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 and exploit 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 who received more education are more likely to be formal workers and work in a non-agricultural sector. Households in which either parent received more education have higher consumption, more assets, and pay more government taxes. These education benefits are transmitted to the next generation. Increased parental education has larger impacts for daughters, particularly if the mother was exposed to the school construction program. Migration and marriage are potential mechanisms linking additional education and improved long-term outcomes.

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1. Introduction

Education has typically been viewed as critical for countries' economic development, but various approaches, both micro and macro, have attempted to measure education's impact and results have been mixed (for example, see Card and Krueger, 1992, Card and Lemieux, 1998, and Duflo, 2001). Much of the macroeconomic literature has found only small impacts of human capital on growth and development (Klenow and Rodriguez-Clare, 1997; Bils and Klenow, 2000; Klenow and Rodriguez-Clare, 2005).[1] Microeconomic research that tries to measure the causal effects of education finds that these effects differ depending on the specific identification strategy used. Randomized control trials focusing on education are important for measuring the effects of specific treatment interventions but are generally not able to measure broader economy-wide impacts of education (Attanasio et al., 2017; Duflo, Dupas, and Kremer, 2017). Furthermore, much of the recent focus on education by developing country governments and non-governmental organizations has been on demand-side education interventions as opposed to the initially more costly supply-side options (see McEwan, 2015 for a review of 111 education interventions).[2] How important is it for people to get education? What adult outcomes are improved through education, by how much, and do these effects persists into the next generation? These questions are of great policy importance and broad research interest. Yet, the

[1] Krueger and Lindahl (2001) try to reconcile the microeconometric evidence indicating that education is an important determinant of income at the individual level with the macroeconomic studies that find increases in education are unrelated to economic growth.

[2] The objective of these education interventions is to improve later-life outcomes for these children, but there is very little evidence on whether they are successful in the long-term (McEwan, 2015 indicates that only 10% of the 111 interventions have any evaluation more than a month after the treatment ended). There are a few exceptions. A de-worming intervention for school children in Kenya showed labor market impacts 10 years after the intervention (Baird et al., 2016). In Jamaica, 20 years after a home visit early childhood stimulation program, children exposed to the program had higher earnings (Gerterl et al., 2014). In the US, preschool and kindergarten programs led to improved adult outcomes (Heckman et al., 2010; Chetty et al., 2018). The worry with many education interventions is the potential for the impact to fade-out over time. In Kenya, Evans and Ngatia (2018) find while in the short-run providing school uniforms improved school participation, eight years after the intervention there were no impacts on treated children.

causal impact of education is usually hard to estimate because the choice of how much education to pursue is correlated with a large number of individual, household, and community characteristics.

In this paper, we examine the impact of education on a wide range of long-run and intergenerational outcomes, using the fastest primary school construction project ever undertaken in the world (World Bank, 1990). Between 1973 and 1978, 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 1970s 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 three main contributions. First, we estimate the reduced form relationship between the school construction program and an extensive range of long-term outcomes many of which are important but have not been previously studied, covering education, work, migration, household consumption, taxes, assets, food intake, health, marriage, and the receipt of welfare programs. The new data also allow us to explore the differential impacts for men and women and measure whether the short-term education impact persists into adulthood when these exposed individuals are now in the forties. Second, we examine the intergenerational impacts of an exogenous increase in education to start to untangle how parent's education impacts their children's education. Third, we are able to study the impacts of supply-side educational interventions that many developing countries are adopting and evaluate whether school construction could pay for itself in terms of higher future government tax revenues.

Figure 1 provides an overview of our findings. We present standardized treatment effects from exposure to the school construction program on a range of outcome indexes. Section 4 provides more details about how the indexes are created for families of outcomes following Kling, Liebman, and Katz (2007). The consistent pattern is that exposure to the school construction program improves almost every family of outcomes that we are able to explore in the data. School construction, not surprisingly, leads to improved educational outcomes (as was previously shown in Duflo, 2001 for men, but we are now able to confirm that it also improves women's education). The education effects for women are concentrated in primary school, while men also see significant increases in lower and upper secondary education. There are significant improvements in the indexes for men and women in work, migration, household consumption, taxes, assets, food intake, and health investments. As adults, men who received more education are more likely to be employed and to work in the formal sector. Households in which either parent received more education have higher consumption, more assets, and pay more government taxes. While health expenditures increase, we do not observe any improvements in health outcomes. Increased education leads to improved marriage market outcomes, with both better educated and healthier spouses. We find no impact of school construction on the likelihood of receiving government welfare.

Parents transmit these effects to the next generation, who have more education, with larger impacts 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 adults, we do not find any evidence of improved health outcomes. Increased parental education has larger impacts for daughters, particularly if the mother was exposed to the school construction program.

In terms of policy implications, preliminary cost-benefit analysis indicates that school construction leads to higher future government tax revenues and approximately 6 years of these additional taxes would have covered the entire costs for the construction of all 61,000 schools. If in addition, teacher salaries are included, then 8 more years of these additional tax revenues would have covered their costs. Overall, 14 years of additional taxes would cover building all 61,000 schools and paying teachers during the 20 year expected life of the school. These results provide strong evidence that supply-side interventions could also be cost-effective for developing country governments to implement.

The rest of this paper is organized as follows. Section 2 describes the institutional context and school construction program in Indonesia and presents the data. Section 3 describes the empirical identification strategy. Section 4 presents the results examining the impacts of increased education on a range of long-term and intergenerational outcomes. Section 5 shows results of a number of robustness checks. Section 6 presents the cost-benefit analysis and Section 7 concludes.

2. Institutional Context and Data

Beginning from Soeharto's rise to presidency in 1967, Indonesia's plan for national development were outlined in a series of Five-Year Development Plans (*Repelita*) by the Ministry of National Development Planning (*Bappenas*). The first *Repelita* in 1969-1974 focused on meeting basic needs and building agricultural infrastructure (World Bank 1990). "One important component of the Government's development program for creating social and physical infrastructure and the reduction of poverty was the establishment of the INPRES expenditure program. INPRES established a means of sharing Central Government revenues with lower-level governments through a system of flexible direct subsidies with the aim of reducing interregional disparity and building and supporting infrastructure in the provinces. There are both general INPRES grants to the provincial, district and village-level governments, and sectoral INPRES grants for the construction of roads, elementary schools, health facilities and reforestation, as well as their operations and maintenance. The infrastructure created by these projects—primary schools, health facilities, roads—provided important benefits to Indonesia's rural population. Moreover, the small-scale infrastructure projects funded under the INPRES program provided substantial employment for unskilled labor in rural areas, growing from an estimated quarter of a million man-years of work in 1970 to about 1.5 million by 1982 (about 2.7% of the labor force)" (World Bank 1990).

In *Bappenas* official report (1974), the government's efforts in the education sector includes revamping the curriculum, addressing inequity in student enrollment, increasing the quantity and quality of teachers, and improving schooling infrastructures. From 1969 to 1973, primary school enrollment increased from 12.8 million to 13.6 million. 63.5 million textbooks were printed and distributed; 90% of which goes to primary education.

In the last year of the first *Repelita*, the president issued a presidential instruction (*Inpres*) to construct 6,000 new primary school buildings, provide 2.8 million textbooks and 6.6 million library books, and recruit more than 50,000 teachers. The effort was continued with another presidential instruction in 1974 to construct another 6,000 primary schools, provide 4.5 million textbooks and 6.9 million library books, train 18,000 and recruit 36,000 teachers (Bappenas 1975). The school constructions represent a substantial improvement from the meager availability of 60,023 primary school buildings in 1968[3]. The second *Repelita* in 1974-1979 fully integrated the massive effort to improve national primary education. In five years, Bappenas reported that 31,000 primary schools were constructed and 56,000 were rehabilitated, 263,000 teachers were recruited and 1.4 million trained, 273 million textbooks and 39 million library books were provided (1979). To study the impact of this large-scale school construction program, we make use of Duflo (2001)'s data of INPRES schools constructed in each district between 1973/74 to 1978/79.[4]

To study the long-term and intergenerational effect of the INPRES school construction program, we use the National Socioeconomic Survey conducted in 2016, henceforth 'Susenas 2016', which is administered by Indonesia's Central Statistics Bureau, Badan Pusat Statistik. Susenas 2016 combines a large sample size of 291,414 households and 1,048,575 individuals[5] with a wide range of variables, including on education, employment, migration, health, marriage market outcomes, households consumption and asset, and taxes.[6] Susenas 2016 is nationally representative and covers all 34 provinces and 511 districts of Indonesia.[7] Appendix Table 1 describes all outcome variables used in this study, together with the mean and standard deviation for men and women separately.

3. Empirical Strategy

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

[3] http://lets-sekolah.blogspot.com/2016/04/pendidikan-di-masa-orde-baru.html
[4] We are grateful to Esther Duflo for sharing the INPRES school construction data
[5] This is considerably larger than the 16,204 households of the fifth Indonesian Family Life Survey (IFLS), conducted in 2014, which Bharati et al. (2017) show is underpowered to estimate the effect of school construction on education.
[6] Susenas 2016 does not include information on earnings, unlike the 1995 Intercensal survey that was used by Duflo (2001). The earnings question was discontinued in the Intercensal survey after the 1995 round.
[7] The smallest geographical unit in the Susenas 2016 is the Indonesian 'kabupaten', loosely translated to district.

school construction program. Children in Indonesia typically attend primary school between the ages of seven to twelve. When the first INPRES schools were constructed in 1974, children who were born in or before 1962 were at least 12 years of age and would not have benefited from the school construction.[8] 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 might be 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 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 1973/74 to 1978/79 per 1,000 children, $Young_{it}$ is an indicator variable for being born between 1968-1972 (ages 2-6 in 1974)[9], μ_j is a

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[8] 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.

