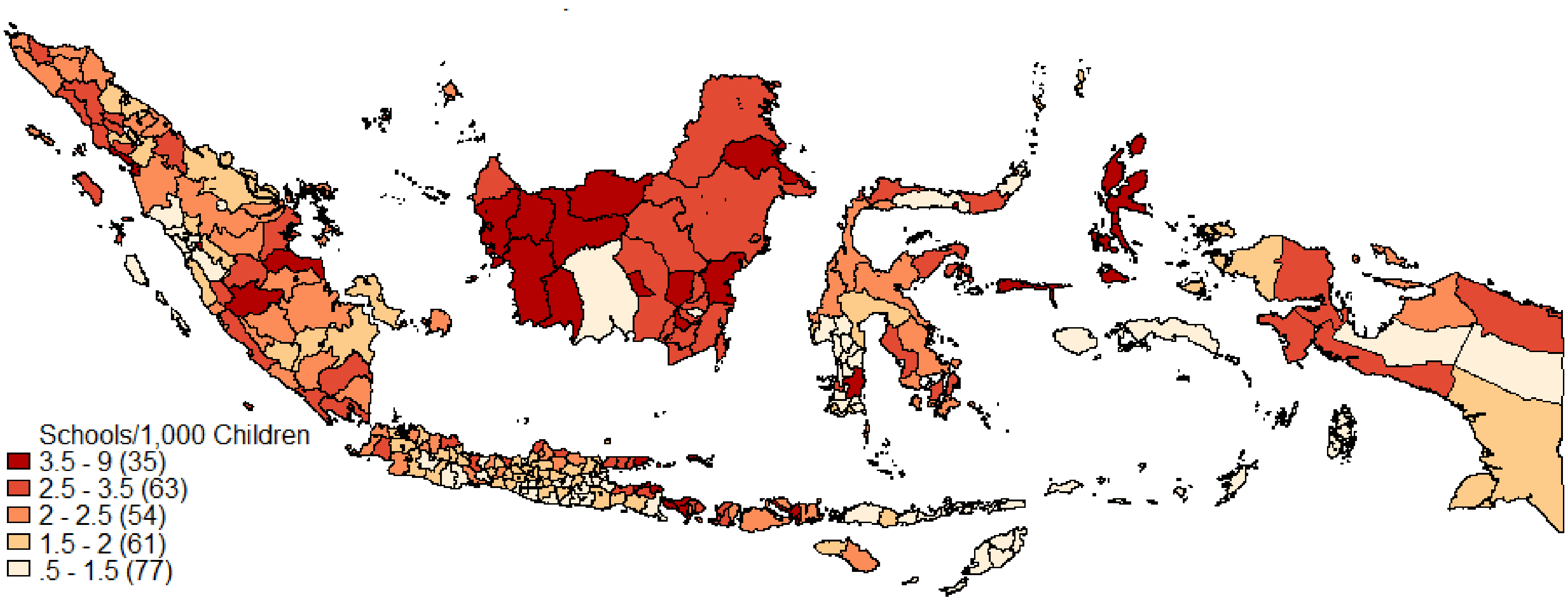

Figure 2. Spatial distribution of schools constructed per 1,000 children between 1973 and 1979

Notes: Number of schools constructed between 1973 and 1979 and children's population in 1971 were obtained from Duflo (2001) and the Indonesian 1971 Census. The legend indicates the range and distribution of schools constructed across the Indonesian archipelago. The numbers in parentheses refer to the number of districts that fall in that range. The total number of districts, 290, reflects their existence in 1993. Districts often split over time; by March 2016, there were 511 districts. In our analyses, we maintain the 1993 district boundaries to allow matching with Duflo (2001)'s school construction data.

