Social interventions, health and wellbeing: The long-term and intergenerational effects of a school construction program*

Bhashkar Mazumder† Maria Fernanda Rosales‡ Margaret Triyana§

October 28, 2019

Abstract

We analyze the long-run and intergenerational effects of a large-scale school building project (INPRES) that took place in Indonesia between 1974 and 1979. Specifically, we link the geographic rollout of INPRES to longitudinal data from the Indonesian Family Life Survey covering two generations. We find that individuals exposed to the program have better health later in life along multiple measures. We also find that the children of those exposed also experience improved health and educational outcomes and that these effects are generally stronger for maternal exposure than paternal exposure. We find some evidence that household resources, neighborhood quality, and assortative mating may explain a portion of our results. Our findings highlight the importance of considering the long-run and multigenerational benefits when evaluating the costs and benefits of social interventions in a middle-income country.

Keywords: Intergenerational transmission of human capital, education, adult well-being

JEL Codes: J13, O15, I38

*We thank Esther Duflo for sharing the administrative school construction data. We are grateful to Prashant Bharadwaj, Taryn Dinkleman, Willa Friedman, Joe Kaboski, Marco Gonzalez-Navarro, Jean Lee, Elaine Liu, Grant Miller, Javaeria Qureshi, and seminar participants at the University of Notre Dame, Columbia and Princeton University Conference for Women in Economics, American Economic Association Annual Meetings, Population Association of the Americas Annual Meetings, NBER Development Meetings (Pre-conference), Millennium Challenge Corporation (MCC), LACEA-RIDGE conference and the NBER Summer Institute. We thank Leah Plachinski for excellent research assistance. We thank Dr. Amich Alhumami and Dr. Abdul Malik for their helpful insight on the school construction program.

†Federal Reserve Bank of Chicago and University of Bergen, bhash.mazumder@gmail.com

‡Rutgers University, maria.rosales@rutgers.edu

§Wake Forest University, mtriyana@alumni.uchicago.edu

1 Introduction

One of the fundamental ways that nations have tried to advance economic development has been through investments in human capital (Becker, 1964; Hanushek and Woessmann, 2015; Schultz, 1961). Many low and middle income countries have embarked on educational reforms such as school construction projects over the last fifty years in an effort to improve living standards. Indeed, one of the prime UN Millennium Development Goals, listed second only to eradicating extreme poverty and hunger, is to achieve universal primary education. While significant progress has been made towards this goal and low and middle income countries annually spend approximately one trillion dollars each year on primary schooling, inadequate access to schools and poor school quality remains a pervasive problem in much of the developing world (UNESCO, 2015; UNICEF, 2018). For example, primary schooling rates in Senegal and Ethiopia were still below 60 percent in 2015.

For the remaining countries in the developing world that still need to embark on large-scale school building initiatives, the potential societal gains may be significantly larger than current research on educational policies may suggest. There is now increasing recognition that in addition to improving purely economic outcomes, educational policies have the potential for producing important spillovers on other aspects of well-being such as health (Galama et al., 2018; Heckman et al., 2018; Oreopoulos and Salvanes, 2011). Furthermore, the human capital gains in one generation may also be transmitted to the next generation, producing future benefits that existing studies typically ignore. However, causal evidence of such spillovers and intergenerational effects are limited, particularly in developing countries (Wantchekon et al., 2014). A failure to properly consider these additional potential benefits could dramatically understate the value of social interventions designed to improve human capital.

We focus on these non-pecuniary and intergenerational spillovers by studying a massive primary school construction program in Indonesia known as the INPRES program. In particular, we examine the effects of this program on the long-term health of individuals who were exposed to these schools (first generation) and the human capital outcomes of their offspring (second generation). Under the INPRES program, the Indonesian government constructed 61,000 elementary schools between 1974 and 1979, doubling the existing stock of schools, thus making schools available to children where none or few had existed before. Our study builds upon earlier studies that have analyzed the effects of this program. Duflo (2001), Duflo (2004), Breierova and Duflo (2004) and Zha (2019) show that INPRES affected education, the marriage market, fertility, and labor-market outcomes of individuals exposed to these schools, while Martinez-Bravo (2017) documents the impacts on local governance and public good provision in Indonesia's main island, Java. As may be expected, the main goal of INPRES was to increase primary school attendance and in a companion paper (Mazumder et al., 2019), we show that there were in fact large effects of the program on primary school completion.

We build upon these studies and show that there were important spillovers on the long-term health of those exposed to INPRES as well as meaningful intergenerational effects. Specifically, we use longitudinal data from the Indonesian Family Life Survey (IFLS), which includes 5 waves between 1993 and 2014, and the Indonesian Family Life Survey-East (IFLS-E), which includes one wave in 2012. This data allow us to track the offspring of INPRES exposed individuals and examine important effects on the human capital outcomes of the next generation.[1] Like the earlier studies, we exploit variation in the rollout of INPRES across Indonesian districts. This enables us to utilize two sources of variation: 1) geographic variation from the intensity of primary schools built across districts; and 2) cohort variation by comparing individuals who were of primary school age or younger to those who were older than primary school age at the inception of the program. As far as we are aware, we are the first paper to use longitudinal data from the Indonesian Family Life Survey (IFLS) and the Indonesian Family Life Survey-East (IFLS-E) to explore these questions. The longitudinal aspect of the IFLS allows us to observe household members over time, including those who have split-off from the original household and formed new households. This enables us to observe outcomes for the next generation, *even if these children are no longer co-resident with their parents,* which would not be possible with cross-sectional data. Furthermore, the rich set of questions in IFLS allows us to track several very useful markers of health for both generations, as well as a range of human capital outcomes for the second generation.

Our first set of results document that there were significant and meaningful effects on the long-run health of individuals in the first generation. Specifically, we find that individuals exposed to INPRES have better self-reported health status, fewer depressive symptoms, and lower rate of self-reported chronic conditions 40 years after the program. We create a summary index of these health measures and show that health is improved by 0.03 to 0.06 standard deviations (SD) per school built per 1,000 children relative to the comparison group. These effects are even stronger for women.

Our second set of results examine the impacts on the children of cohorts exposed to

[1] A contemporaneous paper to ours by Akresh et al. (2018) also examines the long-run effects and intergenerational effects of INPRES. We view their analysis as complementary to ours. They use cross-sectional survey data from 2016 (SUSENAS) and study a different set of outcomes. Other recent papers that use the INPRES project include: Ashraf et al. (2018) who study bride price and female education; Bharati et al. (2016) who analyze impacts on adult time preferences; Bharati et al. (2018) who examine whether INPRES mitigates the effects of adverse weather shocks; and Karachiwalla and Palloni (2019) who examine the impacts on participation in agriculture.

the rollout of INPRES, i.e. the second generation. We find that children born to women exposed to the INPRES program score between 0.08 and 0.10 standard deviations higher in the national 9th grade examination. We also find notable effects on the health of the second generation children. Specifically, maternal access to INPRES elementary schools increases children's height-for-age by 0.05 SDs and reduces the likelihood of childhood stunting by 6% of the mean. We also find improvements in children's self-reported health and a decline in the likelihood of being anemic for daughters. Additionally, we construct an index of health outcomes in the second generation and find an improvement in health of about 0.03 SDs. These impacts are similar for sons and daughters. In general, we find smaller and statistically insignificant effects from father's exposure to the INPRES program. Overall, our results for the first and second generation are robust to alternative specifications and we show evidence from placebo regressions on the comparison group that validate the empirical strategy.

There are several pathways through which improved parental human capital (due to INPRES) could lead to better second generation outcomes. These include assortative mating, household resources, neighborhood quality, migration and fertility.[2] We find some mixed evidence that suggest that women exposed to INPRES are more likely to marry better educated men. In addition, we find that individuals exposed to the program have greater household resources as measured by per capita consumption and housing quality. We also observe that individuals exposed to INPRES are more likely to live in communities with better access to health services. On the other hand, our evidence suggests that fertility and migration responses do not drive our findings.

In addition, we assess the success of the INPRES intervention by conducting a cost-benefit analysis that accounts for the cost of construction and maintenance of the schools.[3] A key conclusion of our analysis is that including the spillover gains generated by the program as well as the effects on both generations makes a very big difference, raising the internal rate of return from 8 percent to as high as 24 percent. Thus, traditional cost-benefit analyses of school building interventions that only account for the labor market returns to education (e.g. Aaronson and Mazumder, 2011; Duflo, 2001) may be far too conservative in their assumptions and may significantly underestimate the full societal benefits.

Our paper contributes to several literatures. First, a vast literature in economics has documented the long-term effects of different types of social interventions on multiple dimensions of human capital outcomes, mainly in high-income countries (Aaronson et al., 2017; Bailey and Goodman-Bacon, 2015; Currie and Gruber, 1996a,b; Deming, 2009; Havnes and

[2]One important potential channel is parental investments in human capital. However, we do not have very good measures to proxy for such investments. In addition, general equilibrium effects may also influence both the first and second generations effects (Duflo, 2004).

[3]See Appendix C for the details of our calculations.

Mogstad, 2011; Hoynes et al., 2016; Miller and Wherry, 2018; Parker and Vogl, 2018; Rossin-Slater and Wüst, 2018). There is also emerging evidence from lower income countries on the long-term effects of interventions, which mainly analyzes demand-side educational interventions such as conditional cash transfers (Parker and Vogl, 2018), vouchers (Bettinger et al., 2017) and compulsory schooling laws (Agüero and Ramachandran, 2018). We add to this literature by examining the long-term effects of a common supply-side educational intervention in the form of school construction that aims to improve schooling access.