[9] Individuals born between 1957 to 1962 (ages 12-17 in 1974) represent older birth cohorts that were not exposed to the school construction program. Following Duflo (2001), we exclude individuals born between 1963 to 1967 (ages 7-11 in 1974) as they might have only partially benefited from the school construction. In addition, Duflo (2001) restricted the young cohort to those born before 1972 in order to allow the exposed individuals to have completed schooling and participated in the labor market by 1995, the collection date for her data. With our 2016 data, sufficient time has passed since these individuals left school, and we can relax these cohort definitions. In robustness

time-invariant district of birth fixed effect, δ_t is the cohort of birth fixed effect, 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.[10] 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 males and females.

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 treatment status (birth cohort and region of birth) of the household head or the spouse. In Equation (1), *j* then refers to the district of birth of the household head while *t* refers to the year of birth of the household head.[11]

The duration between the school construction that started in 1974 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.

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checks discussed in Section 6, we show that results are robust to adding in younger cohorts (born 1973-1980), older cohorts (born 1950-1956), and partially treated cohorts (born 1963-1967).

[10] 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 where the INPRES schools were built. Water and sanitation programs were the second largest set of INPRES programs delivered by the central government.

[11] This treatment 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 1961, 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 decision of treatment status assignment does not influence the main results.

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:

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

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 whether the father or mother belongs to the young cohort, and θ_a is child c's age fixed effect.[12] Standard errors are clustered at the father's or mother's birth district.[13]

4. Results

This section describes the impact of the INPRES school construction program on long-run and intergenerational outcomes. Following the estimation strategy outlined in the previous section, the main explanatory variable is an interaction of the intensity of school construction in a person's birth district with an indicator variable for being young enough to have benefitted from the program.

4.1. The effect of school construction on educational attainment

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

[12] We include child age fixed effects because parents in the old cohort will mechanically have older children 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.

[13] Susenas 2016 only identifies parental relationships for each individual with respect to the household head. If there were two families living in a given household, the children in the second family will be recorded as "other" household members. Since we cannot identify the parental relationship of those children in the second family, our intergenerational analysis is restricted to the household head and his spouse's biological and adopted children.

program, and across two indexes that capture second generation effects on their children. Each index captures a family of outcomes and we follow Kling et al. (2007) to construct these indexes. First, we create z-scores for each outcome in its family relative to the control group, which we define 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" or "Work/Migration". Following Banerjee et al. (2015) to get standardized treatment effects, we then normalize the Kling indexes by converting them into z-scores relative to the mean and standard deviation of the control group. We present estimated regression coefficients and their respective 95% confidence intervals. Appendix Table 1 list the variables that these indexes are based on together with the means and treatment effects for men and women separately.

The first index on education comprises the first five variables of Appendix Table 1, separately for men and women.[14] The program increases average years of education for men by 0.27 years and for women by 0.23. While the point estimate for women is slightly smaller than for men, it represents a higher percentage increase compared to an average years of education of 8.08 for men and 7.15 for women. These differences cannot be distinguished statistically. The next four rows break the education effects down by completed level of education. For men, the program caused a 2.6 percentage point increase in the likelihood of having completed primary school. Even though the INPRES program targeted primary schools only, effects for men continue through lower and higher secondary education at 2.3 and 2.6 percentage points. 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. The school construction program did not affect tertiary education completion rates. Literacy rates are high on average at 95 percent for men and

[14] Educational outcomes are recorded for household members aged five and older, and are missing otherwise.

91 percent for women, and the program raised these further by 1.5 and 3.3 percentage points, respectively.

Figure 2 breaks the education effects further down to show the treatment effects 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 being in school for at least one year with 0.95 percentage points for men and 2.3 percentage points for women. This figure confirms the patterns shown in Appendix Table 1 that the education results for women are concentrated in primary school, while for men they extend throughout lower and upper secondary education.

4.2. The effect of school construction on long-run labor market and demographic outcomes

Having observed large increases in education in response to the INPRES school construction program, Table 1 studies subsequent outcomes on employment, migration, expenditure and demographics.[15] The Work/Migration index in Figure 1 is comprised of seven variables listed in Appendix Table 1. Most notably, and as shown in Table 1, men are 0.6 percentage points more likely to be employed, while the effect for women is twice as small and insignificant.[16] On average, 95 percent of men are employed, compared to only 64 percent of women. In response to the treatment, 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. This represents a significant increase considering that, on average, 33 percent of men work in the formal sector. They furthermore move away from agricultural work, which

[15] For the remainder of our analysis, we continue using the OLS regression specified in Equation (1). 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. 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 circa 0.25 years.

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

they are 1.2 percentage points less likely to hold, compared to 43 percent on average. Labor market effects for women are mostly absent, although we observe a 1.1 percentage point increase in the likelihood that they are self-employed in their own microenterprise (Appendix Table 1).

On average, 28 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 point respectively, as shown in column 4 of Table 1. Appendix Table 1 indicates that the increase in migration is concentrated in shorter distance moves within—rather than between—provinces.

The third index in Figure 1 captures rich expenditure data included in the 2016 Susenas that is collected at the household level.[17] It shows an overall increase of 3.4 percent for men and 4.6 percent for women across the following five expenditure categories: total expenditure, food expenditure, non-food expenditure, the share of total expenditure that is spent on food, and expenditure on education. Specifically, column 5 of Table 1 shows that households in which the man was treated experience a 2.1 percent increase in total expenditure and household in which the woman was treated increase total expenditure by 3.2 percent. The increase is larger for non-food expenditure than for food expenditure (Appendix Table 1). 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 (column 6).

Column 7 studies detailed data on taxes paid in an average month and shows that the total amount of taxes paid increases by 7.8 percent if the father was exposed to the school

[17] 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.

construction program, and by 12.3 percent if the mother was exposed.[18] This may have important implications for the cost effectiveness of the school construction program, which we will analyze in more detail in the next section. Total tax expenditures are comprised of a rich set of tax data that is analyzed in more detail in Appendix Table 1, revealing increases in land and building taxes, taxes on motorized and non-motorized vehicles, local community taxes, and income tax.

In order to approximate household wealth, column 8 studies the impact of school construction on an appliance index that is created as a principle components index over household ownership of durable appliances.[19] Across these categories, the school construction program leads to a 3.0 percent increase if the father is treated and 4.0 percent increase if the mother is treated.

Column 9 of Table 1 shows large increases in preventative health expenditure of 24 percent if the father is treated and 19 percent if the mother is treated. Recall from Figure 1, however, that we did not observe significant improvements in self-reported health.

Finally, column 10 shows an 18 percent increase in the years of education of the husband's spouse and a 12 percent increase for the wife's spouse. 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.

4.3. Second generation impacts of school construction

After observing large and long-term effects of Indonesia's school construction program on a wide range of outcomes, including educational attainment, labor market outcomes, expenditure

[18] All tax values refer to average monthly values measured in 10,000 Indonesian rupiah (IDR).
[19] Asset index is a PCA index of ownership of air conditioner, boat, motorized boat, car, motorcycle, home phone, computer/laptop, flat-screen TV (≥ 30"), gold/jewelry (≥ 10 g), refrigerator, water heater, and LPG gas tube (≥ 5.5 kg).

and taxes, this section investigates whether the effects extend to the next generation by affecting the children of those exposed to the program.

As explained in Section 4, second generation impacts are measured using the same difference-in-differences framework as first generation effects, so 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 parents household are considered and age fixed effects are included to ensure comparisons take place across children of the same age.

Figure 1 already showed improvements in the education index that combines information from five education variables. Appendix Table 1 shows the treatment effects of these five variables separately. Overall, the number of years of education goes up by 9.7 percent for boys and by 16.9 percent for girls. Unlike the first generation education results, no effects are observed at the primary school level, presumably because primary school has become more universal by the time the second generation children attend school. Also unlike the first generation education results, increases in educational attainment now extend to tertiary education, especially for girls who are 0.8 percentage point more likely to have completed tertiary education, compared to a 0.4 percentage point increase for boys.

The education results are broken down by the likelihood of completing at least a certain number of years of education in Figure 3. This figure is the second generation equivalent of Figure 2, and not only can we compare results by the gender of the child, but also by the gender of the parent who was exposed to the program. This figure confirms that treatment effects are small and indistinguishable from zero during primary education. Then, they extend from the start of lower secondary education to completion of most tertiary education at 16 years of education.

While we cannot statistically distinguish the results by gender of the parent or child, there is suggestive evidence that the effects of school construction are highest for daughters when the mother was exposed to the program and lowest for sons when the father was exposed.

We investigate these gender dynamics further in Table 2. The five outcome variables in this tables are those used for the education index in Figure 1 and across all these variables, we see large and statistically significant increases in education if the mother is treated, but not when the father is treated. We test whether these coefficients are different from each other by showing the p-value of equality of coefficients and these reveal that indeed the increase in education attainment is larger when the mother was exposed to the INPRES school construction program compared to when the father was exposed.

5. Conclusion

This paper studies the long-run and intergenerational effects of one of the largest school construction programs in history. We use the difference-in-differences estimation strategy, proposed by Duflo (2001), exploiting variation across birth cohorts and regions in the number of schools built. We combine this with nationally representative data from Susenas 2016 that contains data on a wide range of outcomes related to education, employment, migration, expenditure, taxes and demographics.

We find that men and women exposed to the program attain more education, although women's effects are concentrated in primary school. As adults, men who received more education are more likely to be formal workers and work in a non-agricultural sector. Households in which either parent received more education have higher consumption, more assets, and pay more government taxes. These education benefits are transmitted to the next generation. Increased parental education has larger impacts for daughters, particularly if the mother was exposed to the school construction program.