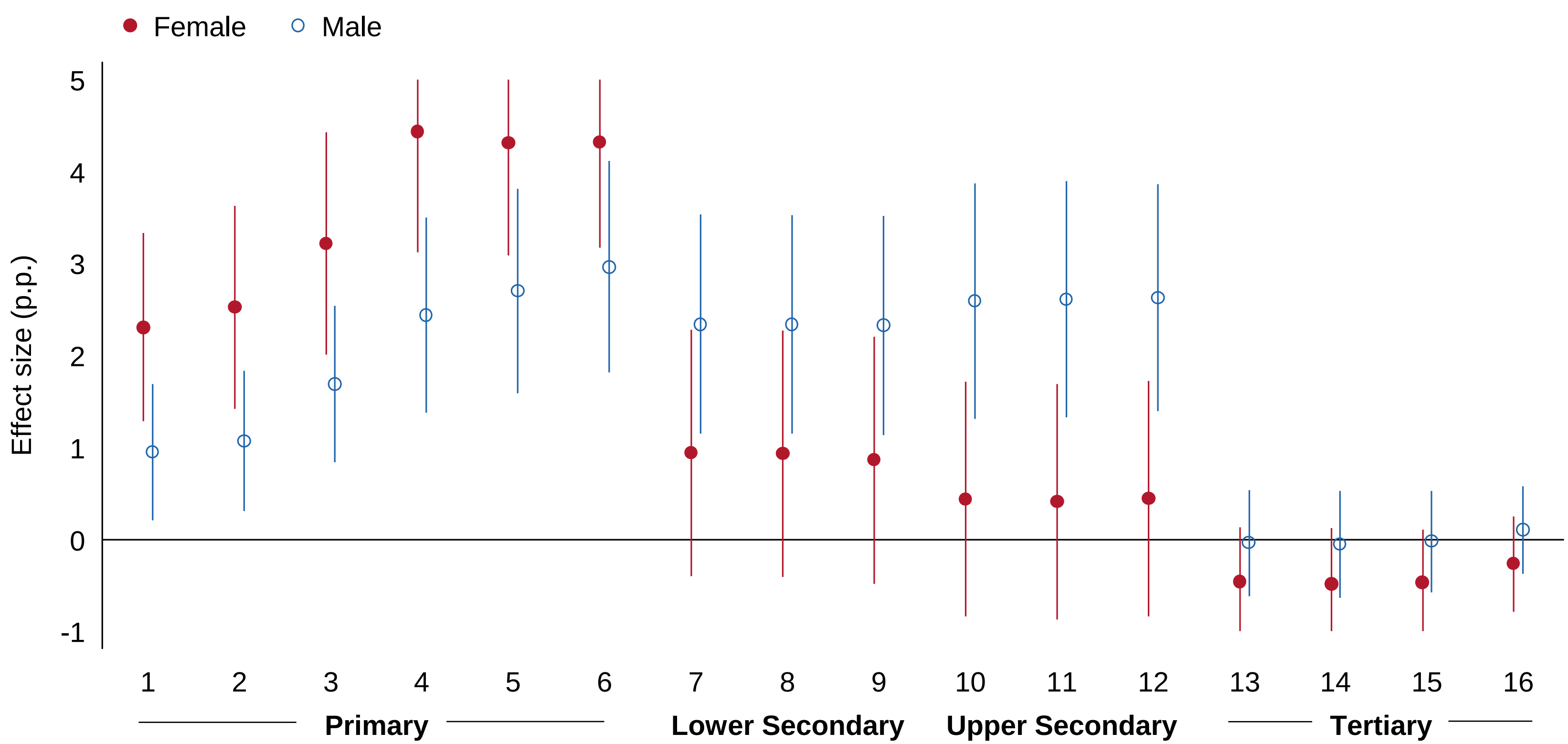

Figure 3. Effect of school construction on the probability of first generation individual attending at least *n*-years of schooling

Notes: Effect size measures the impact of one additional school constructed per 1,000 children on the probability of completing at least *n*-years of schooling in percentage points. We show estimated regression coefficients and their respective 95% confidence intervals.

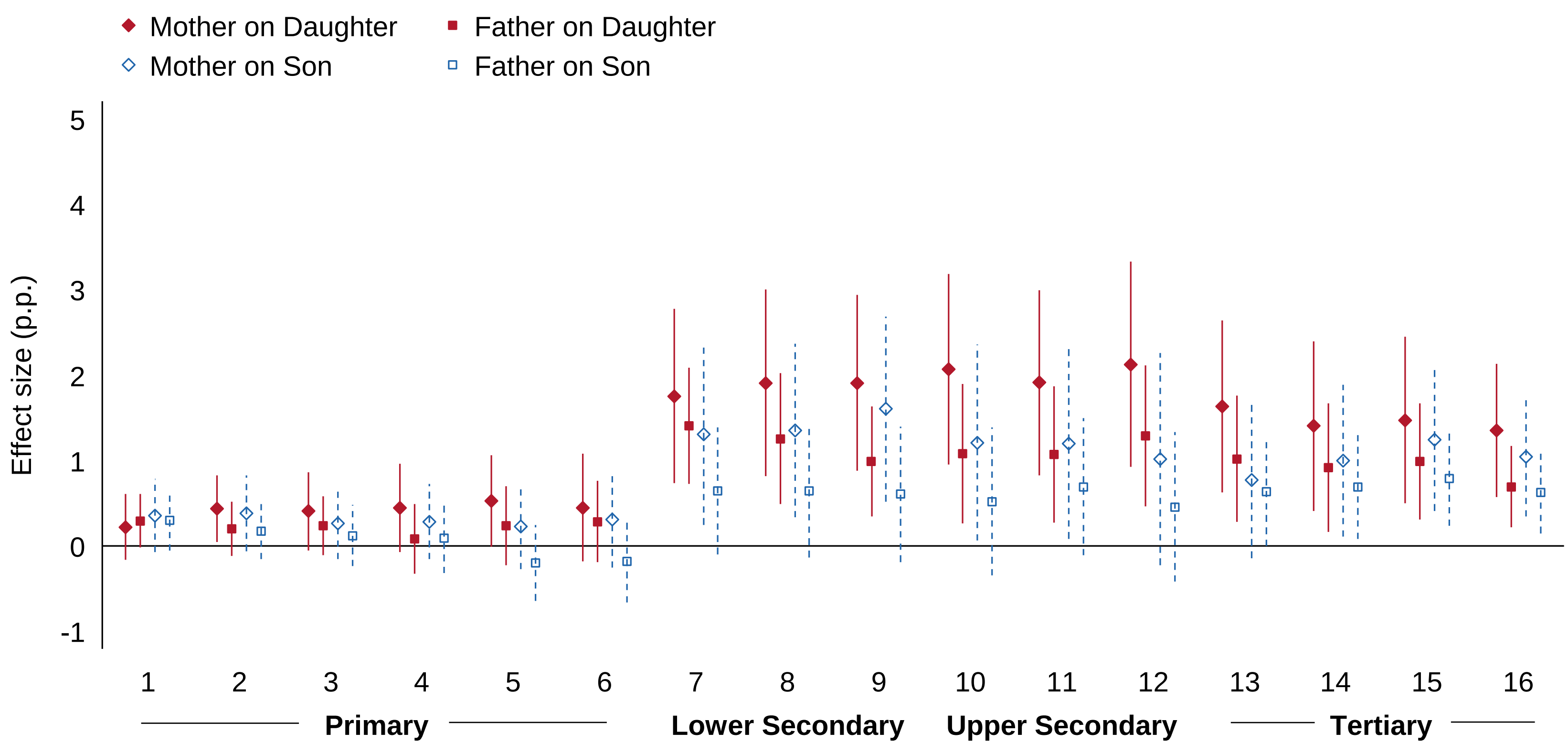

Figure 4 Effect of school construction on the probability of second generation individual attending at least *n*-years of schooling