Second, we provide new evidence on the causal effects of schooling on health in the context of a middle income country. Most of the evidence thus far relies on randomized variation in pre-school access or quasi-experiments that exploit changes to compulsory schooling laws. Thus far, the findings on the causal effects of education on health (or mortality) are quite mixed (see Galama et al., 2018; Grossman, 2006; Mazumder, 2012; Oreopoulos and Salvanes, 2011 for a review).

Third, our paper contributes to the emerging literature on the intergenerational effects of social interventions. Relatively few studies have been able to causally identify the intergenerational effects of policies. This is mainly due to the demanding data requirements needed to answer this question. In particular, the analysis calls for data that measure outcomes in two distinct generations and link family members over time. Most existing studies on intergenerational effects have been done in the context of high-income countries: Head Start in the US (Barr and Gibbs, 2018; Kose, 2019), Medicaid in the US (East et al., 2017), compulsory education in Germany, Sweden and Taiwan (Chou et al., 2010; Huebener, 2017; Lundborg et al., 2014), and preschool in Denmark (Rossin-Slater and Wüst, 2018). There is limited evidence in low and middle income countries (Agüero and Ramachandran, 2018; Grépin and Bharadwaj, 2015; Wantchekon et al., 2014) and we are among the first to study the intergenerational human capital impacts of a national school construction program in a relatively lower income setting. The evidence of whether program effects persist and transmit to the next generation is highly policy relevant as current debates about the funding for such social programs may underestimate their benefits.

The remainder of the paper is organized as follows. Section 2 provides an overview of the program, Section 3 describes the data. Section 4 describes the methods used to estimate the program effects on the first generation and the long-term effects. Sections 5 describes the methods and results on the second generation. Section 6 presents additional robustness checks, and 7 discuss some potential mechanisms. Section 8 concludes with cost-benefit calculations and policy implications.

2 Background

2.1 The Indonesian context and INPRES Program

After proclaiming independence in 1945, Indonesia's constitution declared that every citizen has the right to education and that the government should provide a national education system. Children usually start their six years of primary schooling between the ages of 6 and 7. Primary school enrollment was around 65% in the 1960s (Booth, 1998), but progress in expanding primary school access was particularly fast after 1973.

During the 1970s, the increase in OPEC oil prices led to a boom in oil revenues for the Indonesian government and then President Suharto created the 'Presidential Instructions' program, or INPRES (*Instruksi Presiden*) to redistribute these gains in support of regional economic development (Duflo, 2001). The INPRES program operated at the province, district, and village levels and was categorized into education, health, environmental development, and road development (Tadjoeddin and Chowdhury, 2017). The education program focused solely on developing primary education, which was done in response to Indonesia's 31% illiteracy rate in the 1971 Census.

With the goal of achieving universal primary schooling, the INPRES Primary School (*Sekolah Dasar* INPRES, or SD INPRES) program was launched after Suharto signed the Presidential Instructions No. 10 in 1973, and the program began its implementation in 1974. To increase equity in access to basic education, the SD INPRES program targeted places with low primary school enrollment (Duflo, 2001). Provinces outside the main island of Java, which have been traditionally poorer and more rural, received more funding to allow for the building of more schools in those areas. The 1973 program was followed by further expansion in 1978-79, after Suharto signed the Presidential Instructions No. 3 in 1977. By 1979, the SD INPRES program had constructed over 61,000 primary schools, increasing the number of schools available by 2 schools per 1,000 children (Duflo, 2001). This rapid growth makes this program one of the fastest primary school construction interventions and a successful case of a large-scale school expansion on record (World Bank, 1989, 1990).

Under SD INPRES, funding for schools in each area ranged from IDR 2.5 to 7 million (in 1975 IDR), which is more than the IDR 2 million (in 1975 IDR) estimated cost of building a 'model school' (Daroesman, 1972). The program sought to reach at least 85% of primary school aged children in the country by building new schools and renovating some pre-existing primary schools. The renovation targeted public, private, and Islamic schools. These new schools also included improvements in classroom spacing, thereby avoiding the double shifts of students (where some students would attend the morning session while others would attend the afternoon session, resulting in a shorter instruction time per student). Additionally, these new schools were provided with school equipment (e.g., books, libraries) and adequate water and sanitation (Duflo, 1999; World Bank, 1989).

INPRES schools were smaller than existing ones and were staffed with teachers to attain a ratio of one teacher per 40 to 50 students, which was an improvement from a ratio of 50 to 60 students per teacher in the 1950s.[4] The creation of schools was accompanied by improvements in teacher training[5] and increases in teacher salaries. The primary school curriculum was standardized in 1975 to ensure a national standard of education. Additionally, the SD INPRES program efforts for increasing primary school completion were accompanied by the elimination of primary school registration fees in 1977, all of which contributed to an increase in primary school enrollment to 91% by 1981.

Other programs that were implemented at the time included an INPRES health program that focused on providing water and sanitation. This program, based on Presidential Instructions No. 5 in 1974, sought to build 10,500 piped water connections in villages and 150,000 toilets. This water and sanitation program was distributed based on the pre-program incidence of cholera and other diarrheal diseases, access to clean water, the availability of hygiene and sanitation workers, and a preliminary survey. Following Duflo (2001), we include the water and sanitation program as a control variable in our analysis of the SD INPRES program to avoid confounding issues.

The INPRES program continued with the increased inflow of oil revenue up to the early 1980s. By 1980, the INPRES program included primary education, health clinic, market, and road construction, and a reforestation program. The cost of INPRES programs rose from IDR 276 billion in 1973 (in 1980 prices) to 714 billion (in 1980 prices) (Tadjoeddin and Chowdhury, 2017). The long-term effects of the INPRES primary school program will inform other social interventions in Indonesia as well as other low and middle income countries since recent social interventions in lower income countries have focused on community-based programs. In Indonesia, such programs include the *Kecamatan Development Project* (KDP) and *Program Nasional Pemberdayaan Masyarakat* (PNPM). These programs seek to empower local communities in deciding resource allocation, including investments in infrastructure such as schools, as well as human capital investment such as teacher training.

The effects of INPRES literature

A handful of studies have examined the impacts of the INPRES primary school construction program on several outcomes using a difference-in-difference approach that exploits the

[4] https://unesdoc.unesco.org/ark:/48223/pf0000014169eng. Last accessed May 22, 2019.

[5] For example, the government developed and accelerated a program to construct primary teacher training schools (World Bank, 1989).

geographic intensity in the construction of schools and cohort variation. The earliest of these studies is that of Duflo (2001), who focused on men born between 1950 and 1972 to examine the effects of the program on educational attainment and wages. She finds that an additional school built per 1000 school-age children increased years of schooling by 0.12 to 0.19 years and wages by 1.5 to 2.7 percent. In a subsequent study, Breierova and Duflo (2004) use an instrumental variable approach that harnessed the variation of the INPRES program to study the effects of mother's and father's education on child health and find that parental education reduced infant and child mortality. Also, Duflo (2004) studies the general equilibrium effects of this large program and shows that the increase in education among exposed individuals increased their participation in the formal labor market and had a negative effect on the wages of older cohorts.

Martinez-Bravo (2017) examines the effect of the INPRES school construction program on local governance and public good provision and finds that the program led to a significant increase in the provision of public goods, such as the number of doctors, the presence of primary health care centers, and access to water. A potential mechanism behind these effects is the increase in the education of the village head.

We build on these studies that documents positive effects on the first generation exposed to the program to examine whether these individuals have better health 40 years after the intervention and whether these gains transmit to the next generation human capital formation.[6]

2.2 Existing evidence on school construction interventions

Improving access to education through school building is a popular supply-side intervention in low and middle income countries, where students may need to travel long distances to reach the closest school (Glewwe and Kremer, 2006; Kazianga et al., 2013). Several studies have examined whether improvements in school infrastructure have causal effects on enrollment and various short and medium term student outcomes such as test scores (e.g. Burde

[6] A contemporaneous paper to ours by Akresh et al. (2018) examines the long-term effects of INPRES on the socio-economic well-being of the first generation and the intergenerational effects on school attainment. Their analysis is complementary to ours, using nationally representative cross-sectional data in 2016 (Socio-economic survey, SUSENAS) and studying a different set of outcomes. One important difference is that their analysis does not include adult children who are no longer co-resident with their parents. In contrast, we exploit the IFLS-E and the longitudinal nature of the IFLS data to include all children including those who no longer reside with their parents as adults. We find that roughly 55 percent of the relevant children are no longer co-resident with their parents by 2014 in the IFLS. Other recent papers that use the INPRES project include: Ashraf et al. (2018) who study bride price and female education; Bharati et al. (2016) who analyze impacts on adult time preferences; Bharati et al. (2018) who examine whether INPRES mitigates the effects of adverse weather shocks; and Karachiwalla and Palloni (2019) who examine the impacts on participation in agriculture.

and Linden, 2013; Alderman et al., 2003; Kazianga et al., 2013 and Berlinski et al., 2009).[7] However, these studies did not examine longer-term outcomes, intergenerational effects or other dimensions of human capital such as health. A few studies of historical interventions have begun to explore such outcomes. These include Wantchekon et al. (2014) who found that colonial era missionary schools built in Benin had human capital spillovers as well as intergenerational effects, and Chou et al. (2010) who found that the 1968 Taiwanese compulsory schooling reform (which included a school construction component) improved birth weight and infant health in the next generation.