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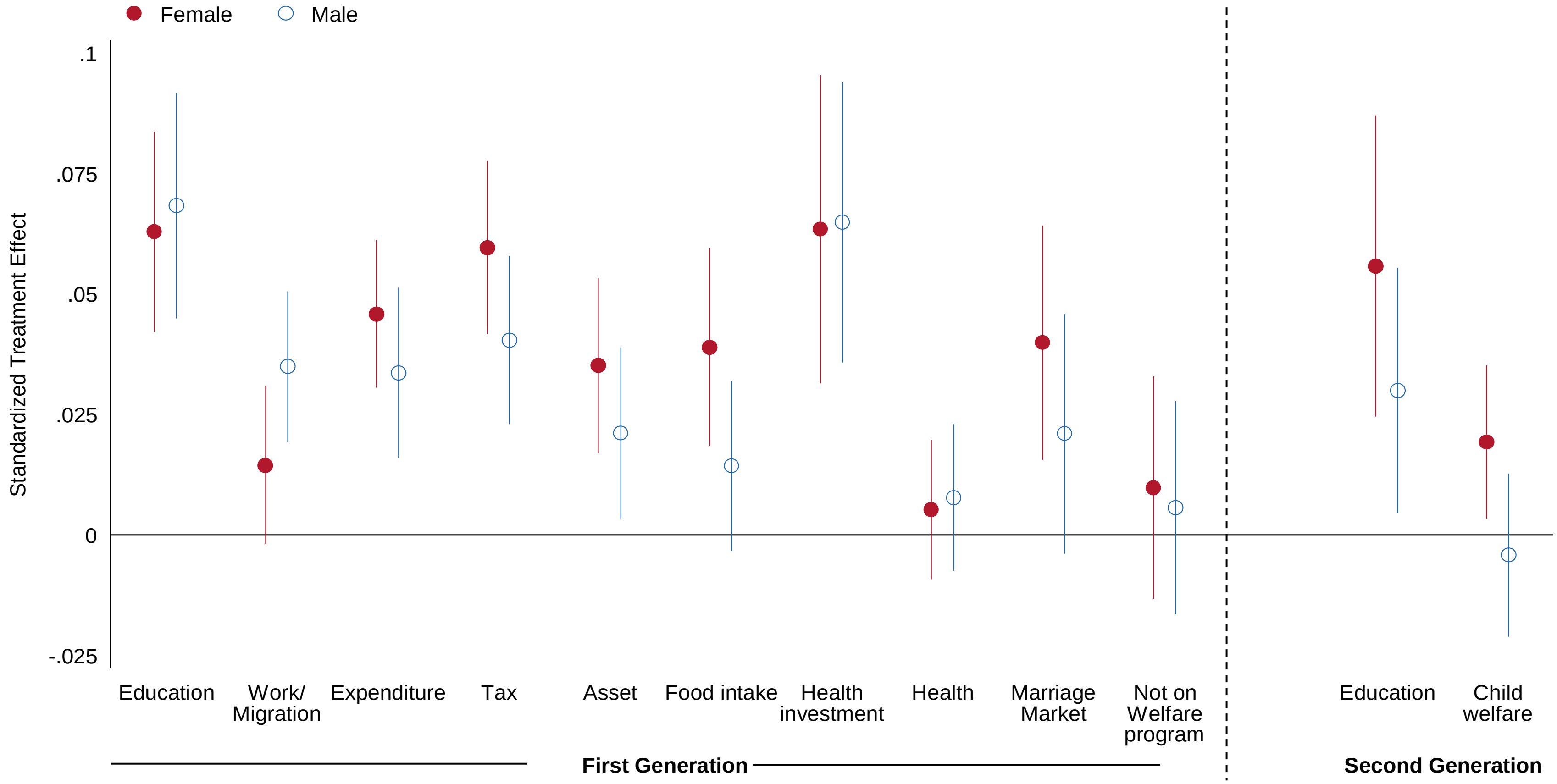


Figure 1. Effect of school construction on indexes

Notes: Following Kling et al. (2007), we define indexes for families of outcomes by, first, defining the z-scores for each outcome in its family relative to the control group (defined 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 normalize the Kling indexes by converting them into z-scores relative to the mean and standard deviation of the control group (defined as the old cohort in the low program intensity regions). We present estimated regression coefficients and their respective 95% confidence intervals. The outcomes making up the outcomes in each family are listed in Appendix Table 1.

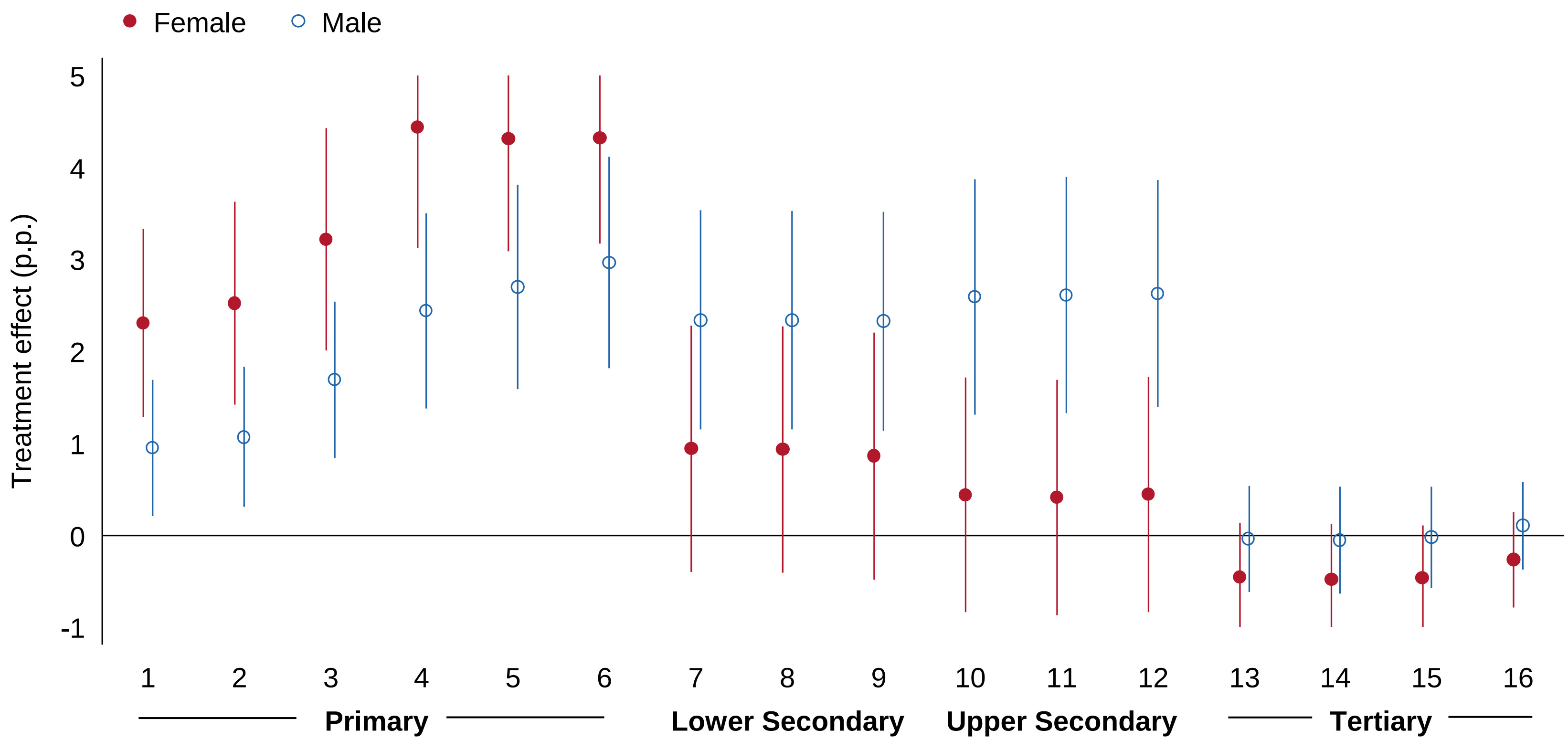


Figure 2. Effect of school construction on the probability of first generation individual attending at least *n*-years of schooling

Notes: Treatment effects measure the effect of one additional school constructed 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.