Notes: Effect size measures the impact of one additional school constructed per 1,000 children in the mother's or father's birth district on the probability of a second generation individual (daughter or son) attending at least *n*-years of schooling in percentage points. Each dot represents a coefficient in a separate regression. We show estimated regression coefficients and their respective 95% confidence intervals.

Table 1. Effect of school construction on first generation's education

		Mean / SD		Effect of Program Exposure on:	
Outcome	**Description**	**Men**	**Women**	**Men**	**Women**
Years of schooling	Based on highest education level and grade attended. Standard durations of study are assumed; grade retentions are not counted	8.022 (4.230)	7.105 (4.215)	0.268*** (0.047) [0.000]	0.234*** (0.042) [0.000]
Completed Primary	Indicator defined as 1 if highest diploma completed is higher than or equal to Primary	0.813 (0.390)	0.727 (0.446)	0.026*** (0.006) [0.000]	0.041*** (0.006) [0.000]
Completed Lower Secondary	Indictor defined as 1 if highest diploma completed is higher than or equal to Lower Secondary	0.385 (0.487)	0.312 (0.463)	0.023*** (0.006) [0.000]	0.008 (0.007) [0.422]
Completed Upper Secondary	Indicator defined as 1 if highest diploma completed is higher than or equal to Upper Secondary	0.338 (0.473)	0.261 (0.439)	0.026*** (0.006) [0.000]	0.005 (0.006) [0.422]
Completed Tertiary	Indicator defined as 1 if highest diploma completed is higher than or equal to Tertiary	0.095 (0.293)	0.077 (0.267)	-0.001 (0.003) [0.741]	-0.003 (0.003) [0.422]
Literate	Literacy is a binary outcome and is self-reported	0.953 (0.212)	0.909 (0.287)	0.015*** (0.004) [0.001]	0.033*** (0.006) [0.000]
Education index	Aggregates all 6 outcomes and standardizes it to the mean of the old cohort in low-program regions. Effects are interpreted as standard deviation changes from the mean.			0.068*** (0.012)	0.063*** (0.011)

Notes: Effects of program exposure are the regression coefficients of young cohort dummy interacted with the number of schools constructed in region of birth. All regressions control for district of birth and cohort of birth fixed effects, children's population and enrollment in 1971, and water and sanitation program intensities that vary by region of birth interacted with birth year dummies. Robust standard errors clustered at region of birth are shown in parentheses. Stars denote statistical significance at 1, 5, and 10% levels based on unadjusted p-values. FDR q-values are computed over all 6 outcomes and are shown in square brackets. FDR q-values indicate the probability of false positives among *significant* tests. There are 72,367 and 71,423 observations in the men's and women's regressions, respectively.

Table 2. Effect of school construction on first generation's work and migration

		Mean / SD		Effect of Program Exposure on:	
Outcome	**Description**	**Men**	**Women**	**Men**	**Women**
Work	Indicator defined as 1 if individual worked in the past week or has an occupation but was temporarily absent from work in the past week	0.948 (0.223)	0.638 (0.481)	0.006** (0.003) [0.080]	0.003 (0.005) [0.953]
Work hours	Hours worked in the past week conditional on working, i.e. missing for non-working individuals	40.981 (17.115)	36.227 (18.792)	0.258 (0.158) [0.101]	0.157 (0.208) [0.953]
Formal worker	Indicator defined as 1 if individual reported working as an employee as opposed to being self-employed, family/unpaid work or freelance work, conditional on working	0.327 (0.469)	0.236 (0.425)	0.011*** (0.004) [0.032]	-0.005 (0.005) [0.953]
Non-agriculture sector	Indicator defined as 1 for working in a sector outside of agriculture; conditional on working	0.560 (0.496)	0.547 (0.498)	0.012*** (0.005) [0.032]	0.002 (0.005) [0.953]
Service sector	Indicator for working in trade, hotel, restaurant, transportation; warehousing, information, communication; finance and insurance, and service sectors, conditional on working	0.364 (0.481)	0.459 (0.498)	0.010*** (0.004) [0.032]	-0.000 (0.006) [0.953]
Migrant	Indicator defined as 1 if the current district of residence is not the same as the individual's birth district	0.273 (0.445)	0.245 (0.430)	0.007** (0.003) [0.085]	0.008** (0.003) [0.166]
Local migration	Indicator defined as 1 if migration occurred within the individual's birth province	0.108 (0.310)	0.106 (0.307)	0.005* (0.003) [0.101]	0.005** (0.003) [0.229]
Work/Migration index	Aggregates all 7 outcomes and standardizes it to the mean of the old cohort in low-program regions. Effects are interpreted as standard deviation changes from the mean.			0.038*** (0.008)	0.011 (0.008)

Notes: Effect of program exposure are the regression coefficients of young cohort dummy interacted with the number of schools constructed in region of birth. All regressions control for district of birth and cohort of birth fixed effects, children's population and enrollment in 1971, and water and sanitation program intensities that vary by region of birth interacted with birth year dummies. Robust standard errors clustered at region of birth are shown in parentheses. Stars denote statistical significance at 1, 5, and 10% levels based on unadjusted p-values. FDR q-values are computed over all 7 outcomes and are shown in square brackets. FDR q-values indicate the probability of false positives among *significant* tests. There are 72,367 observations for men and 68,574 conditional on working. There are 71,423 observations for women and 45,560 conditional on working.