A related literature has analyzed the effects of the Rosenwald Schools which were built for blacks living in rural parts of the American South largely during the 1920s. The institutional setting for these schools is similar to what many developing countries face today. Aaronson and Mazumder (2011) show that the schools led to significantly higher educational attainment and test scores. Aaronson et al. (2014) show that the Rosenwald schools impacted fertility patterns and Aaronson et al. (2017) find that the schools improved long-term health.

2.3 The effect of education on health

One of the most striking findings in the social sciences is the gradient in health and mortality by socioeconomic status (e.g Cutler et al., 2011; Mackenbach et al., 2008). However, whether this association is truly causal and whether it can be influenced by educational polices is less clear and may depend on the context. If large-scale school building programs such as INPRES can improve not only educational attainment but also provide meaningful spillovers to health, then the case for such interventions become even more salient. Furthermore, if educational improvements also result in health improvements into the *next generation* then the case for these policies is even stronger.

There is a vast literature on this topic and a large variety of mechanisms have been proposed for how education can improve health. One theoretical perspective hypothesizes that education can improve productive efficiency and allocative efficiency (Grossman, 1972; Kenkel, 1991). Through productive efficiency, higher education leads to a higher marginal product for a given set of health inputs, so parents can have healthier children due to improvements in health and non-health resources. Through allocative efficiency, higher educated individuals choose more efficient inputs into health investment. Examples of these efficiencies include greater financial resources, improved knowledge, better decision-making ability and changes to time preference.

The empirical evidence on the causal effects of education on health appears to be mixed

[7] For a recent review of the literature see Glewwe and Muralidharan (2016).

but few studies have specifically explored the effects of school construction in the context of a lower income country.[8] Recent reviews of the literature include Cutler et al. (2011); Galama et al. (2018); Grossman (2015); Mazumder (2012). One possible explanation for the mixed findings in the literature is that education may induce other behavioral changes such as migration, and depending on the setting, these other behavioral changes can lead to worse health and therefore obscure the health promoting aspects of education.[9] Unlike much of the previous literature that has examined pre-school or compulsory schooling reforms, we provide evidence on the effect of expanding access to primary education on an individual's own physical and mental health in a lower income setting.

A growing number of studies extend the analysis of the effects of education on the health of offspring in the next generation. In general, parental education, especially that of the mother has been shown to be a strong predictor of children's outcomes such as birth weight (Currie and Moretti, 2003). However, changes in compulsory schooling laws in several countries have resulted in mixed evidence. Evidence from the UK shows little effects on child health (Lindeboom et al., 2009), while positive effects have been found in Taiwan (Chou et al., 2010), Zimbabwe (Grépin and Bharadwaj, 2015) and Turkey (Dursun et al., 2017). Previous work in our setting by Breierova and Duflo (2004) found that INPRES led to lower child mortality.

Potential mechanisms for these intergenerational effects include better access to information and improvements in women's health-seeking behavior (Dursun et al., 2017; Thomas et al., 1991). While these studies focus on children's outcomes before the age of 5, we examine children's outcomes when they are older to evaluate the persistence of the health effects. Our study contributes to the small but growing literature on the intergenerational effects of a large-scale education program in lower income countries.

[8] For example, early education programs like the Perry pre-school program, Abecedarian program, and Head Start in the US have been shown to improve health outcomes in adolescence and adulthood (Campbell et al., 2014; Carneiro and Ginja, 2014; Conti and Heckman, 2013). On the other hand, studies that exploit compulsory schooling laws across various countries (e.g. Clark and Royer, 2013, Oreopoulos, 2007) affecting different age groups in different time periods provide only mixed evidence on the effects of education on various health outcomes (Mazumder, 2012). For smoking and obesity, analyses from several high-income countries shows mixed evidence and some heterogeneity by gender and demographic characteristics (Galama et al., 2018).

[9] Aaronson et al. (2017) find that it is important to control for the effects of education on migration in order to isolate health promoting effects of education. Their study is in the context of the black schooling in the rural American South during the 1920s where the Great Migration of blacks to the North led to large reductions in health (Black et al., 2015).

3 Data

This paper uses data from the Indonesian Family Life Survey (IFLS), which combines the main IFLS and the IFLS-East (IFLS-E). The main IFLS is a longitudinal household survey that is representative of approximately 83 percent of the Indonesian population in 1993. Subsequent waves (IFLS 2 to 5) in 1997, 2000, 2007 and 2014 sought to re-interview all original households, as well as any households that had split-off. The IFLS-E, conducted in 2012, is modeled after the main IFLS and covers 7 provinces in the eastern part of Indonesia that were excluded by the main IFLS.[10] In 1993, IFLS-1 included 7,224 households residing in 13 provinces, which covered more than 200 districts. The IFLS-E included 2,500 households residing in seven provinces in eastern Indonesia, which covered about 50 districts. Thus, the main IFLS and IFLS-E included almost 300 of Indonesia's 514 districts.

The IFLS is well-suited to study long-term and intergenerational outcomes as it covers all the main geographic regions in Indonesia and collects comprehensive socio-demographic information, including information on respondents' place and date of birth, which is crucial for the assignment of INPRES exposure. The longitudinal aspect of the main IFLS and the fact that it follows split-off households allow us to link parents to their children at multiple points in time. Therefore, we can observe outcomes for second generation children regardless of whether they remained co-resident with their parents, instead of relying on children living in the household at only one point in time (cross-section).[11] This feature of the IFLS allows us to observe children of individuals in the comparison and treated groups at relatively similar ages because of the long-time span of the survey. Also, attrition rates across the five waves of the IFLS are low: the original household re-contact rate was 92% in IFLS-5, and 87.8% of original households in IFLS-1 were either interviewed in all 5 waves or died (Strauss et al., 2016).

Sample of interest We analyze the long-term and intergenerational outcomes of first generation individuals who were born between 1950 and 1972. Those born between 1950 and 1962 were older than primary school age (older than age 12) at the time of INPRES (in 1974) and thus were not exposed to the new schools, while those born between 1963 and 1972 were younger than age 11 during INPRES and thus could benefit from the school expansion. We call this sample the "expanded sample". It is worth nothing that the treated group in this sample comprises individuals partially and fully exposed to the INPRES schools. Those

[10] In a companion paper, we compare the estimated effect of the INPRES program on primary school completion using the nationally representative Intercensal Census (SUPAS) and the SUPAS restricted to the IFLS and IFLS-E provinces and find similar estimates (Mazumder et al., 2019).

[11] We find that around 55 percent of children born to first generation sample are no longer co-resident with their parents by 2014 in the IFLS.

partially exposed were older than age 7 but younger than age 12, so only a part of their primary school ages occurred after the program onset, while those who are fully exposed were younger than age 7 at the time of the program, thus exposed to INPRES schools during all their primary school years. Following Duflo (2001), we also present estimates for a sample that defines the treated group as those who were fully exposed (born between 1968 and 1972) and the non-exposed group as individuals who are closer in age (born between 1957 and 1962). We call this sample the "restricted sample". In both samples, individuals have completed most of their fertility cycle by 2012 or 2014, which enable us to examine their children's outcomes.

Long-term Outcomes Our first outcome of interest is an indicator for primary school completion, which is constructed using each individual's education history that included the highest level of education completed. Our next outcomes of interest include measures of self-reported health. Adult respondents were asked to report their physical health through a series of questions on self-reported health status and chronic conditions.[12] Using this information, we construct the following outcomes of interest: good self-reported health, the number of days a respondent missed his or her activities in the past 4 weeks prior to the survey, any diagnosed chronic conditions, and the number of diagnosed conditions. Some of these self-reported health measures have been found to be highly predictive of well-known health markers such as mortality (DeSalvo et al., 2005; Idler and Benyamini, 1997; Miilunpalo et al., 1997).[13]

We also examine mental health outcomes based on self-reported depressive symptoms. The IFLS collects information on depressive symptoms using 10 questions from the Center for Epidemiologic Studies Depression Scale (CES-D), which has been clinically validated. We score each symptom in the 10 questions and use the sum of the scores based on reported symptoms, where higher scores indicate a higher likelihood of having depression.[14] Because

[12]Although the IFLS also includes health care utilization, we exclude this from our analysis due to the low incidence of preventive care in developing countries, including Indonesia (Dupas, 2011). In our sample, only 8% of respondents reported obtaining at least one general health check-up in the 5 years prior to the survey. Body Mass Index (BMI) can also be calculated but it is *apriori* unclear whether this is a positive or negative outcome in developing countries. There is evidence of a positive association between SES and BMI in low and lower-middle income countries including Indonesia (Dinsa et al., 2012; Sohn, 2017). Finally, blood pressure readings are also available in the data which can be used to measure hypertension. We did not include this in our main analysis so that our results were a consistent set of self-reported health measures. We find evidence of a decline in hypertension in the restricted sample but not the expanded sample (results are available upon request).

[13]This has also been demonstrated by some studies in developing countries that find that self-reported health status is predictive of mortality even after controlling for socio-demographic factors (Ardington and Gasealahwe, 2014; Razzaque et al., 2014).

[14]Details of the construction of these variables are available in Appendix A.

some of our outcomes of interest are assessed for individuals older than 40 years old, we use information from the IFLS-E in 2012 and IFLS-5 in 2014.