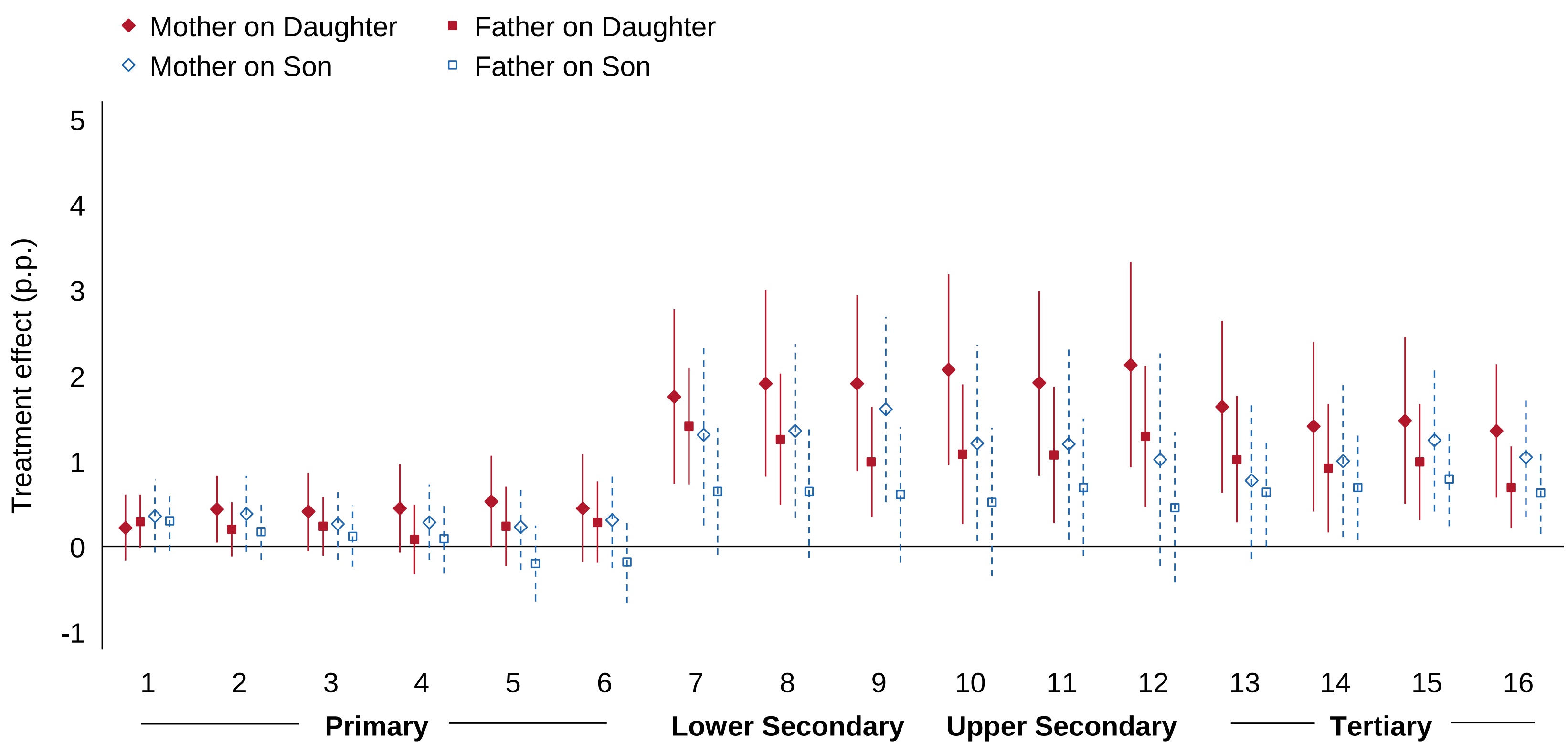


Figure 3 Effect of school construction on the probability of second generation individual attending at least *n*-years of schooling

Notes: Treatment effects measure the effect of one additional school constructed in the parent's birth district on the probability of second generation individual attending at least *n*-years of schooling, in percentage points. Each dot represents a coefficient in an independent regression, i.e. parents' treatments are regressed separately. We show estimated regression coefficients and their respective 95% confidence intervals.

Table 1. Effect of school construction on first generation's employment, migration, expenditure, and demographics

	(1) Work	(2) Formal worker	(3) Non-agricultural sector	(4) Migrant	(5) Total expenditure	(6) Education expenditure	(7) Tax expenditure	(8) Appliance index	(9) Preventive health expenditure	(10) Spouse's education
Panel A: Male										
Schools constructed * Young cohort	0.006**	0.011***	0.012***	0.007**	0.021***	0.160**	0.078***	0.030*	0.242***	0.180***
	(0.003)	(0.004)	(0.005)	(0.003)	(0.007)	(0.064)	(0.017)	(0.017)	(0.068)	(0.046)
Observations	72,367	68,574	68,574	72,367	68,687	68,687	68,687	68,687	68,687	64,422
Mean	0.948	0.327	-0.440	0.273	391.649	13.971	4.749	-0.035	0.744	7.635
FDR q-value	0.0565	0.0320	0.0320	0.0565	0.0241	0.0474	0.0000	0.0743	0.0032	0.0007
Panel B: Female										
Schools constructed * Young cohort	0.003	-0.005	0.002	0.008**	0.032***	0.193**	0.123***	0.040**	0.193***	0.116***
	(0.005)	(0.005)	(0.005)	(0.003)	(0.007)	(0.076)	(0.019)	(0.015)	(0.071)	(0.043)
Observations	71,423	45,560	45,560	71,423	66,249	66,249	66,249	66,249	66,249	55,468
Mean	0.638	0.236	-0.453	0.245	375.616	12.202	4.552	-0.069	0.671	7.426
FDR q-value	0.6536	0.6536	0.6536	0.0950	0.0000	0.0547	0.0000	0.0547	0.0498	0.0498

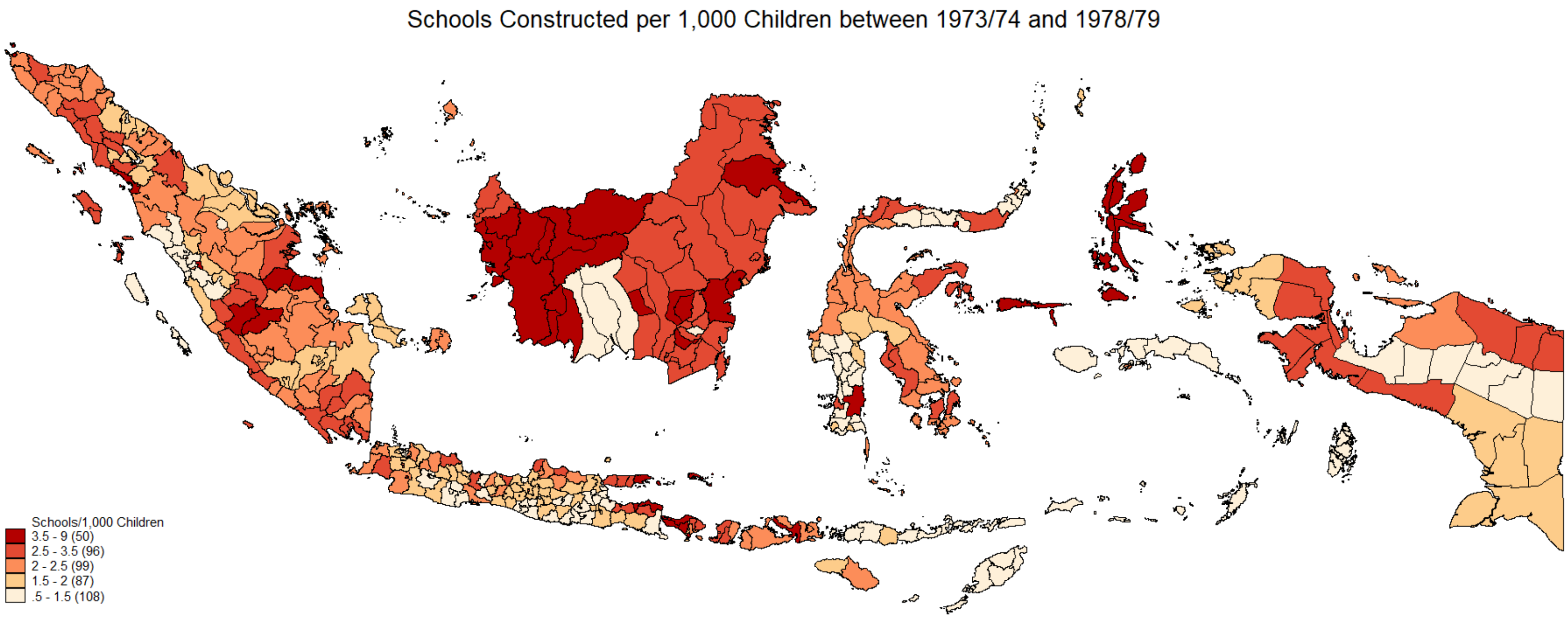
Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$ refers to the regular p-value. FDR q-value reports the false discovery rate adjusted p-value for multiple testing correction across columns within panel and table. An FDR q-value of 0.05 implies that 5% of *significant* tests will result in false positives. Robust standard errors in parentheses, clustered at the birth district level. The sample is restricted to male (Panel A) and female (Panel B) born in the sample period: 1957-1962 (old cohort) and 1968-1972 (young cohort). Columns 1 and 4 include all individuals, columns 2 and 3 are restricted to working individuals, columns 5-9 are restricted to the head of the household and his spouse, column 10 is restricted to individuals with a co-resident spouse. Schools constructed indicates the number of INPRES primary schools constructed in one's birth district between 1973/74 and 1978/79 per 1,000 children. Young cohort is an indicator defined as 1 for being born between 1968-1972. Work is an indicator defined as 1 for working in the past week or having an occupation but being temporarily absent from work in the past week. Formal worker is an indicator for working as an employee, conditional on work. Non-agricultural sector is an indicator defined as 1 for working in a sector outside of agriculture; it is missing for non-working individuals. Migrant is defined as 1 if the current district of residence is not the same as the individual's birth district. All expenditure values refer to the household's average monthly expenditure; means are reported in 10,000 Indonesian Rupiah (IDR) increments. In 2016, the average daily exchange rate was $1 USD=13,308 IDR. In the regression, we define an inverse hyperbolic sine transformation to the nominal values since expenditure data tends to be skewed and a log transformation would not be defined for zero expenditures. Appliance index is a PCA index over ownerships of appliances (see Appendix Table 1 for more details). Spouse's education refers to the spouse's completed years of schooling.