Table 3. Effect of school construction on first generation's living standards

Outcome	Description	Mean / SD		Effect of Program Exposure on:	
		Men	Women	Men	Women
Total (Rp10k)	Household's average monthly expenditure; means are reported in 10,000 Indonesian Rupiah (IDR) increments. We apply an inverse hyperbolic sine transformation in the regression.	391.649	375.616	0.021***	0.032***
		(352.495)	(343.823)	(0.007)	(0.007)
				[0.010]	[0.000]
Food (Rp10k)	Estimates can be interpreted as percentage changes. Total expenditures are made up of food and non-food expenditures.	194.443	184.222	0.014**	0.028***
		(120.447)	(121.110)	(0.007)	(0.007)
				[0.036]	[0.000]
Non-food (Rp10k)		197.206	191.393	0.027***	0.039***
		(271.884)	(261.111)	(0.008)	(0.008)
				[0.004]	[0.000]
Non-food/Total	Share of non-food over total expenditures.	44.592	45.144	0.287***	0.237***
		(13.376)	(13.751)	(0.110)	(0.102)
				[0.024]	[0.021]
Education (Rp10k)	Education expenditures fall under non-food expenditures and include admission, tuition, extracurricular fees, textbooks, stationery, and tutoring	13.971	12.202	0.160**	0.193**
		(33.167)	(30.346)	(0.064)	(0.076)
				[0.024]	[0.011]
Living standards index	Aggregates total, food, and education expenditures and excludes non-food expenditure and non-food/total ratio to avoid collinearity in the regression. Standardizes it to the mean of the old cohort in low-program regions. Effects are interpreted as standard deviation changes from the mean.			0.032***	0.047***
				(0.010)	(0.009)

Notes: Effect of program exposure are the regression coefficients of young cohort dummy interacted with the number of schools constructed in region of birth. All regressions control for district of birth and cohort of birth fixed effects, children's population and enrollment in 1971, and water and sanitation program intensities that vary by region of birth interacted with birth year dummies. Robust standard errors clustered at region of birth are shown in parentheses. Stars denote statistical significance at 1, 5, and 10% levels based on unadjusted p-values. FDR q-values are computed over all 5 outcomes and are shown in square brackets. FDR q-values indicate the probability of false positives among *significant* tests. There are 68,687 and 66,249 observations in the men's and women's regressions, respectively.

Table 4. Effect of school construction on first generation's taxes

Outcome	Description	Mean / SD Men	Mean / SD Women	Effect of Program Exposure on: Men	Effect of Program Exposure on: Women
Total (Rp10k)	Self-reported tax expenditures include the following components and "other"	4.749 (11.433)	4.552 (10.743)	0.078*** (0.017) [0.000]	0.123*** (0.019) [0.000]
Land & building (Rp10k)	Taxes on land and/or building ownership	0.465 (2.742)	0.506 (2.446)	0.041* (0.022) [0.120]	0.075*** (0.021) [0.000]
Vehicle (Rp10k)	Motorized and non-motorized vehicle license fees	3.610 (8.076)	3.398 (7.821)	0.154*** (0.047) [0.003]	0.267*** (0.052) [0.000]
Local (Rp10k)	Levies/retributions; examples include: neighborhood/citizen associations, garbage, security, cemetery, parking, fees	0.469 (2.259)	0.468 (2.074)	0.048 (0.033) [0.148]	0.082** (0.039) [0.036]
Tax index	Aggregates all 4 outcomes and standardizes it to the mean of the old cohort in low-program regions. Effects are interpreted as standard deviation changes from the mean.			0.036*** (0.009)	0.059*** (0.010)

Notes: Effect of program exposure are the regression coefficients of young cohort dummy interacted with the number of schools constructed in region of birth. All regressions control for district of birth and cohort of birth fixed effects, children's population and enrollment in 1971, and water and sanitation program intensities that vary by region of birth interacted with birth year dummies. Robust standard errors clustered at region of birth are shown in parentheses. Stars denote statistical significance at 1, 5, and 10% levels based on unadjusted p-values. FDR q-values are computed over all 4 outcomes and are shown in square brackets. FDR q-values indicate the probability of false positives among *significant* tests. Total taxes includes land and building, vehicle, local, and other taxes. Other taxes include vehicle citations and income taxes, which were largely voluntary and represent a small contribution to government budget. "Other" taxes represent less than 5% of household tax expenditures. There are 68,687 and 66,249 observations in the men's and women's regressions, respectively.