To summarize the multiple health outcomes, we construct a summary index following Kling et al. (2007) and Hoynes et al. (2016). We standardize each health outcome by subtracting the mean and dividing by the standard deviation of the comparison group and we equalize signs across outcomes, so that positive values of the index represent poor health outcomes. Then, we create a new summary index variable that is the simple average of all standardized outcomes. The components include self-reported poor health (instead of self-reported good health that we use earlier), the number of days missing one's primary activity, any chronic conditions, the number of chronic conditions, and mental health score.

Intergenerational Outcomes Respondents born between 1950 and 1972, the first generation, have children who were born between 1975 and 2006, which make up the "second generation". Regarding children's health, the IFLS collects the following health measures: height and weight, hemoglobin count, and self-reported general health status (which was obtained from the primary caregiver for respondents under 15). Given the timing of the IFLS survey years and the wide range of birth years of the second generation, the oldest individuals in the second generation were 18 in 1993 (in IFLS-1) and the youngest were 8 in 2014 (in IFLS-5). Therefore, for the second generation's health outcomes, we focus on children aged between 8 and 18 in each wave of the IFLS.

Our second generation's health outcomes of interest include height-for-age and stunting, defined as more than two standard deviations below the mean in height-for-age z-score.[15] These health measures capture children's general growth trajectory and are considered good proxies for long-term and cumulative nutritional status (Falkner and Tanner, 1986; Mason, 1990). We also include anemia, from children's hemoglobin count, to capture children's nutritional status and self-reported general health to capture overall health status. Similar to the first generation long-term outcomes, we summarize these health outcomes for the second generation by creating a health index. The components include being stunted, being anemic, and having self-reported poor health (instead of self-reported good health), thus higher values represent poor health outcomes.[16]

Additionally, the IFLS includes children's educational history, which allows us to obtain children's primary and secondary school completion as well as scores on the national primary

[15] We use the 2007 WHO growth chart, which is applicable to children between 0 and 19 years of age.

[16] Details of the construction of these variables are available in Appendix A.

school (6th grade) and secondary school (9th grade) exams.[17] The scoring of the national examination changed between the 1980s and 2000s, so for consistency of comparison across the years, we create z-scores for each year of examination. In this study, we focus on the secondary school examinations since our companion paper (Mazumder et al., 2019) examines the intergenerational impacts of INPRES on primary test scores.

INPRES Exposure variable We combine administrative data on the number of INPRES primary schools built between 1974-1978 at the district level with the IFLS.[18] We assign geographic exposure to the INPRES program using the number of schools constructed in the first generation's district of birth, since using district of residence during respondents' primary school age is potentially endogenous. For the second generation, we use the household roster to identify biological mother-child and father-child pairs born to the first generation individuals, who were born between 1950 and 1972.[19]

Summary The analyzed first generation sample includes around 12,000 adults born between 1950 and 1972 with information on their place of birth and observed at any wave of the IFLS.[20] For the first generation long-term outcomes, the analyzed sample consists of around 10,200 individuals observed in the IFLS-E in 2012 or the IFLS-5 in 2014. Descriptive statistics for the sample are reported in Table A.1 (Panel A).

The average number of INPRES schools built in the first generation's district of birth is 2.1, consistent with the national average. The first generation sample is balanced across gender. About 60% of the sample were born between 1963 and 1972, and 46% of the sample are Javanese, the main ethnic group in Indonesia. First generation individuals score an average of 5.5 on the mental health screening questionnaire in 2012 (IFLS-E) and 2014 (IFLS-5). 72% of adults in the sample reported being healthy and 42% reported having at least one diagnosed chronic condition.

[17]While Indonesia has had national examinations since 1950s, the standardized national curriculum was implemented in 1975 and the national examination after the curriculum standardization began in 1980. Thus, test scores are only available for the second generation.

[18]Appendix Figures A.1 and A.2 show the comparison between the national INPRES rollout and the provinces covered by the main IFLS and IFLS-E. We are not aware of other previous studies that have utilized the IFLS-E to study the effects of INPRES on these outcomes.

[19]Details of the construction of these variables are available in Appendix A.

[20]A potential drawback of the IFLS data is its small sample size. To address this, we performed power calculations that validate the use of this data. Based on the number of birth districts (clusters) in the IFLS, which corresponds to 333, a control group with a mean of 0 and a standard deviation of 1 and an intraclass correlation of 0.03 (which is the ICC for the health indices), the sample of first generation individuals in the main IFLS and the IFLS-E allows us to detect treatment effect sizes between 0.03 and 0.04 standard deviations at the 10% and 5% significance levels with 80% power. For the first-order outcome of the intervention, primary school completion, the IFLS sample can detect effect sizes between 0.03 and 0.04 percentage points at the 10% and 5% significance level with 80% power.

The second generation sample consists of about 10,000 individuals who are the children of the first generation sample (Panel B). The sample is balanced across gender, and about 40% are first-born. The average year of birth is 1988. About half of the children have mothers who were born between 1963 and 1972, and similarly about a half have fathers who were born between 1963 and 1972. Children's health measures were taken in each wave, and we report the observation in each wave for children between the ages of 8 and 18. The average height for age z-score is -1.6 and 36% are stunted. About one fourth of the second generation have anemia, and almost 90% have good self-reported health.

4 The First Generation

4.1 Estimation Strategy

In the first part of the analysis, we estimate the long-term effects of INPRES on outcomes measured roughly forty years later. Following Duflo (2001), we exploit variation in exposure to the primary schools by birth cohort and geography as described in Section 3.

We estimate the intent to treat effects using the following equation:

$$y_{idt} = \beta(exposed_t \times INPRES_d) + \sum_t (P_d \times \tau_t)\delta_t + X_{idt}\gamma + \alpha_d + \tau_t + \epsilon_{idt} \qquad (1)$$

where y_{idt} is the outcome of interest for individual i born in district d in year t. $exposed_t$ is a dummy variable equal to 1 if individual i was born in the relevant birth cohorts exposed to INPRES. In the expanded sample, this indicator takes the value of 1 for cohorts born between 1963 and 1972, while in the restricted sample, the exposed cohorts were born between 1968 and 1972. $INPRES_d$ captures the intensity of the program: the number of schools (per 1,000 school-aged children) built in birth district d during the school construction program. α_d and τ_t are district and year-of-birth fixed effects. $P_d \times \tau_t$ captures birth-year fixed effects interacted with the following district-level covariates: the number of school-aged children in the district in 1971 (before the start of the program), the enrollment rate of the district in 1971 and the exposure of the district to another INPRES program: a water and sanitation program. These interactions control for the factors underlying the allocation of the INPRES school program and for other programs that could confound the program effects. X_{idt} is a set of individual characteristics: gender, ethnicity (Javanese indicator) and month of birth fixed effects.[21] Standard errors are clustered at the district of birth level. β measures the effect of one school built per 1,000 children. We estimate the models separately by gender.

The causal effect of exposure to INPRES schools is identified as long as the program placement of schools across districts is exogenous conditional on district of birth, cohort fixed-effects, and the interactions of year of birth and district level covariates. Hence, if before the construction of schools in 1974, districts with high program intensity had differential growth in educational outcomes compared to low program intensity districts, this might suggest that our identification assumption is not credible. Using the main IFLS and ILFS-E data, we show the similar trends in primary school completion for cohorts who finished primary school before the program was implemented in high and low program intensity districts.[22] Appendix figures A.3 and A.4 show that the levels in high and low intensity districts are different, but the trends in primary school completion before the construction of INPRES schools appear reasonably similar.

To validate our empirical strategy, we also perform placebo regressions using the IFLS data on comparison cohorts (individuals born between 1950 and 1962) for all the health outcomes of interest in the first generation. These regressions define a "placebo exposed group" as cohorts born between 1957 and 1962, while those born between 1950 and 1956 serve as the comparison group. We find small and statistically insignificant effects on all of our health outcomes (Figure A.5), thereby providing reassuring evidence on the absence of pre-trends before program implementation.

4.2 First Generation Long-term Effects

In our companion paper, Mazumder et al. (2019), we show evidence of the first order effects of the INPRES school construction program on primary school completion, the margin that the program targeted. We replicate the estimates in Table A.2, which has two panels: Panel A presents estimates for the "expanded sample", comprising of individuals born between 1950 and 1972. The treated cohorts, those born between 1963 and 1972, pool partially and fully exposed individuals. Panel B presents estimates for the "restricted sample" that excludes the partially exposed. This sample comprises of individuals born between 1957-1962 in the comparison and those born between 1968-1972 in the treated group. Each panel

[21] Our specification modestly improves upon Duflo (2001) in two ways. We use an ethnicity dummy for whether the individual is Javanese and we include month of birth dummies. We control for being Javanese since they are the largest ethnic group in Indonesia and in our sample, and the group has different means. We include month of birth to control for potential seasonality (Yamauchi, 2012). In Mazumder et al. (2019), we also estimate a model for primary school completion that uses the identical covariates as Duflo (2001) by excluding month of birth and ethnicity, and find similar effects.

[22] Following Duflo (2001), high program districts are defined as districts "where the residual of a regression of the number of schools on the number of children is positive".

has three columns: all, male and female. The results suggest that exposure to the INPRES primary schools increased the probability of completing primary school for both men and women. The effects are between 2.5 and 3 percentage points per primary school per 1,000 children in the "expanded sample", while the effects are between 3 and 5 percentage points among individuals fully exposed to the program in the "restricted sample".