Table 2. Marginal effect of parents' schooling treatment on children's education

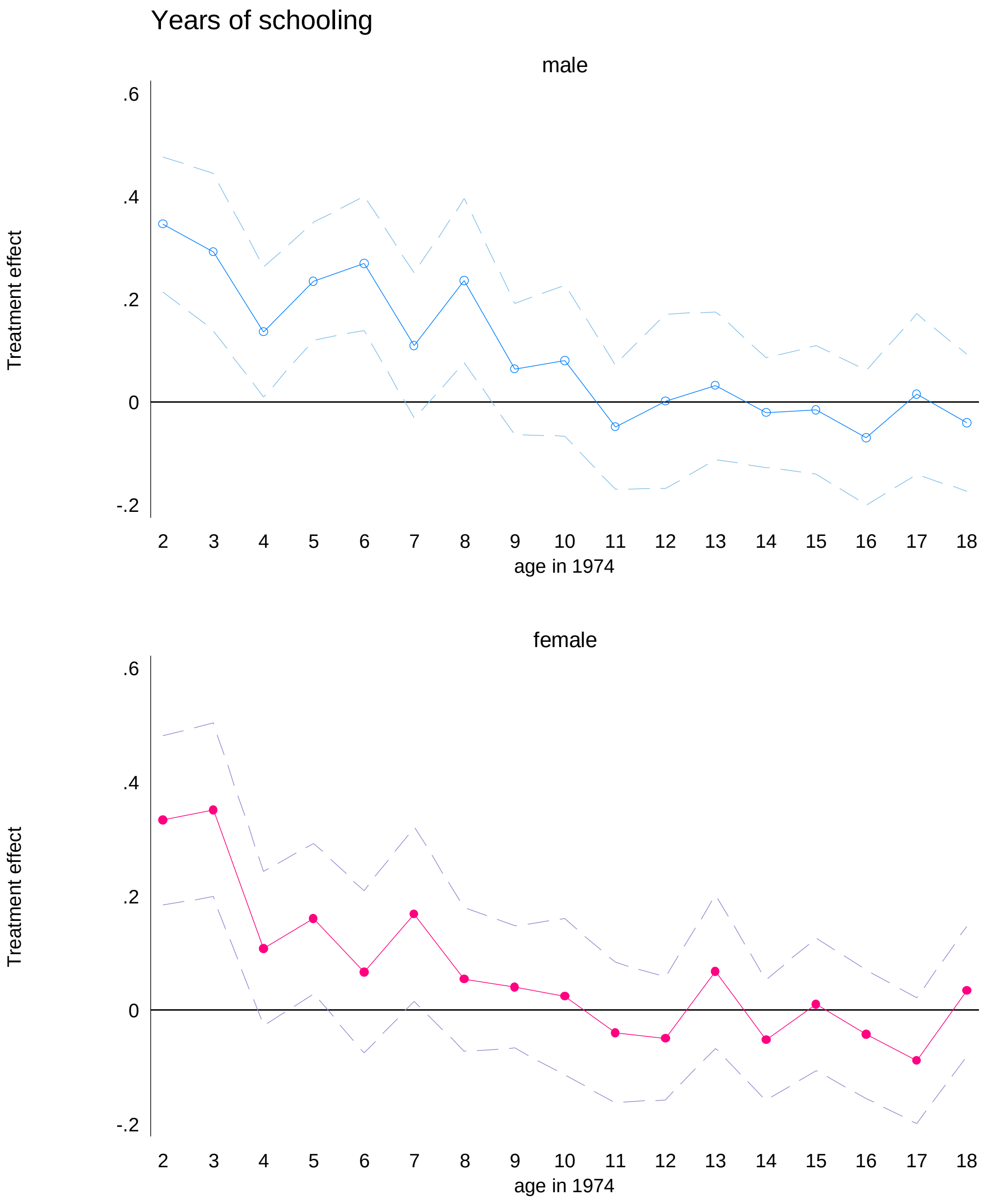
	(1) Years of schooling	(2) Completed Primary	(3) Completed Lower Sec.	(4) Completed Upper Sec.	(5) Completed Tertiary
Father's treated	-0.023	-0.007	-0.004	-0.003	-0.001
	(0.065)	(0.006)	(0.008)	(0.006)	(0.004)
Mother's treated	0.272***	0.016**	0.021**	0.019**	0.012**
	(0.085)	(0.007)	(0.010)	(0.009)	(0.005)
Father = Mother	0.017	0.040	0.082	0.071	0.099
Observations	44,105	44,105	44,105	44,105	44,105
Mean	8.674	0.703	0.481	0.276	0.057

Notes: Sample is restricted to children in households whose father and mother are born in the sample period: 1957-1962 (old cohort) and 1968-1972 (young cohort). Robust standard errors are clustered at the father and mother's birth districts; two-way clustering method follows Cameron, Gelbach, and Miller (2006). Father's treated indicates the interaction of the number of INPRES primary schools constructed in the father's birth district and an indicator that the father is a young cohort. Mother's treated is defined similarly. Father = Mother indicates the p-value of testing the equality of coefficients of father's and mother's treated.