Table 5. Effect of school construction on first generation's housing and assets

Outcome	Description	Mean / SD Men	Mean / SD Women	Effect of Program Exposure on: Men	Effect of Program Exposure on: Women
Urban	Indicator for residing in an urban area	0.425	0.438	-0.001	0.002
		(0.494)	(0.496)	(0.004)	(0.004)
				[0.822]	[0.576]
Rent equivalent (Rp10k)	Actual monthly rent if house is rented, or estimated rent value if house is owned or leased by the employer	42.991	43.085	0.012	0.028***
		(56.342)	(56.573)	(0.008)	(0.008)
				[0.293]	[0.001]
Floor area (m^2)	House's floor area in square meters	79.894	81.355	1.229**	1.480***
		(58.651)	(59.726)	(0.566)	(0.510)
				[0.119]	[0.011]
Utilities (Rp10k)	Expenditure on electricity, water, gas, and kerosene	15.714	15.729	0.051**	0.085***
		(20.983)	(21.796)	(0.022)	(0.024)
				[0.102]	[0.002]
Asset index	PCA index on binary ownerships of motorcycle, car, home phone, computer, TV, jewelry, refrigerator, water heater, LPG gas tube, boat, and air conditioner	-0.035	-0.069	0.030*	0.040**
		(1.868)	(1.882)	(0.017)	(0.015)
				[0.223]	[0.020]
Housing/Assets index	Aggregates all 5 outcomes and standardizes it to the mean of the old cohort in low-program regions. Effects are interpreted as standard deviation changes from the mean.			0.021**	0.035***
				(0.009)	(0.009)

Notes: Effect of program exposure are the regression coefficients of young cohort dummy interacted with the number of schools constructed in region of birth. All regressions control for district of birth and cohort of birth fixed effects, children's population and enrollment in 1971, and water and sanitation program intensities that vary by region of birth interacted with birth year dummies. Robust standard errors clustered at region of birth are shown in parentheses. Stars denote statistical significance at 1, 5, and 10% levels based on unadjusted p-values. FDR q-values are computed over all 5 outcomes and are shown in square brackets. FDR q-values indicate the probability of false positives among *significant* tests. There are 68,687 and 66,249 observations in the men's and women's regressions, respectively.

Table 6. Effect of school construction on first generation's nutrition

Outcome	Description	Mean / SD		Effect of Program Exposure on:	
		Men	Women	Men	Women
Calories	Household's accounts of units of food consumed in the past week (e.g. 5 kg of rice) are converted into nutritional intake by the Central Statistics Agency. Following their procedure, we convert the weekly intake to monthly intake. In the regressions, we apply an inverse hyperbolic transformation for reasons discussed above. The mean of calories intake is reported in 1 kcal increments. The means of protein, fat, and carbohydrate intakes are reported in 1 kg increments.	260.915	249.699	0.005	0.018***
		(106.001)	(109.833)	(0.004)	(0.005)
				[0.301]	[0.001]
Protein		7.116	6.831	0.006	0.018***
		(3.254)	(3.330)	(0.005)	(0.005)
				[0.301]	[0.001]
Fat		6.074	5.810	0.011**	0.023***
		(3.110)	(3.150)	(0.004)	(0.006)
				[0.061]	[0.000]
Carbohydrates		40.869	39.040	0.005	0.017***
		(17.728)	(18.245)	(0.004)	(0.005)
				[0.301]	[0.001]
Nutrition index	Aggregates all 4 outcomes and standardizes it to the mean of the old cohort in low-program regions. Effects are interpreted as standard deviation changes from the mean.			0.014	0.039***
				(0.009)	(0.010)

Notes: Effect of program exposure are the regression coefficients of young cohort dummy interacted with the number of schools constructed in region of birth. All regressions control for district of birth and cohort of birth fixed effects, children's population and enrollment in 1971, and water and sanitation program intensities that vary by region of birth interacted with birth year dummies. Robust standard errors clustered at region of birth are shown in parentheses. Stars denote statistical significance at 1, 5, and 10% levels based on unadjusted p-values. FDR q-values are computed over all 5 outcomes and are shown in square brackets. FDR q-values indicate the probability of false positives among *significant* tests. There are 68,687 and 66,249 observations in the men's and women's regressions, respectively.

Table 7. Effect of school construction on first generation's health investment

Outcome	Description	Mean / SD: Men	Mean / SD: Women	Effect of Program Exposure on: Men	Effect of Program Exposure on: Women
Total health expenditure (Rp10k)	Total monthly household health expenditures, which aggregates curative, medicine, and preventive health expenditures	7.517 (34.130)	7.961 (35.245)	0.071* (0.038) [0.114]	0.055 (0.041) [0.185]
Preventive measures (Rp10k)	Consist of pregnancy checks, immunizations, medical check-ups, family planning, and other expenditures, e.g., vitamins, massage, gym memberships	0.744 (3.225)	0.671 (3.135)	0.242*** (0.068) [0.002]	0.193*** (0.071) [0.013]
Family planning (Rp10k)	A sub-category under preventive health expenditures, which includes costs of contraceptives and consultations	0.286 (0.872)	0.219 (0.856)	0.321*** (0.061) [0.000]	0.226*** (0.071) [0.008]
Private hospital (Rp10k)	A sub-category under curative health expenditures and is distinct from expenditures on public hospitals, clinics, and traditional healers	2.101 (20.718)	2.200 (22.266)	0.048** (0.023) [0.114]	0.075*** (0.024) [0.008]
Health insurance (Rp10k)	Health insurance is distinct from life, accidental, vehicle, and house insurances	3.821 (16.425)	3.635 (14.047)	0.083 (0.055) [0.134]	0.142*** (0.048) [0.009]
Health investment index	Aggregates all 5 outcomes and standardizes it to the mean of the old cohort in low-program regions. Effects are interpreted as standard deviation changes from the mean.			0.065*** (0.015)	0.063*** (0.016)

Notes: Effect of program exposure are the regression coefficients of young cohort dummy interacted with the number of schools constructed in region of birth. All regressions control for district of birth and cohort of birth fixed effects, children's population and enrollment in 1971, and water and sanitation program intensities that vary by region of birth interacted with birth year dummies. Robust standard errors clustered at region of birth are shown in parentheses. Stars denote statistical significance at 1, 5, and 10% levels based on unadjusted p-values. FDR q-values are computed over all 5 outcomes and are shown in square brackets. FDR q-values indicate the probability of false positives among *significant* tests. There are 68,687 and 66,249 observations in the men's and women's regressions, respectively.