We next turn to examining the long-run health outcomes in the first generation. We begin by examining self-reported general health, followed by chronic conditions, and finally mental health. These outcomes are measured in 2012 (IFLS-E) and 2014 (IFLS-5). Table 1 presents the estimated program effects for two outcomes: good self-reported health (if a respondent reported his or her self status as 'very healthy' or 'healthy') shown in columns 1 to 3 and the number of days of missed regular activities in columns 4 to 6. We find clear evidence of health improvements. Specifically, using the expanded sample, an additional school per 1,000 children improves self-reported health by 3.9 percentage points (Panel A, column 1). This represents an increase of around 6% relative to the mean. We find that there is a statistically significant increase of 6.2 percentage points for women (column 2) whereas for men, the point estimate is 1.7 percentage points and not statistically significant at conventional levels (column 3). We also find that exposure to INPRES decreased the number of days of missed activities by 0.17 days (almost 10% of the mean) (Panel A, column 4), with larger effects for women in the expanded sample. Panel B of Table 1 shows that the impacts are similar among individuals in the restricted sample that excludes the partially exposed.

Table 2 presents results of the estimated long-term effects of INPRES on self-reported chronic conditions. Using the expanded sample, we find that an additional primary school per 1,000 children lowers the likelihood of reporting any chronic conditions by 2.5 percentage points (6% of the mean). This impact is stronger among treated women, who are 3.9 percentage points less likely to report a diagnosed chronic condition (Panel A, columns 1 to 3). In terms of the number of diagnosed conditions (columns 4 to 6), we find that INPRES exposure is associated with a lower number of diagnosed chronic illnesses and this effect is concentrated among women, for whom the number of conditions decreases by 0.075 (9% of the mean) for each additional primary school constructed (per 1,000 children). Table 2 Panel B again shows that these patterns and magnitudes are similar for the restricted sample.

We then examine the effects of the program on adult mental health as an additional marker of well-being (Table 3). We use 10 items on depressive symptoms from the CES-D scale and use the sum of the scores. We find that adults exposed to INPRES are less likely to report symptoms of depression. This effect is stronger and statistically significant in the restricted sample when we focus on women who were fully exposed to the program. These women experience a decline in the CES-D depressive symptoms index by 9% of the mean (Panel B, column 3).

Since we consider several health outcomes, inference may be a concern due to multiple hypothesis testing. To address this, following Kling et al. (2007) and Hoynes et al. (2016), we use a health index, where higher values represent poor health. The components include self-reported poor health, the number of days missing primary activity, any chronic conditions, the number of chronic conditions, and mental health score (higher scores correspond to more symptoms of depression). Figure 1 corroborates our previous findings. Adults exposed to the INPRES primary school experience a decline in the poor health index by 0.04 standard deviations for each additional primary school constructed (per 1,000 children). This impact is concentrated among treated women, for whom the effect corresponds to -0.06 standard deviations.[23] These results are similar in the expanded and restricted sample.

Taken together, those exposed to INPRES have better self-reported physical and mental health, which suggests the link between improved education and health. We contribute to the literature on the non-pecuniary effects of improving education by providing evidence from a lower middle income country. Our results are consistent with evidence from Bharati et al. (2016), who find that INPRES increased patience in adulthood. Theoretically and empirically, more patience is associated with better health investments (Fuchs, 1980), and the effects of INPRES on time preferences constitute a potential mechanisms for our findings on long-term health.[24] In addition, many of the health markers examined in this section have been shown to be highly correlated with adult mortality (Idler and Benyamini, 1997).

Magnitudes

The average number of INPRES schools built was 1.98 per 1,000 children. This implies that on average, exposure to INPRES primary schools increases the probability of being 'healthy' or 'very healthy' by about 20% of the mean. Similarly, at the average exposure, we find a 7 to 14% reduction in reporting any chronic conditions and a 10 to 14% reduction in the number of reported chronic conditions. For comparison, our findings are in line with previous studies that have used changes in compulsory schooling laws (CSL) to estimate the impacts of education on similar self-reported health outcomes, even though such studies mainly focus on higher-income countries. For example, Mazumder (2008) and Oreopoulos (2007) find that an additional year of schooling from CSL in the US, UK and Canada reduces the probability of being in fair or poor health by around 20% of the mean. Additionally, we

[23]We also adjust the standard errors for multiple hypothesis following Simes (1986) and most of our results are robust to this adjustment.

[24]We are unable to examine time preference because the IFLS-E does not include this module.

find that first generation individuals exposed to INPRES had fewer mental health symptoms, by 7 to 9% of the mean at the average level of INPRES exposure. This finding is similar to the effects of changes in CSL laws on well-being: the CSL law in the UK is associated with an increase in overall life satisfaction by about 6% of the mean (Oreopoulos, 2007), while the estimated effect of education reforms on the reduction in depression in several European countries (Austria, Germany, Sweden, the Netherlands, Italy, France and Denmark) is about 7% (Crespo et al., 2014). Overall, our findings on the long-term effects of improving access to primary school on health outcomes are similar to estimates from previous literature that document the link between education and health.

5 The Second Generation

5.1 Estimation Strategy

In this section, we examine the effects of parental exposure to INPRES on second generation human capital outcomes, exploiting the longitudinal nature of the IFLS.[25] Specifically, we estimate the following equation:

$$y_{idt} = \beta(ParentExposed_t \times INPRES_d) + \sum_t (P_d \times \tau_t)\delta_t + X_{idt}\gamma + \alpha_d + \tau_t + +\epsilon_{idt} \qquad (2)$$

where y_{itd} is the outcome of interest for child i whose mother/father was born in district d in year t. The interaction $ParentExposed_t \times INPRES_d$ captures parental exposure to INPRES based on parental district and year of birth. X_{idt} is a set of child characteristics: gender, birth order, and year and month of birth dummies. α_d and τ_t are parent's district and year-of-birth fixed effects. The rest of the variables are as defined in equation 1. Standard errors are clustered at the parent's district of birth level. As in the previous section, we consider the effects separately for the expanded sample and the restricted sample. The coefficient β captures the effect of parental exposure to one INPRES school built per 1,000 (first generation) students.

We estimate the models separately for mother's and father's INPRES exposure. This provides the reduced form effect of exposure of any one parent separately but does not account for the possibility that both parents could have been exposed. Therefore, we also estimate models that include both maternal and paternal exposure in Section 6. These models are more demanding on our data and less precisely estimated, but as we show below,

[25]We rely on co-resident children in the IFLS-E.

our results are robust to this specification.

Our empirical strategy relies on the assumption that parental INPRES program exposure is uncorrelated with unobserved characteristics that vary across districts over time that also may affect the second generation's outcomes. To test the parallel trends assumption, following the placebo test of the first generation, we perform a falsification test that uses the children of adults in the comparison group (adults born between 1950 and 1962). In this placebo regression, we assume that adults born between 1957 and 1962 were exposed to the program. We find no statistically significant difference in children's educational and health outcomes (Figure A.6). These results suggest similar pre-trends in the outcomes across districts among children born to adults not exposed to the INPRES program.

5.2 Intergenerational effects

Education We begin by examining whether parent exposure to INPRES resulted in improved educational outcomes in the next generation. Previous evidence documents that children born to women who were exposed to INPRES perform better on the national primary school examination (Mazumder et al., 2019). We now extend the analysis to examine scores on the national secondary school examination. We find that children of mothers who were exposed to INPRES perform significantly better on the national secondary school examination (Table 4). On average, maternal exposure to one INPRES school (per 1,000 students) increases their children's secondary test scores by 0.08 standard deviations in the expanded sample (Panel A) and 0.10 standard deviations in the restricted sample (Panel B). The estimated effects are roughly similar for sons and daughters in the expanded sample (Columns 2-3, Panel A). However, in the restricted sample, the effect of maternal exposure is only statistically significant for daughters and the point estimate is much larger for daughters (0.18 SDs) than for sons (0.04 SDs). Turning to fathers' INPRES exposure, the estimates are typically a little smaller and in no case are they statistically significant (Columns 4-6).

We also examine children's secondary school completion. Here we find small and statistically insignificant effects of maternal and paternal INPRES exposure (Table A.4, Columns 1 and 4 respectively).[26] The estimated effects of maternal and paternal exposure are similarly small and not significant for sons and daughters in the expanded and restricted samples (Panel A, Columns 2-3, 5-6 and Panel B respectively). These results are likely due to the fact that Indonesia implemented 6 years of compulsory schooling in 1984 and expanded that to 9 years of compulsory schooling in 1989. Since the majority of the second generation

[26] The second generation was born between 1975 and 2006, therefore the majority should have completed primary school by age 13 and the older children should have completed secondary school (9th grade) by the age of 16. We also examine children's cognitive skills and find no significant effect.

individuals in our sample are affected by the compulsory schooling laws, this could explain why parental education has no effect on school completion but does appear to affect test scores.[27]

Health We now analyze the intergenerational effects of INPRES on several measures of health. We begin with two measures that capture the cumulative effects of health investments: children's height-for-age and stunting.[28] Given the timing of the IFLS and the fact that second generation children were born between 1975 and 2006, children are observed between ages 8 and 18 across IFLS survey years. Also, because children's height is measured in each wave, we may observe a child multiple times. To take into account the multiple observations per child, we add wave fixed effects to equation 2 and compute standard errors that are clustered two ways at the parent's district of birth and respondent (child) level. Table 5 shows that parental exposure to an additional INPRES school per 1,000 children improved height in the next generation. In particular, we find a 0.06 standard deviation increase in children's height-for-age z-score among those whose mothers were exposed to the program, and the effect is stronger for daughters (Panel A of Table 5). We find small and statistically insignificant effects among children whose fathers were exposed to the program (Columns 4 to 6). The estimated effects are noisy, but qualitatively similar when we focus on the restricted sample (Panel B). These results suggest that maternal education is an important channel to improve the health of the next generation.