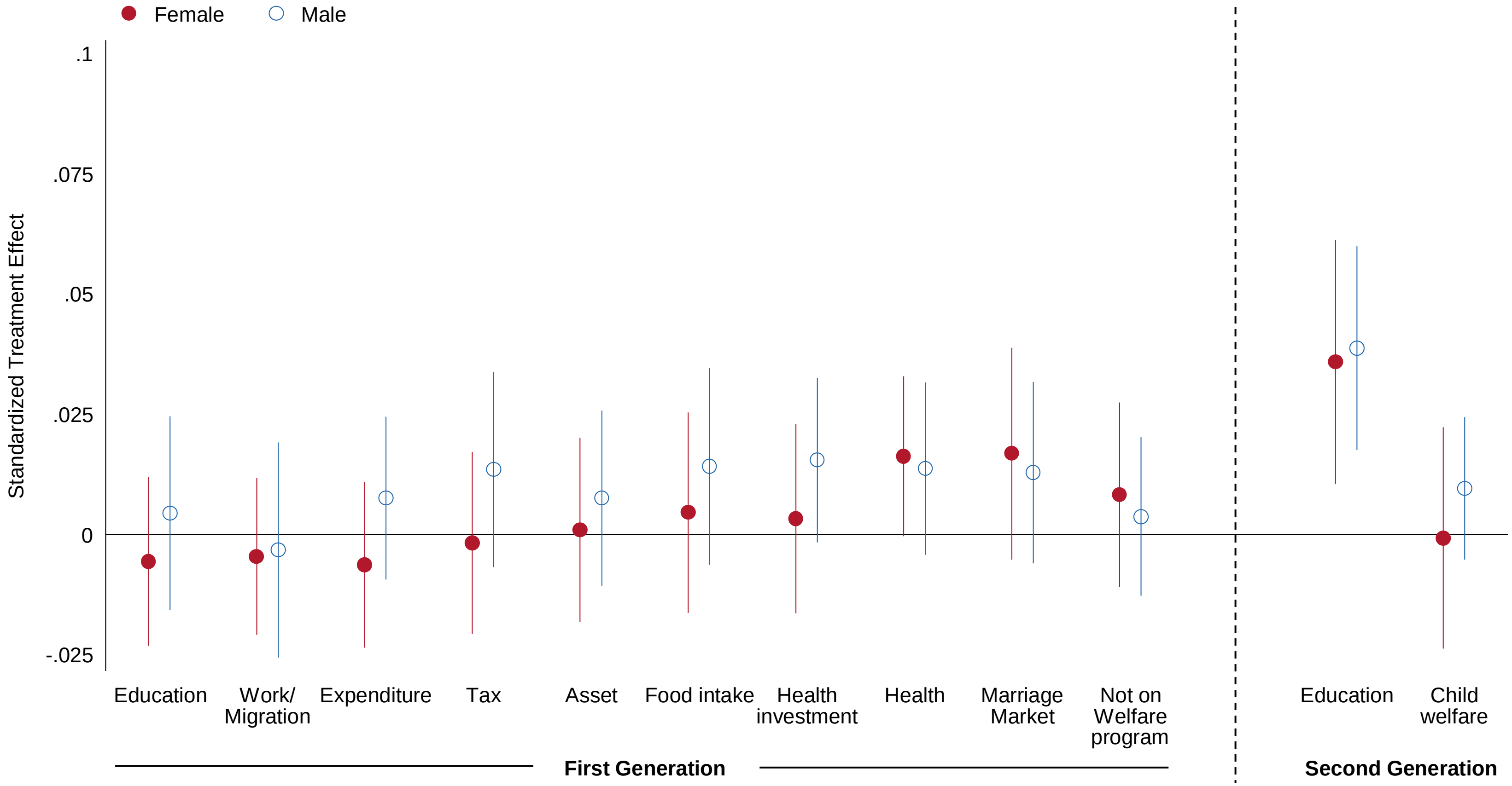
Online Appendix



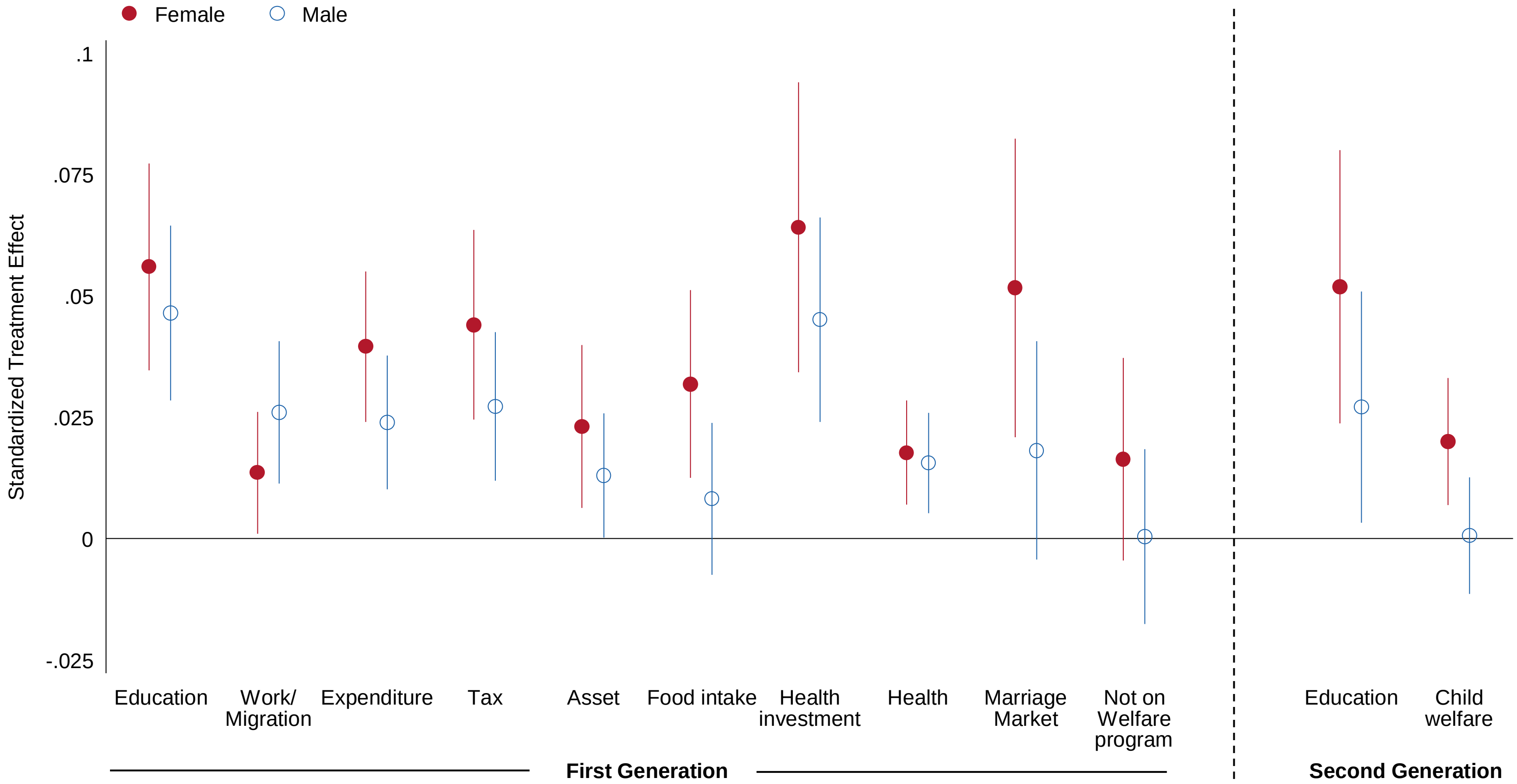
Appendix Figure 1. Spatial distribution of schools constructed per 1,000 children between 1973/74 and 1978/79



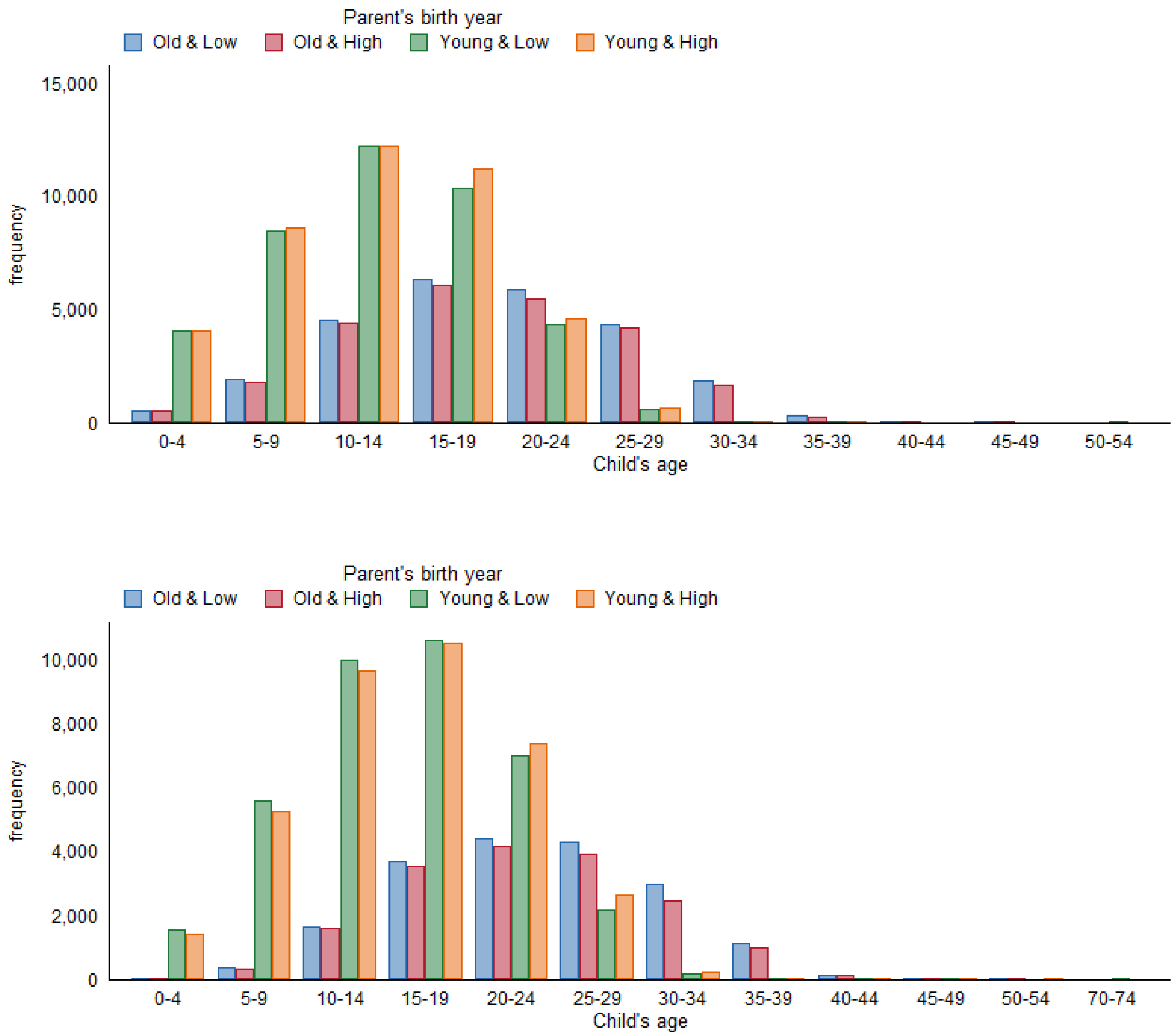
Appendix Figure 2. Effect of school construction on first generation individual's years of schooling by age in 1974 (relative to omitted pool of age 19-24)



Appendix Figure 3. Placebo effect of school construction on individuals too old to benefit from primary school construction



Appendix Figure 4. Effect of school construction extending the sample experiment to include all individuals born between 1950 and 1980



Appendix Figure 5. Child's age distribution by parents' treatment group (top panel: father; bottom panel: mother)

Appendix Table 1. Description of variables used in summary indexes and their treatment effects

Outcome	Description	Mean / SD Men	Mean / SD Women	Treatment effect on: Men	Treatment effect on: Women
<u>First Generation</u>					
Education					
Years of schooling	Based on highest education level and grade attended. Standard durations of study are assumed; grade retentions are not counted	8.075	7.153	0.268***	0.234***
		(4.244)	(4.233)	(0.047)	(0.042)
Completed Primary	Indicator taking a value of 1 if highest diploma completed is higher than or equal to Primary	0.815	0.729	0.026***	0.041***
		(0.388)	(0.444)	(0.006)	(0.006)
Completed Lower Secondary	Indictor taking a value of 1 if highest diploma completed is higher than or equal to Lower Secondary	0.392	0.318	0.023***	0.008
		(0.488)	(0.466)	(0.006)	(0.007)
Completed Upper Secondary	Indicator taking a value of 1 if highest diploma completed is higher than or equal to Upper Secondary	0.343	0.266	0.026***	0.005
		(0.475)	(0.442)	(0.006)	(0.006)
Completed Tertiary	Indicator taking a value of 1 if highest diploma completed is higher than or equal to Primary	0.097	0.079	-0.001	-0.003
		(0.296)	(0.270)	(0.003)	(0.003)
Literate	Literacy is a binary outcome and is self-reported	0.954	0.910	0.015***	0.033***
		(0.211)	(0.286)	(0.004)	(0.006)
Work/ Migration					
Work	Work is a binary outcome, defined as "1" for individuals who worked in the past week or those who have an occupation but were temporarily absent from work in the past week. "0" otherwise	0.947	0.635	0.006**	0.003
		(0.225)	(0.481)	(0.003)	(0.005)
Work hours	Hours worked in the past week conditional on working, i.e. missing for non-working individuals	41.051	36.275	0.258	0.157
		(17.125)	(18.805)	(0.158)	(0.208)
Formal worker	Indicator defined as 1 if individuals reported working as an employee as opposed to being self-employed, family/unpaid work and freelance work, conditional on working	0.331	0.238	0.011***	-0.005
		(0.471)	(0.426)	(0.004)	(0.005)
Self-employed	Indicator defined as 1 for working as a self-employed in a micro-enterprise, i.e. without any employees, conditional on work	0.253	0.234	-0.006	0.011***
		(0.435)	(0.424)	(0.004)	(0.004)
Non-agriculture sector	Indicator defined as 1 for working in a sector outside of agriculture; conditional on working	-0.434	-0.449	0.012***	0.002
		(0.496)	(0.497)	(0.005)	(0.005)
Service sector	Indicator for working in trade, hotel, or restaurant, transportation; warehousing, information, or communication; finance and insurance; and service sectors, conditional on working	0.370	0.463	0.010***	-0.000
		(0.483)	(0.499)	(0.004)	(0.006)
Migrant	Indicator defined as 1 if the current district of residence is not the same as the individual's birth district	0.275	0.248	0.007**	0.008**
		(0.447)	(0.432)	(0.003)	(0.003)
Local migration	Indicator defined as 1 if migration occurred within the individual's birth province	0.107	0.104	0.005*	0.005**
		(0.309)	(0.306)	(0.003)	(0.003)
Expenditures					
Total (Rp10k)	All expenditure values refer to the household's average monthly expenditure; means are reported in 10,000 Indonesian Rupiah (IDR) increments. In 2016, the average daily exchange rate was 1 USD for 13,308 IDR. In the regression, we define an inverse hyperbolic sine transformation to the nominal values since expenditure data tends to be skewed and a log transformation would not be defined for zero	15.645	15.577	0.021***	0.032***
		(0.665)	(0.714)	(0.007)	(0.007)
Food (Rp10k)		15.015	14.934	0.014**	0.028***
		(0.580)	(0.635)	(0.007)	(0.007)
Non-food (Rp10k)		14.793	14.737	0.027***	0.039***
		(0.841)	(0.883)	(0.008)	(0.008)