Table 8. Effect of school construction on first generation's health outcomes

		Mean / SD		Effect of Program Exposure on:	
Outcome	**Description**	**Men**	**Women**	**Men**	**Women**
No health complaint	Self-reported indicator defined as 1 if did not experience a health complaint in the past month	0.690 (0.463)	0.646 (0.478)	0.004 (0.004) [0.352]	0.003 (0.004) [0.771]
Non-disrupted days	Self-reported number of days in the past month (maximum of 30 days) that a health complaint did not disrupt daily activities	28.851 (4.012)	28.801 (4.064)	0.042 (0.028) [0.266]	0.027 (0.033) [0.771]
No severe health complaint	Self-reported indicator defined as 1 if did not experience a severe health complaint in the past month	0.951 (0.216)	0.949 (0.221)	0.005*** (0.002) [0.025]	-0.001 (0.002) [0.771]
Health outcomes index	Aggregates all 3 outcomes and standardizes it to the mean of the old cohort in low-program regions. Effects are interpreted as standard deviation changes from the mean.			0.015** (0.007)	0.004 (0.007)

Notes: Effect of program exposure are the regression coefficients of young cohort dummy interacted with the number of schools constructed in region of birth. All regressions control for district of birth and cohort of birth fixed effects, children's population and enrollment in 1971, and water and sanitation program intensities that vary by region of birth interacted with birth year dummies. Robust standard errors clustered at region of birth are shown in parentheses. Stars denote statistical significance at 1, 5, and 10% levels based on unadjusted p-values. FDR q-values are computed over all 3 outcomes and are shown in square brackets. FDR q-values indicate the probability of false positives among *significant* tests. There are 72,367 and 71,423 observations in the men's and women's regressions, respectively.

Table 9. Effect of school construction on first generation's marriage market

		Mean / SD		Effect of Program Exposure on:	
Outcome	**Description**	**Men**	**Women**	**Men**	**Women**
Age of first marriage	Age of first marriage for ever-married household members	25.219 (5.022)	20.888 (4.788)	0.058 (0.053) [0.476]	0.050 (0.059) [0.395]
Spouse's education	Spouse's years of schooling is defined only for household heads and spouses	7.635 (4.081)	7.426 (4.192)	0.180*** (0.046) [0.000]	0.116*** (0.043) [0.028]
Spouse still alive	Indicator defined as 1 if marital status is married or divorced, as opposed to widowed; missing for never married individuals	0.971 (0.169)	0.866 (0.340)	-0.002 (0.002) [0.476]	0.010** (0.004) [0.032]
Children 0-14	Number of children aged 0-14 living in the household	0.910 (1.059)	0.559 (0.868)	-0.012 (0.017) [0.476]	-0.035** (0.016) [0.063]
Marriage market index	Aggregates all 4 outcomes and standardizes it to the mean of the old cohort in low-program regions. Effects are interpreted as standard deviation changes from the mean. For the index, we reverse the sign for children 0-14 to indicate a positive outcome.			0.022 (0.013)	0.053*** (0.016)

Notes: Effect of program exposure are the regression coefficients of young cohort dummy interacted with the number of schools constructed in region of birth. All regressions control for district of birth and cohort of birth fixed effects, children's population and enrollment in 1971, and water and sanitation program intensities that vary by region of birth interacted with birth year dummies. Robust standard errors clustered at region of birth are shown in parentheses. Stars denote statistical significance at 1, 5, and 10% levels based on unadjusted p-values. FDR q-values are computed over all 4 outcomes and are shown in square brackets. FDR q-values indicate the probability of false positives among *significant* tests. The age of first marriage and the spouse still alive regressions have 70,571 and 69,623 observations for men and women, respectively because it is set to missing if the individual is never married. The spouse's education regression has 64,422 and 55,468 observations because it is set to missing if the spouse does not currently live in the household (divorce, widow). The children 0-14 regression has 68,687 and 66,249 observations and corresponds to the number of household heads and spouses in Table 3.