We also find a reduction in stunting rates, which is consistent with the estimated increase in height-for-age among these children (Table 6, Panel A). Children born to mothers exposed to INPRES are 2 percentage points less likely to be stunted, which corresponds to 6% of the mean. We find no significant effect through paternal exposure to INPRES. Using the restricted sample yields qualitatively similar results. Given Indonesia's high stunting rates and the adverse effects of stunting, our results suggest that improved access to education in one generation can spillover to enhancing health in the next generation. These results are consistent with earlier work that finds an effect on infant mortality through maternal but not paternal exposure to INPRES (Breierova and Duflo, 2004).

Additionally, we examine other markers of health among second generation individuals: being anemic and self-reported health (Tables 7 and 8). We find that daughters born to mothers exposed to INPRES are 4.4 percentage points less likely to be anemic (12% of

[27] We did not examine high school completion since a significant fraction of second generation children are not old enough to be at that level of education. However, we will explore this as future IFLS become available.

[28] We did not analyze weight indicators as those are considered more short-term measures of health rather than a long-term cumulative indicator such as height (Group et al., 1986).

the mean, per one school per 1,000 students). Also, paternal exposure to INPRES has a similar impact on daughters' anemia status. Turning to self-reported health status, table 8 shows that maternal exposure to INPRES increases the likelihood of being healthy, with similar impacts for both sons and daughters (1.3-1.5 percentage points). Since we assess the intergenerational health impacts on several measures, we construct a summary index using a similar procedure as the one described for the first generation health outcomes. The components of the second generation health index are the following indicators: being stunted, being anemic and self-reported not healthy, thus higher values represent "poor health" outcomes. Figure 2 corroborates our previous findings, second generation children born to mothers exposed to INPRES experience a decline in the "poor health" index by 0.04 standard deviations for each additional primary school constructed (per 1,000 students).[29] This effect is similar across sons and daughters in both the expanded and restricted samples.[30]

One potential concern when considering outcomes in the second generation analysis is whether fertility responses among treated women could alter the composition of the sample, which, in turn, could influence outcomes. For example, a treated mother could delay or increase spacing compared to comparison mothers. We explore this in section 6 and provide evidence that fertility responses are not likely to influence the composition of the second generation sample.

Taken together, mothers exposed to the program have children with better health and educational outcomes, which suggests the importance of interventions that improve maternal education in order to improve children's future outcomes. Our findings are consistent with previous studies that have shown the intergenerational effects of social interventions in high income countries on children's health and education (Barr and Gibbs, 2018; Chou et al., 2010; East et al., 2017; Huebener, 2017; Kose, 2019; Lundborg et al., 2014; Rossin-Slater and Wüst, 2018), and our results contribute to the growing evidence in low and middle income countries (Agüero and Ramachandran, 2018; Grépin and Bharadwaj, 2015; Wantchekon et al., 2014), where intergenerational mobility tends to be lower than in high income countries.

[29] We also adjust the standard errors for multiple hypothesis following Simes (1986) and most of our results are robust to this adjustment.

[30] One concern is the use of multiple observations per individual since a child in the IFLS may be between ages 8 and 18 in several waves of the survey. To address this, we estimate equation 2 using the average outcome across waves and restricting the sample to one observation per child, weighted by the number of observations per child. The estimated effect is similar to our earlier findings. Children whose mothers were exposed to INPRES have 0.05 standard deviations higher average height for age z-score and are about 2.5 percentage points less likely to be ever stunted (Table A.3).

Magnitudes

We now consider the size of the intergenerational effects on education and health. Under INPRES, 1.98 schools were built per 1,000 students, which implies that, on average, maternal exposure to INPRES increases secondary test scores between 0.16 and 0.20 SDs. Ideally we would like to compare these effect sizes to the impacts of other social interventions in developing countries on similar intergenerational outcomes. However, such studies are nonexistent, highlighting the relevance of our findings. Therefore, for comparison, we refer to effect sizes from other interventions on similar outcomes measured on those directly exposed in the first generation. For the case of Conditional Cash Transfers (CCT), Baird et al. (2014, 2011) find that 2 years of exposure to a CCT program in Malawi that focused on 13-22 year-old girls increased their English test scores between 0.13-0.14 SDs and math scores between 0.12-0.16 SDs. Similarly, Barham et al. (2013) examines the long-term effects of exposure to the Nicaraguan CCT program in primary school on boys' test scores ten years later and find that the program increases average test scores by 0.2 SDs. For the case of merit-based scholarship programs, Friedman et al. (2016) examine the effects of a scholarship program in rural Kenya that targeted girls who were in grade 6 and find that their test scores increase by 0.2 SDs in grade 10-11. Taken together, the effects of INPRES on second generation test scores are comparable to the effect sizes of interventions like CCTs.

In terms of intergenerational health outcomes, we find that maternal exposure to INPRES raises height for age z-score (HAZ) by 0.12 SDs and reduces stunting by 0.05 percentage points (14% of the mean). In comparison, Fernald et al. (2008) find that a doubling of Mexico's CCT program led to a 0.16 SDs increase in HAZ and reduced stunting by 9% among children ages 10-14. Similarly, Barham (2012) finds that exposure to the Matlab Maternal and Child Health and Family Planning Program in Bangladesh increases HAZ by 0.2 SDs. Nores and Barnett (2010) summarize the impacts from early childhood interventions in developing countries and report that average effect sizes on long-term child health outcomes (ages 7 or above) are around 0.12 SDs. Overall, the magnitudes of our findings on the second generation effects of the INPRES program are similar to estimates from studies that evaluate other social interventions in developing countries.

6 Robustness

6.1 Alternative specifications

Intergenerational effects: including both parents' exposure In the previous section, we generally find larger effects from maternal than paternal exposure to INPRES in our baseline specification when we consider each parent separately. One possible concern with that specification is that the comparison group may be contaminated. This is because the program could affect the marriage market (Akresh et al., 2018; Ashraf et al., 2018; Zha, 2019). For example, a father in the comparison group (older than primary school age at INPRES rollout) could marry a woman in the treatment group and thus be indirectly exposed to INPRES. Therefore, we also run specifications where we include both maternal and paternal exposure as a robustness check (Table 9).[31]

The estimated effect of maternal exposure on the national secondary examination when we include paternal exposure is 0.03 to 0.15 standard deviations, which is quite similar to our earlier estimates. We also find that the intergenerational health effects of maternal exposure are also robust to the inclusion of paternal exposure. The index for poor health is 0.02 to 0.05 SDs lower. Children are on average taller by 0.06 SDs in height for age, with a stronger effect for daughters. While noisy, we find a similar point estimate for the effect of maternal exposure on stunting with the inclusion of both parents' exposure. Overall, we find stronger intergenerational effects from maternal access to the INPRES program than from paternal exposure, and these findings are robust to estimating the impacts from both parents' exposure simultaneously. The finding of larger effects from maternal exposure than paternal exposure is consistent with earlier work by Lundborg et al. (2014) and Huebener (2017) for a mid-20th century schooling reform in Sweden and Germany respectively.[32]

Alternative exposure variable Following Duflo (2001), our main specifications, as depicted in equations (1) and (2), use the number of INPRES primary schools built between 1973-74 and 1978-79 per 1,000 children in the district of birth as a measure of the first generation's geographic intensity of exposure to the program. This assumes that individuals are exposed to the stock of schools at the end of the program. For robustness, we define an alternative exposure variable using the number of schools constructed during an individual's primary school years (between the ages of 6 and 11) based on his/her age during the years of the program implementation at his/her district of birth. Table A.5 shows our main results for the first and second generation outcomes using this alternative specification and illustrates that the estimated effects are similar in magnitude and statistical significance to

[31] We estimate a similar equation to equation 2 for mother's exposure and include the following father's covariates: birth province, year of birth, interactions between year of birth (in two-year bins) and these district-level covariates: the number of school-aged children in the district in 1971 (before the start of the program), the enrollment rate of the district in 1971 and the exposure of the district to the contemporaneous water and sanitation program. This specification is much more demanding on our data and leads to less precise estimates.

[32] In contrast, Chou et al. (2010) and Agüero and Ramachandran (2018) find that both maternal and paternal exposure to CSL influence their children's outcomes.

those from our main specification.

Alternative sample In addition, following Duflo (2001), our sample of interest corresponds to first-generation men and women born between 1950 and 1972, which combines non-exposed, partially, and fully exposed individuals. For example, individuals born in 1972 were aged 2 when the INPRES program started in 1974, thus they are fully exposed to the schools constructed. As an additional robustness check, we expand our sample to include additional cohorts of fully exposed individuals: those born up to 1975. These individuals are likely to have completed their fertility cycle by 2012-14 (IFLS-E in 2012 and IFLS-5 in 2014), thereby allowing us to observe the second generation's outcomes. Results using this alternative sample are remarkably similar to our main sample estimates (Table A.6).

7 Potential mechanisms

In this section, we explore some of the possible mechanisms for our key findings. This is a challenging endeavor both because there are a multitude of hypothesized channels and because there are important data limitations. For example, ideally, we would like to be able to explore the extent to which parents who were exposed to INPRES chose to invest more in their children's health through investments such as breastfeeding or vaccinations. Unfortunately, we do not have data on these kinds of parental investments. Nevertheless, there are several important channels which we are able to investigate using the richness of the IFLS data. For instance, our finding that INPRES led to improvements in maternal health could have also led to human capital gains in the second generation. We then perform some simple calculations to assess the relative contributions of these mechanisms.