	expenditures. Total expenditures are divided into two groupings: food and non-food.				
Non-food/Total (%)	Share of total monthly non-food expenditures divided by the household's total monthly expenditures	44.716	45.291	0.287***	0.237**
		(13.435)	(13.827)	(0.110)	(0.102)
Education (Rp10k)	Education expenditures fall under non-food expenditures and include admission, tuition, and extracurricular fees, textbooks, stationeries, and tutoring	8.627	7.343	0.160**	0.193**
		(5.701)	(6.095)	(0.064)	(0.076)
Tax expenditure					
Total (Rp10k)	Tax expenditures fall under non-food expenditures and include the following components	10.373	10.265	0.078***	0.123***
		(1.885)	(1.943)	(0.017)	(0.019)
Land & building (Rp10k)	Tax on land and/or building ownerships	7.824	7.948	0.041*	0.075***
		(2.287)	(2.166)	(0.022)	(0.021)
Vehicle (Rp10k)	Motorized and non-motorized vehicle license fees	8.481	7.963	0.154***	0.267***
		(4.536)	(4.848)	(0.047)	(0.052)
Local (Rp10k)	Levies/retributions; examples include: neighborhood/citizen associations, garbage, security, cemetery, parking, fees	3.643	3.697	0.048	0.082**
		(4.606)	(4.620)	(0.033)	(0.039)
Other (Rp10k)	Other non-specified taxes; examples include: vehicle citations, income, taxes	0.294	0.283	0.043***	0.030**
		(1.719)	(1.689)	(0.012)	(0.013)
Asset					
Rent equivalent (Rp10k)	Actual monthly rent, if house is rented, or estimated rent value if house is owned or leased by the employer	13.250	13.238	0.012	0.028***
		(0.926)	(0.941)	(0.008)	(0.008)
Floor area (sq.-m)	House's floor area in squared meters	79.972	81.562	1.229**	1.480***
		(59.020)	(60.175)	(0.566)	(0.510)
Utilities (Rp10k)	Expenditure on electricity, water, gas, and kerosene	11.965	11.988	0.051**	0.085***
		(2.124)	(1.995)	(0.022)	(0.024)
Appliance index	PCA index on binary ownerships of gas tube, fridge, air conditioner, water heater, home phone, computer, jewelry, motorcycle, boat, motorized boat, car, and TV	-0.013	-0.044	0.030*	0.040**
		(1.891)	(1.909)	(0.017)	(0.015)
Urban	Indicator for residing in an urban area	0.433	0.445	-0.001	0.002
		(0.496)	(0.497)	(0.004)	(0.004)
Food intake					
Calorie	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 multiply the weekly intake by 30/7 to get the monthly intake. In the regression, we similarly apply an inverse hyperbolic transformation for reasons discussed above. The mean of calorie intake is reported in 1 kcal increments. The means of protein, fat, and carbohydrate intakes are reported in 1 kg increments.	13.084	13.027	0.005	0.018***
		(0.414)	(0.447)	(0.004)	(0.005)
Protein		9.465	9.411	0.006	0.018***
		(0.459)	(0.488)	(0.005)	(0.005)
Fat		9.278	9.221	0.011**	0.023***
		(0.532)	(0.554)	(0.004)	(0.006)
Carbohydrate		11.220	11.159	0.005	0.017***
		(0.435)	(0.470)	(0.004)	(0.005)
Health investment					
Total health expenditure	Total monthly household health expenditure, which aggregates curative, medicine, and preventive health expenditures	9.457	9.422	0.071*	0.055
		(3.623)	(3.654)	(0.038)	(0.041)
Preventive measures	Consist of pregnancy checks, immunizations, medical check-ups, family planning, and other preventive health expenditures, such as, vitamins, massage, gym memberships	4.419	3.828	0.242***	0.193***
		(4.921)	(4.822)	(0.068)	(0.071)
Private hospital		0.784	0.804	0.048**	0.075***

	A sub-category under curative health expenditures and is distinct from expenditures on public hospitals, clinics, and traditional healers	(3.024)	(3.060)	(0.023)	(0.024)
Family planning	A sub-category under preventive health expenditures, which includes costs of contraceptives and consultations	2.858 (4.389)	2.161 (4.009)	0.321*** (0.061)	0.226*** (0.071)
Health insurance	Health insurance is distinct from life, accidental, vehicle, and house insurances	4.528 (5.604)	4.602 (5.599)	0.083 (0.055)	0.142*** (0.048)
Health outcomes					
No health complaint	Self-reported indicator taking the value of 1 if did not experience a health complaint in the past month	-0.310 (0.462)	-0.354 (0.478)	0.004 (0.004)	0.003 (0.004)
No disruption	Self-reported indicator taking the value of 1 if did not experience a health complaint that disrupted daily activities in the past month	-0.163 (0.369)	-0.175 (0.380)	0.003 (0.003)	0.002 (0.003)
Fewer disruption (days)	Self-reported number of days that a health complaint disrupted daily activities. Signs are reversed; i.e. positive treatment effect means a reduction in duration and the mean is reported in negative	-0.009 (0.093)	-0.011 (0.106)	-0.001* (0.001)	0.001 (0.001)
No severe health complaint	Self-reported indicator taking the value of 1 if did not experience a severe health complaint in the past month	-0.049 (0.216)	-0.051 (0.220)	0.005*** (0.002)	-0.001 (0.002)
Marriage market					
Age of first marriage	Age of first marriage for ever-married household members	25.246 (5.025)	20.921 (4.800)	0.058 (0.053)	0.050 (0.059)
Spouse's education	Spouse's years of schooling is defined only for household head and spouse because the survey does not identify the spouse of other household members and is missing for non-coresident spouse	7.679 (4.094)	7.479 (4.213)	0.180*** (0.046)	0.116*** (0.043)
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)	0.010** (0.004)
Fewer children 0-14	Indicates the number of children aged 0-14 living in the household. Signs are reversed	-0.912 (1.059)	-0.559 (0.867)	0.012 (0.017)	0.035** (0.016)
Not a recipient of welfare programs:					
Cash Transfer	*Bantuan Langung Tunai (BLT)* is an unconditional cash transfer intended to compensate for the removal of gas price subsidy	-0.040 (0.197)	-0.039 (0.194)	0.002 (0.002)	0.001 (0.002)
Rice for Poor	*Beras Miskin (Raskin)/ Beras Sejahtera (Rastra)* provide monthly rice allowance for poor households	-0.388 (0.487)	-0.402 (0.490)	-0.002 (0.004)	0.009* (0.005)
Poor Student's Assistance	*Bantuan Siswa Miskin (BSM)* is a cash transfer conditional on school enrollment	-0.155 (0.362)	-0.126 (0.332)	0.001 (0.004)	-0.000 (0.004)
Social Protection Card	Provided to poor households, which entitles them to social welfare programs, such as: *Raskin*, *BSM*, education and health subsidies	-0.184 (0.388)	-0.179 (0.383)	0.001 (0.004)	0.000 (0.004)
<u>Second Generation</u>					
Education					
Years of schooling	Same as above.	7.980 (4.345)	8.876 (4.282)	0.097*** (0.032)	0.169*** (0.045)
Completed Primary	Same as above.	0.638 (0.481)	0.729 (0.445)	0.000 (0.002)	0.001 (0.003)
Completed Lower Secondary	Same as above.	0.414 (0.493)	0.506 (0.500)	0.006* (0.003)	0.015*** (0.005)
	Same as above.	0.219	0.303	0.009**	0.014***

Completed Upper Secondary		(0.414)	(0.459)	(0.004)	(0.005)
Completed Tertiary	Same as above.	0.041	0.065	0.004*	0.008**
		(0.199)	(0.246)	(0.002)	(0.003)
Age-for-grade	Indicator for starting primary school at most by age 7 and never repeating school up to Upper Secondary education.	0.837	0.792	0.011***	0.018***
		(0.369)	(0.406)	(0.004)	(0.005)
Child welfare					
Fewer work days	Number of days worked in the past week unconditional on work, i.e. 0 for non-working individuals. Signs are reversed.	-1.678	-2.178	0.044**	0.031
		(2.667)	(2.864)	(0.021)	(0.019)
Fewer work hours	Number of hours worked in the past week unconditional on work, i.e. 0 for non-working individuals. Signs are reversed.	-11.307	-14.974	0.299*	0.215
		(19.703)	(21.621)	(0.157)	(0.151)
No health complaint	Same as above.	-0.204	-0.178	-0.008***	0.004
		(0.403)	(0.382)	(0.003)	(0.003)
No disruption	Same as above.	-0.114	-0.096	-0.002	0.003
		(0.317)	(0.294)	(0.002)	(0.002)
Fewer disruption (days)	Same as above.	-0.011	-0.010	-0.001	0.000
		(0.106)	(0.098)	(0.001)	(0.001)
No severe health complaint	Same as above.	-0.022	-0.020	-0.000	-0.000
		(0.147)	(0.140)	(0.001)	(0.001)

Notes: There are 72,367 male and 71,423 female individuals in our sample. For household-level outcomes, i.e. same outcome shared by all household members, e.g. expenditures, asset, food intake, we use observations from 68,687 fathers and 66,249 mothers. The average age of our first generation sample is 49.90 and the average household size is 4.06. The headings correspond to the summary indexes in Figure 1 and the listed outcomes refer to the outcomes used to construct the summary index.