Table 10. Effect of school construction on first generation's welfare program participation

Outcome	Description	Mean / SD Men	Mean / SD Women	Effect of Program Exposure on: Men	Effect of Program Exposure on: Women
Cash Transfer	Unconditional cash transfer to compensate for the removal of gas price subsidy for poor households	0.041 (0.197)	0.039 (0.194)	-0.002 (0.002) [0.742]	-0.001 (0.002) [0.914]
Rice for Poor	Monthly rice allowance for poor households	0.392 (0.488)	0.406 (0.491)	0.002 (0.004) [0.850]	-0.009* (0.005) [0.200]
Poor Student's Assistance	Cash transfer conditional on school enrollment	0.056 (0.363)	0.127 (0.333)	-0.001 (0.004) [0.850]	0.000 (0.004) [0.914]
Social Protection Card	Card provided to poor households, which entitles them to social welfare programs mentioned above	0.186 (0.389)	0.180 (0.384)	-0.001 (0.004) [0.850]	-0.000 (0.004) [0.914]
Welfare program non-participation index	Aggregates all 4 outcomes and standardizes it to the mean of the old cohort in low-program regions. Effects are interpreted as standard deviation changes from the mean. For the index, we reverse the sign for the 4 welfare programs to indicate a positive outcome.			0.006 (0.011)	0.010 (0.012)

Notes: Means indicate the fraction of program recipients. Effect of program exposure are the regression coefficients of young cohort dummy interacted with the number of schools constructed in region of birth. All regressions control for district of birth and cohort of birth fixed effects, children's population and enrollment in 1971, and water and sanitation program intensities that vary by region of birth interacted with birth year dummies. Robust standard errors clustered at region of birth are shown in parentheses. Stars denote statistical significance at 1, 5, and 10% levels based on unadjusted p-values. FDR q-values are computed over all 4 outcomes and are shown in square brackets. FDR q-values indicate the probability of false positives among *significant* tests. There are 68,687 and 66,249 observations in the men's and women's regressions, respectively.

Table 11. Effect of school construction on second generation's education

Outcome	Description	Mean / SD		Effect of Program Exposure by:	
		Father	Mother	Fathers on Children	Mothers on Children
Years of schooling	Child's years of school based on highest education level and grade attended. Standard durations of study are assumed; grade retentions are not counted	7.967 (4.340)	8.854 (4.278)	0.097*** (0.032) [0.014]	0.169*** (0.045) [0.001]
Completed Primary	Indicator defined as 1 if child's highest diploma completed is higher than or equal to Primary	0.637 (0.481)	0.728 (0.445)	0.000 (0.002) [0.928]	0.001 (0.003) [0.796]
Completed Lower Secondary	Indictor defined as 1 if child's highest diploma completed is higher than or equal to Lower Secondary	0.413 (0.492)	0.504 (0.500)	0.006* (0.003) [0.171]	0.015*** (0.005) [0.006]
Completed Upper Secondary	Indicator defined as 1 if child's highest diploma completed is higher than or equal to Upper Secondary	0.217 (0.412)	0.300 (0.458)	0.009** (0.004) [0.061]	0.014*** (0.005) [0.013]
Completed Tertiary	Indicator defined as 1 if child's highest diploma completed is higher than or equal to Tertiary	0.041 (0.198)	0.064 (0.245)	0.004* (0.002) [0.171]	0.008** (0.003) [0.044]
Age-for-grade	Indicator for child starting primary school by age 7 and never repeating school up to Upper Secondary	0.835 (0.371)	0.789 (0.408)	0.011*** (0.004) [0.030]	0.018*** (0.005) [0.002]
Second generation education index	Aggregates all 6 outcomes and standardizes it to the mean of the old cohort in low-program regions. Effects are interpreted as standard deviation changes from the mean.			0.030** (0.013)	0.056*** (0.016)

Notes: Effect of program exposure are the regression coefficients of father or mother's young cohort dummy interacted with the number of schools constructed in father or mother's region of birth. All regressions control for parent's district of birth and cohort of birth fixed effects, child age fixed effects, children's population and enrollment in 1971, and water and sanitation program intensities that vary by region of birth interacted with birth year dummies. Robust standard errors clustered at parent's region of birth are shown in parentheses. Stars denote statistical significance at 1, 5, and 10% levels based on unadjusted p-values. FDR q-values are computed over all 6 outcomes and are shown in square brackets. FDR q-values indicate the probability of false positives among *significant* tests. The survey restricts questions on educational attainment to individuals aged 5 and older. There are 120,838 and 105,523 observations in the father's and mother's regressions, respectively.

Table 12. Effect of school construction on second generation's child wellbeing

Outcome	Description	Mean / SD: Father	Mean / SD: Mother	Effect of Program Exposure by: Fathers on Children	Effect of Program Exposure by: Mothers on Children
Non-work days	Number of days <u>not</u> worked in the past week by the child unconditional on work, i.e. 7 for non-working individuals	5.317 (2.670)	4.820 (2.865)	0.044** (0.021) [0.136]	0.031 (0.019) [0.463]
Non-work hours	Number of hours <u>not</u> worked in the past week by the child unconditional on work, i.e. 168 for non-working individuals	156.679 (19.704)	153.047 (21.597)	0.299* (0.157) [0.173]	0.215 (0.151) [0.463]
No health complaint	Self-reported indicator defined as 1 if child did not experience a health complaint in the past month	0.797 (0.402)	0.823 (0.382)	-0.008*** (0.003) [0.042]	0.004 (0.003) [0.463]
Non-disrupted days	Self-reported number of days in the past month (maximum of 30 days) that a health complaint did <u>not</u> disrupt child's daily activities	29.492 (2.086)	29.550 (2.067)	-0.026* (0.016) [0.198]	0.007 (0.015) [0.893]
No severe health complaint	Self-reported indicator defined as 1 if child did not experience a severe health complaint in the past month	0.978 (0.147)	0.980 (0.140)	-0.000 (0.001) [0.751]	-0.000 (0.001) [0.893]
Second generation wellbeing index	Aggregates all 5 outcomes and standardizes it to the mean of the old cohort in low-program regions. Effects are interpreted as standard deviation changes from the mean.			-0.004 (0.009)	0.017** (0.008)