Household resources The first channel we consider is whether the greater access to education afforded by INPRES allows individuals to accumulate more resources and make more productive investments in health Grossman (1972).[33] To measure household resources, we use data on per capita consumption, which is a widely used proxy for household income and well-being.[34] Specifically, we use the IFLS-5 (in 2014) and IFLS-E (in 2012) to measure

[33] We, unfortunately, are not able to analyze the role of productive investments in health due to the low rate of preventive care. We are also not able to examine the role of allocative efficiency (Kenkel, 1991) through improved knowledge due to data limitation. The IFLS includes questions on breast cancer awareness for women in the first generation, but the response rate is low.

[34] See for example Aizer et al. (2016) and Akee et al. (2018)who find that household income and resources are an important input for individual health and children's human capital. Per capita consumption is recorded more precisely than household income and it is considered a better proxy for permanent income than current income (Grosh et al., 2000).

log per capita expenditure (in 2012 *Rupiah*), which is based on weekly or monthly per capita food and non-food expenditure.[35] In addition, we use data from the same survey years to construct a housing quality index which we use as a proxy for the household environment.[36] The index combines poor housing characteristics, which include: poor toilets, floors, roofs, walls and whether there is high occupancy per room.[37] We standardize each housing item by subtracting the mean and dividing by the standard deviation of the comparison group, and create an index that is the average of the standardized outcomes. Thus, higher values of the index reflect poor housing quality.

We find that individuals who were exposed to INPRES do in fact have greater household resources as captured by these two measures (Table 11). Specifically, we show that each additional INPRES school per 1,000 children leads to approximately 5% higher consumption (columns 1-3) and a lower index of poor housing quality of about 0.03 to 0.04 SDs (columns 4-6).[38] However, as we describe in greater detail below, we find that these improvements can account for very little of our findings on long-term health and improvements in offspring human capital.

Migration A second potentially important channel is migration as INPRES may have led individuals to move to either better or worse areas. For example, Aaronson et al. (2017) find that in the context of schools built for rural blacks in the American South in the first half of the 20th century, a failure to separately account for the effects on migration to areas where blacks suffered from higher mortality, can obscure the health promoting effects of education.[39] To address this, we directly consider how INPRES may have influenced migration decisions. In other words, we examine whether individuals exposed to INPRES are more or less likely to migrate out of their place of birth than those not exposed to the program. We explore several indicators of migration out of the place of birth as explained below.

First generation: We begin by considering migration in the first generation. We use two indicators for migration and estimate equation 1 (Table A.8, Panel A). First, we create an

[35] We exclude annual non-food expenditure, which includes items such as land and vehicle purchases.

[36] We also considered an index of asset ownership and found similar albeit noisier results to using consumption and housing quality (Table A.9). The asset index includes: savings, vehicle, land, TV, appliances, refrigerator, and house.

[37] Poor toilet is captured by not having access to a toilet (including shared or public toilet). Poor floor includes board or lumber, bamboo, or dirt floor. Poor roof includes leaves or wood. Poor wall includes lumber or board and bamboo or mat. High occupancy per room is defined as more than two persons per room in the house (based on household size).

[38] We explore the role of each item of the index and find that the results are consistent across the housing items (Tables A.11-A.12).

[39] Black et al. (2015) found that Blacks who migrated North during the Great Migration experienced a substantial reduction in longevity.

indicator variable for each individual in the sample that takes the value one if the district of birth is different from the current district of residence in any of the waves of the IFLS, and zero otherwise. Second, we create a similarly coded variable that only compares the district of birth to the district of residence in 2012 (IFLS-E) or 2014 (IFLS-5), which are the waves we use to measure our outcomes of interest for the first generation.[40] We find that exposure to INPRES has no significant or sizable effect on either of these indicators of migration. Additionally, we estimate equation1 on the sample of non-movers and find similar estimates on long-term health (Table A.13).

Second generation: We further explore migration by considering the possibility of parents (first generation) migrating to a potentially better or worse community before or after the birth of their child (second generation) by estimating equation 2 (Table A.8, Panel B). For the former, we create an indicator that takes the value one if the mother's district of birth is different from the child's district of birth. For the latter, we create an indicator that takes the value one if the child's district of birth is different from the child's current district of residence. We find that maternal exposure to the INPRES program has no significant effect on migration before or after the birth of her child. For robustness, we estimate the impacts of the second generation's human capital outcomes on the sample of non-movers and find similar intergenerational effects (Table A.13).

Neighborhood quality Another potential channel through which schooling can impact long-term and intergenerational outcomes is through human capital externalities. One prominent example is that education could lead to higher levels of political participation (Wantchekon et al., 2014), which could in turn lead to policies that improve socioeconomic outcomes. Indeed, Martinez-Bravo (2017) found that INPRES increased the level of education of potential candidates to local leadership positions, which in turn led to improvements in local governance and greater provision of public goods. This finding suggests that we ought to examine the role of neighborhood quality as a potential mechanism behind our results. There is also strong evidence from the U.S. and Australia that children's human capital is shaped by the neighborhood where they grow up (Aaronson, 1998; Chetty and Hendren, 2018; Chetty et al., 2016; Deutscher, 2019).

To investigate the potential role of neighborhoods, we use the IFLS community survey (IFLS-5 in 2014 and IFLS-E in 2012) to create indices of education and health service provision at the community level (village or township in rural and urban areas respectively).

[40]The first measure is broader in that it accounts for some individuals who might have left their district of birth but then returned, and individuals who were no longer in the sample in 2014. The second measure is directly comparable to our estimation sample.

We also create a poverty index based on the fraction of households in the community in the following social assistance programs: subsidized rice program (*Raskin*), subsidized national health insurance (*Jamkesmas*), subsidized regional health insurance (*Jamkesda*). *Raskin* is a national program that provides rice, the staple food, at highly subsidized prices for poor households. *Jamkesmas* is also a national program that provides health coverage for poor households. *Jamkesda* is similar to *Jamkesmas*, but provided at the province or district level. Thus, the poverty index captures both poverty and access to anti-poverty programs.

Greater provision of educational services at the local level may explain the second generation's improved educational outcomes, while access to health services may explain both the first and second generation's improved health outcomes. One caveat is that we are only able to observe neighborhood quality for non-movers in IFLS-5 (in 2014).[41] Fortunately, migration does not appear to drive our estimated effects, thereby minimizing selection concerns. For each community in IFLS-5 (in 2014) and IFLS-E (in 2012), we create an education index using the number of primary, junior high, and high schools used by the community.[42] Similarly, the health index includes the following: an indicator for having a majority of the residents using piped water, an indicator for having a majority of the residents using private toilet, the number of community health centers, and the number of midwives available to the community.

We find no statistically significant effect on neighborhood access to education facilities (Table A.14, columns 1-3).[43] In contrast, we find that individuals who were exposed to INPRES have better access to health services in their communities, and this effect is statistically significant for the restricted sample that focuses on the fully exposed (Panel B, columns 4-6). In addition, this effect seems to be concentrated among women. Similarly, we find that women in the restricted sample tend to reside in communities with lower poverty (by 0.069 standard deviations, columns 7-9). These results are in line with the INPRES program improving public good provision. However, as we discuss below, by our calculations, these effects on neighborhood quality explain a relatively small portion of the estimated effects we find.

[41]The main IFLS collects information on the original 312 communities sampled in IFLS-1 in each wave of the survey. Therefore, neighborhood quality measures are only available for individuals who resided in the original 312 communities in 2014.

[42]We standardize each item by subtracting the mean and dividing by the standard deviation of the comparison group, and create a new summary index variable that is the simple average of all standardized outcomes.

[43]We estimate equations 1 and 2 and cluster the standard errors by district of birth (parental district of birth for the second generation) and community.

Marriage and Fertility In this section, we consider the possibility that exposure to INPRES resulted in behavioral changes regarding marriage and fertility, which in turn, could affect long-term outcomes and also impact the subsequent generation. A large literature has documented positive assortative mating on education (Anukriti and Dasgupta, 2017; Behrman and Rosenzweig, 2002; Hahn et al., 2018). We explore the impacts of INPRES on the spousal characteristics of the first generation men and women (Tables 10) and find some evidence of improved marital outcomes for women exposed to the program, but not for exposed men. Specifically, women fully exposed to the program are more likely to marry higher educated men. However, the estimates are smaller and noisier for the expanded sample which defines both partially and fully exposed individuals as treated.

With respect to fertility, a similarly large literature starting with Becker and Lewis (1973) has suggested that improving women's education may increase the opportunity cost of having children, thus delaying childbearing.[44] Also, better educated women may make better fertility choices through contraceptives (Kim, 2010).[45]

We investigate women's fertility responses to INPRES exposure and find no significant effects on several measures including: the age of first pregnancy, number of live births, or birth spacing between the first and second child (Table A.7).[46]

Maternal health The documented effects on the first generation female health are themselves potential channels for the gains observed in the second generation. In particular, there is growing causal evidence that documents the impacts of maternal mental health, especially during pregnancy, on children's human capital and long-term outcomes (Aizer et al., 2016; Black et al., 2016; Persson and Rossin-Slater, 2018). Additionally, in low and middle income countries, maternal mental health has been identified as an important predictor of child development (Walker et al., 2011).