Appendix Table 2. Effect of school construction on first generation's education (extended cohort definitions)

	(1)	(2)	(3)	(4)	(5)
Sample	1957-1962 and 1968-1972	… + 1950-1956	… + 1963-1967	… + 1973-1980	1950-1980
Panel A: Male					
Schools constructed *	0.268***	0.267***	0.221***	0.211***	0.172***
Young cohort	(0.047)	(0.039)	(0.037)	(0.044)	(0.032)
Observations	72,367	98,895	98,781	138,617	197,951
Mean	8.022	7.500	7.938	8.478	8.047
Panel B: Female					
Schools constructed *	0.234***	0.219***	0.209***	0.245***	0.210***
Young cohort	(0.042)	(0.044)	(0.039)	(0.044)	(0.045)
Observations	71,423	97,268	99,843	140,142	200,644
Mean	7.105	6.496	6.901	7.790	7.194

Notes: * p<0.10, ** p<0.05, *** p<0.01 refers to the regular p-value. Robust standard errors in parentheses, clustered at the birth district level. Column (1) sample is restricted to Duflo (2001)'s original definition, i.e. individuals born in the sample period: 1957-1962 (old cohort) and 1968-1972 (younger cohort). Column (2) to (5) extend the sample as indicated in the column title. Panel A looks only at men and Panel B only at women. INPRES school constructed per 1,000 children denotes the continuous number of INPRES schools constructed per 1,000 children population in one's birth district. Young cohort is an indicator defined as 1 for being born after 1967.

Appendix Table 3. Marginal effect of parents' schooling treatment on children's education on original and extended cohort, separate gender

Born between:	1957-1962 and 1968-1972					1950-1980				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Years of schooling	Completed Primary	Completed Lower Sec.	Completed Upper Sec.	Completed Tertiary	Years of schooling	Completed Primary	Completed Lower Sec.	Completed Upper Sec.	Completed Tertiary
Panel A: Sons										
Father's treated	-0.050	-0.009	-0.013	-0.008	-0.000	0.061	0.003	0.001	0.004	0.000
	(0.083)	(0.008)	(0.011)	(0.009)	(0.004)	(0.049)	(0.005)	(0.005)	(0.004)	(0.002)
Mother's treated	0.195**	0.011	0.024**	0.008	0.005	0.174***	0.006	0.018***	0.008	0.001
	(0.098)	(0.009)	(0.012)	(0.011)	(0.006)	(0.060)	(0.005)	(0.007)	(0.006)	(0.003)
Father = Mother	0.108	0.178	0.054	0.369	0.501	0.122	0.691	0.043	0.532	0.800
Observations	24,366	24,366	24,366	24,366	24,366	133,896	133,896	133,896	133,896	133,896
Mean	8.575	0.708	0.480	0.271	0.045	7.787	0.629	0.402	0.211	0.034
Panel B: Daughters										
Father's treated	0.007	-0.003	0.004	0.002	-0.004	0.052	0.001	0.003	0.002	0.000
	(0.104)	(0.010)	(0.013)	(0.010)	(0.006)	(0.053)	(0.005)	(0.005)	(0.005)	(0.002)
Mother's treated	0.362***	0.020*	0.021	0.036***	0.022***	0.214***	0.010**	0.021***	0.025***	0.010***
	(0.122)	(0.010)	(0.014)	(0.013)	(0.008)	(0.057)	(0.004)	(0.006)	(0.006)	(0.003)
Father = Mother	0.072	0.200	0.457	0.087	0.030	0.035	0.260	0.029	0.002	0.016
Observations	19,739	19,739	19,739	19,739	19,739	112,570	112,570	112,570	112,570	112,570
Mean	8.796	0.697	0.484	0.281	0.071	7.875	0.614	0.397	0.213	0.051

Notes: * p<0.10, ** p<0.05, *** p<0.01 refers to the regular p-value. FDR q-value reports the false discovery rate adjusted p-value for multiple testing correction across columns within panel and table. An FDR q-value of 0.05 implies that 5% of *significant* tests will result in false positives. Robust standard errors are clustered at the father and mother's birth districts; two-way clustering method follows Cameron, Gelbach, and Miller (2006). Father's treated indicates the interaction of the number of INPRES primary schools constructed in the father's birth district and an indicator that the father is a young cohort. Mother's treated is defined similarly. Father = Mother indicates the p-value of testing the equality of coefficients of father's and mother's treated within each panel. Panel A looks at the son's sample and Panel B at the daughter's sample. Years of schooling is a continuous variable denoting the number of years of education attended. Completed primary is an indicator defined as 1 for completing at least primary school diploma. Completed lower secondary, completed upper secondary, and completed tertiary are similarly defined.

Appendix Table 4. Mediators of the effect of school construction on second generation's years of schooling

Mediator:	(1) None	(3) Work/ Migration	(4) Health	(5) Marriage	(6) Expenditure	(7) Asset	(8) Food intake	(9) Health investment	(10) Tax	(11) All
Panel A: Father										
Schools constructed * Young cohort	0.097***	0.082***	0.096***	0.083***	0.077**	0.082***	0.095***	0.086***	0.082***	0.050*
	(0.032)	(0.031)	(0.032)	(0.030)	(0.030)	(0.028)	(0.032)	(0.031)	(0.030)	(0.027)
Mediator		0.373***	0.043***	0.493***	0.812***	0.772***	0.112***	0.165***	0.508***	
		(0.016)	(0.010)	(0.013)	(0.016)	(0.019)	(0.018)	(0.013)	(0.016)	
Observations	120,838	120,838	120,838	120,838	120,838	120,838	120,838	120,838	120,838	120,838
Mean	7.967	7.967	7.967	7.967	7.967	7.967	7.967	7.967	7.967	7.967
Panel B: Mother										
Schools constructed * Young cohort	0.169***	0.164***	0.168***	0.137***	0.120***	0.133***	0.163***	0.156***	0.139***	0.100**
	(0.045)	(0.045)	(0.045)	(0.044)	(0.042)	(0.039)	(0.045)	(0.043)	(0.042)	(0.040)
Mediator		0.343***	0.067***	0.486***	1.076***	1.037***	0.183***	0.189***	0.657***	
		(0.014)	(0.013)	(0.018)	(0.019)	(0.019)	(0.020)	(0.014)	(0.019)	
Observations	105,523	105,523	105,523	105,523	105,523	105,523	105,523	105,523	105,523	105,523
Mean	8.854	8.854	8.854	8.854	8.854	8.854	8.854	8.854	8.854	8.854

Notes: * p<0.10, ** p<0.05, *** p<0.01 refers to the regular p-value. Robust standard errors are clustered at the parent's birth district. Summary indexes are as defined in Figure 1 (see Appendix Table 1 for more details).

Appendix Table 5. Effect of school construction on first generation's household expenditures (various transformations)

	Total expenditure			Education expenditure		
	Log	Nominal	Per-capita	Log	Nominal	Per-capita
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Father						
Schools constructed *	0.021***	9.882***	0.016**	0.013	0.309	0.140**
Young cohort	(0.007)	(3.628)	(0.007)	(0.010)	(0.309)	(0.056)
Observations	68,687	68,687	68,687	48,123	68,687	68,687
Mean	391.649	391.649	391.649	13.971	13.971	13.971
FDR q-value	0.0206	0.0323	0.0831	0.3181	0.3181	0.0494
Panel B: Mother						
Schools constructed *	0.032***	11.022***	0.018***	-0.010	-0.191	0.167**
Young cohort	(0.007)	(2.583)	(0.007)	(0.014)	(0.383)	(0.067)
Observations	66,249	66,249	66,249	39,492	66,249	66,249
Mean	375.616	375.616	375.616	12.202	12.202	12.202
FDR q-value	0.0000	0.0001	0.0334	0.6173	0.6173	0.0378

Notes: * p<0.10, ** p<0.05, *** p<0.01 refers to the regular p-value. FDR q-value reports the false discovery rate adjusted p-value for multiple testing correction across columns within panel and table. An FDR q-value of 0.05 implies that 5% of *significant* tests will result in false positives. Robust standard errors in parentheses, clustered at the father/mother's birth district level. The sample is restricted to father/mother born in the sample period: 1957-1962 (old/control cohort) and 1968-1972 (young/treated cohort). Panel A looks at father sample and Panel B at mother sample. INPRES school constructed per 1,000 children denotes the continuous number of INPRES schools constructed per 1,000 children population at father or mother's birth district, in Panel A and B, respectively. Young cohort is an indicator defined as 1 for being born between 1968-1972. All expenditure values are defined at the household level and refer to the household's average monthly expenditure. Nominal values are reported in 10,000 Indonesian Rupiah (IDR) increments. In 2016, the average daily exchange rates was 1 USD for 13,308 IDR. Column (1)-(3) look at total household expenditure and column (4)-(6) look at education expenditure. Log transformations are applied to the total household expenditure in column (1) and (4). Inverse hyperbolic sine transformations are applied to the per-capita household expenditure in column (3) and (6).