Notes: Effect of program exposure are the regression coefficients of father or mother's young cohort dummy interacted with the number of schools constructed in father or mother's region of birth. All regressions control for parent's district of birth and cohort of birth fixed effects, child age fixed effects, children's population and enrollment in 1971, and water and sanitation program intensities that vary by region of birth interacted with birth year dummies. Robust standard errors clustered at parent's region of birth are shown in parentheses. Stars denote statistical significance at 1, 5, and 10% levels based on unadjusted p-values. FDR q-values are computed over all 5 outcomes and are shown in square brackets. FDR q-values indicate the probability of false positives among *significant* tests. The survey restricts questions on labor market outcomes to individuals aged 10 and older; questions on health outcomes are asked to all individuals. There are 100,293 and 94,067 observations in the father's and mother's regressions for labor market outcomes; 129,971 and 108,607 observations in the father's and mother's regressions for health outcomes.

Table 13. Effect of school construction on second generation's education, by parent and child gender

	Years of schooling	
	(1)	(2)
Parents born between:	1957-1962 and 1968-1972	1950-1980
Panel A: Sons and Daughters		
Father exposed	0.001	0.044**
	(0.038)	(0.021)
Mother exposed	0.160***	0.118***
	(0.059)	(0.035)
Father = Mother (p-value)	0.046	0.050
Mean	8.674	7.827
Observations	44,105	246,466
Panel B: Sons Only		
Father exposed	-0.038	0.042
	(0.049)	(0.026)
Mother exposed	0.139**	0.094**
	(0.069)	(0.040)
Father = Mother (p-value)	0.076	0.267
Mean	8.575	7.787
Observations	24,366	133,896
Panel C: Daughters Only		
Father exposed	0.036	0.046**
	(0.051)	(0.023)
Mother exposed	0.188***	0.140***
	(0.072)	(0.038)
Father = Mother (p-value)	0.134	0.026
Mean	8.796	7.875
Observations	19,739	112,570

Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors in parentheses, clustered at the father and mother's birth district level using the multiway clustering method of Cameron, Gelbach, and Miller (2011). Father exposed indicates an interaction of the number of INPRES primary schools constructed in the father's birth district and an indicator that the father is in the young cohort. Mother exposed is defined similarly. Father = Mother indicates the p-value testing the equality of coefficients of father exposed and mother exposed within each panel. The sample in Panel A consists of both sons and daughters, Panel B sons only, and Panel C daughters only.

Table 14. Cost-benefit analysis of school construction

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Parameters										
Discount rate (%)	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0
Teachers salary growth (Y/N)	N	Y	Y	Y	Y	Y	Y	Y	Y	Y
Lifetime curvature (Y/N)	N	N	Y	Y	Y	Y	Y	Y	Y	Y
GDP/capita growth (%)	0	0	0	3.25	3.25	3.25	3.25	3.25	3.25	3.25
Students/schools	120	120	120	120	180	180	180	180	180	180
School lifetime (years)	20	20	20	20	20	40	40	40	40	40
Start paying taxes after age:	18	18	18	18	18	18	22	22	22	22
Recurrent costs/salaries multiplier	1.25	1.25	1.25	1.25	1.25	1.25	1.25	1.5	1.5	1.5
Teachers/schools	3	3	3	3	3	3	3	3	6	6
Life expectancy	60	60	60	60	60	60	60	60	60	70
Costs										
Schools construction						0.78				
Teachers training						0.12				
Teachers' salaries	1.65	2.95	2.95	2.95	2.95	5.08	5.08	6.10	12.19	12.19
Benefits										
Paid by cohorts born in			1968-1980			1968-2000		1968-2000		
Collected between years			1987-2040			1987-2060		1991-2060		1991-2070
Tax receipts	9.00	9.00	7.32	6.11	9.16	19.87	18.14	18.14	18.14	20.74
Net Benefit (Benefits - Costs)	6.56	5.26	3.58	2.37	5.42	14.00	12.27	11.25	5.15	7.76
Breakeven year	1998	2001	2007	2017	2009	2013	2016	2018	2031	2031
Living standards	61.69	61.69	53.18	43.64	65.46	142.00	128.34	128.34	128.34	146.49
Net Benefit (Benefits - Costs)	59.24	57.95	49.44	39.90	61.72	136.12	122.47	121.45	115.36	133.50
Breakeven year	1990	1991	1992	1995	1994	1994	1998	1999	2003	2003
Internal Rate of Return (%)										
Tax receipts	10.48	8.87	7.68	6.64	8.10	9.11	8.53	8.05	6.05	6.37
Living standards	20.68	19.38	17.69	14.83	16.84	17.57	15.77	15.26	13.08	13.15

Note: All values are in billions of US dollars in 2016. Assumptions on number of students and teachers per school, recurrent costs/salaries multiplier, and school lifetime follow Duflo (2001). Schools construction costs were obtained from Duflo (2001), teachers training from Daroesman (1972), and teachers' salaries from various sources (see Appendix C for more details). Benefits are paid by cohorts that could attend the full 6 years of primary education until their death. Breakeven year is the first year when the present discounted value of benefits exceeds that of costs. Tax receipts consist of direct tax expenditures plus 10% VAT on total expenditures. Living standards is proxied with total household expenditures. Internal rate of return is the discount rate that equates the present discounted value of benefits and costs.