The contribution of various mechanisms In order to assess the potential contribution of the mechanisms presented above in explaining our main findings we rely on some relatively simple "back of the envelope" calculations. We do this by combining: i) estimated associations between each mechanism and our outcome of interest in the *comparison* cohorts; ii) our estimated effects of INPRES on each mechanism. We then compare the implied effects from this exercise to our estimates of INPRES on the outcome of interest. It is worth

[44] See for example Aaronson et al. (2014); Hahn et al. (2018).

[45] We examine the impact of INPRES exposure on women's contraception knowledge and ever use of contraceptive methods and find no statistically significant effect. Ideally, we would have information on women's history of contraceptive use to analyze use of contraception before the first birth.

[46] Following Aaronson et al. (2014), we also examine fertility on the extensive margin and find small and non statistically significant effect.

noting that for part i), the associations we use are purely observational in nature as we lack a strong research design for causal inference.[47] For the first generation outcomes, we focus on the following mechanisms: per capita expenditure as a proxy for household resources, the community health services index as a proxy for neighborhood quality, and assortative mating. Our calculations imply that household resources, neighborhood quality, and assortative mating combined explain less than 10% of the effect of INPRES on long-term health, while the remaining can be attributed to the effect of the program on own individual's education, other human capital externalities and other mechanisms we could not explore, such as decision-making and knowledge acquisition.[48]

For the second generation calculations, we focus on the following mechanisms: household resources, community health services quality, maternal and paternal education. Our back of the envelope calculations imply that these mechanisms combined explain between 15% and 57% of the effect of INPRES on the second generation's human capital outcomes, with

[47] If there is omitted variable bias, then it is possible we may be overstating the effect of each mechanism.

[48] The regressions to assess the associations include socio-demographic characteristics (i.e: gender, ethnicity), year of birth fixed effects and place of birth fixed effects. For the first generation, we observe that log per capita expenditure is associated with a 1.6 percentage point (pp.) increase in self-reported health (among the non-exposed). Combining this with the INPRES effect on log per capita expenditure (between 0.054 and 0.046 in Table 11) and the impact of INPRES on self-reported health (between 3.3 and 4 percentage points in Table 1), the INPRES induced improvement in per capita expenditure could explain between 2.5% to 3.5% of the INPRES effect on adult self-reported health. For neighborhood quality, we find that a one standard deviation increase in the community health services index is associated with a 0.02 percentage point increase in self-reported health. Combining this with the effect of INPRES exposure on community access to health services (0.024 to 0.055 SDs in community health services index in Table A.14) and the INPRES effect on self-reported health, the INPRES induced improvement in the quality of exposed individual's community of residence explains between 1.1% and 3% of the program effect on self-reported health. For assortative mating, the association between spouse primary school completion and individual's self-reported health is very large (5 pp. from Table 10), thus it should be taken with caution. Combining this association with the effect of INPRES on spouse's primary school completion (0.6 to 3.2 pp.) and the program effect on self-reported health, INPRES impact on spouse education may explain up to 3% of the effect on self-reported health.

household resources and maternal education playing an important role.[49] The remainder would then be attributed to the contributions of the program effect on maternal health[50], other human capital externalities from INPRES, and other mechanisms we could not explore, such as maternal bargaining power.

8 Discussion and Conclusion

We find that increased access to primary education through a massive school construction program in Indonesia has important spillover effects both on other dimensions of human capital, and on the *offspring* of individuals exposed to the program. Individuals exposed to INPRES have better health outcomes 40 years later, including better self-reported health status, fewer mental health symptoms, and fewer chronic conditions. These findings constitute new evidence on the causal effects of education on health in the context of a developing country.

We also find striking evidence of intergenerational spillovers. Children of mothers exposed to INPRES had significantly higher test scores, are less likely to be stunted, and have better self-reported health. We find no statistically significant effects through paternal exposure and the point estimates are either similar or smaller than those from maternal exposure. Our findings are not driven by fertility responses or migration and are robust to alternative specifications. We present some evidence that greater household resources and better neigh-

[49]For the case of child height, we observe that log per capita expenditure is associated with a 0.18 SDs increase in height for age z-score (HAZ). Combining this with the impact of INPRES on log per capita expenditures (between 0.054 and 0.046 in Table 11) and the impact of INPRES on HAZ (between 0.04 and 0.056 SDs in Table 5), the INPRES induced improvement in per capita expenditures explains between 15% and 22% of the INPRES effect on HAZ. For neighborhood quality, we find that a one standard deviation increase in the community health services index is associated with a 0.08 SDs increase in HAZ. Combining this with the effect of INPRES on community access to health services index (0.024 and 0.055 SDs in Table A.14) and the INPRES effect on HAZ, the INPRES induced improvement in the quality of exposed individuals' community of residence could explain between 3.4% and 10% of the program effect on HAZ. For parental education, we observe that mother's primary school completion is associated with 0.16 SDs increase in HAZ, while paternal primary school association is associated with a 0.08 SDs increase. Putting this together with the INPRES effect on the first generation female and male primary school completion (between 3 and 5 pp) and the effect of the program on second generation HAZ, we find that the INPRES effect on maternal primary school completion could potentially explain between 9% and 20% of the intergenerational effect and paternal education explain between 4% and 6%. We perform similar calculations for other second generation outcomes. For the second generation health index, we observe that household resources explain between 6% and 9%, community health services index between 2.6% and 7.5%, mother's education between 6.2% and 14%; and father's education between 3.6% and 6%. For children's secondary school test scores, we find that household resources may explain between 5% and 10%, mother's education between 7% and 10%; and father's education between 3% and 5% of the INPRES effect.

[50]We were unable to calculate associations between maternal mental health and children's human capital outcomes for the comparison group because the children of the comparison group are mainly observed in the earlier waves of the IFLS, where adult mental health was not measured.

borhood quality are potential mechanisms for our intergenerational findings. Our findings point to one important way in which policy makers can influence human capital outcomes even into the subsequent generation.

How important are these spillovers and do they justify the kinds of large-scale expenditures on school construction programs such as INPRES? We directly address this through a cost benefit analysis. Specifically, we calculate the real internal rate of return of the program both with and without taking into account the spillover effects. In order to make these calculations, we make several simplifying assumptions. For example, we make the conservative assumption that the INPRES schools were operational for twenty years and phased out by 1997.[51] For benefits, we only include the earnings and health gains for each generation. We use the results from existing studies in order to translate the magnitude of the health and educational improvements we observe in the second generation, on lifetime earnings. The complete details are available in Appendix C.

We estimate that the internal rate of return of the INPRES program is about 7.9% when only including the returns to primary school completion for the first generation. This is similar both to what Duflo (2001) finds for INPRES as well as what Aaronson and Mazumder (2011) find for the Rosenwald school construction program that took place in the rural American South early in the 20th century. When we include the long-term health improvements in the first generation, the estimated internal rate of return rises to about 8.8%.[52] When we further account for the intergenerational benefits (test scores and health), we find a vastly higher rate of return of as much as 24.8%. This suggests that traditional cost-benefit analyses of this type of intervention that only take into account the returns to education may significantly underestimate the societal benefits. These findings have highly salient policy implications for countries that are still struggling with access to basic education and are contemplating large-scale schooling interventions. Our results strongly suggest that these nations should take into account the potential spillover gains to health and to subsequent generations.

References

Aaronson, D. (1998). Using sibling data to estimate the impact of neighborhoods on children's educational outcomes. *Journal of human resources 33*(4), 915–946.

[51] Thus, cohorts born between 1963 and 1989 received benefits from the program.

[52] Akresh et al. (2018) estimate the internal rate of return based on the first generation's taxes and improved living standards. They find a rate of return between 10.5% and 20.7%.

Aaronson, D., F. Lange, and B. Mazumder (2014). Fertility transitions along the extensive and intensive margins. *American Economic Review 104*(11), 3701–24.

Aaronson, D. and B. Mazumder (2011). The impact of rosenwald schools on black achievement. *Journal of Political Economy 119*(5), 821–888.

Aaronson, D., B. Mazumder, S. G. Sanders, and E. J. Taylor (2017). Estimating the effect of school quality on mortality in the presence of migration: Evidence from the jim crow south. *SSRN Working Paper*.

Aaronson, D., B. Mazumder, and M. Stinson (2017). The intergenerational effects of the rosenwald schools.

Agüero, J. M. and M. Ramachandran (2018). The intergenerational transmission of schooling among the education-rationed. *Journal of Human Resources*, 0816–8143R.

Aizer, A., S. Eli, J. Ferrie, and A. Lleras-Muney (2016). The long-run impact of cash transfers to poor families. *American Economic Review 106*(4), 935–71.

Aizer, A., L. Stroud, and S. Buka (2016). Maternal stress and child outcomes: Evidence from siblings. *Journal of Human Resources 51*(3), 523–555.

Akee, R., W. Copeland, E. J. Costello, and E. Simeonova (2018). How does household income affect child personality traits and behaviors? *American Economic Review 108*(3), 775–827.

Akresh, R., D. Halim, and M. Kleemans (2018). Long-term and intergenerational effects of education: Evidence from school construction in indonesia. Technical report, National Bureau of Economic Research.

Alderman, H., J. Kim, and P. F. Orazem (2003). Design, evaluation, and sustainability of private schools for the poor: the pakistan urban and rural fellowship school experiments. *Economics of Education review 22*(3), 265–274.

Anukriti, S. and S. Dasgupta (2017). Marriage markets in developing countries. In *The Oxford Handbook of Women and the Economy.*

Ardington, C. and B. Gasealahwe (2014). Mortality in south africa: Socio-economic profile and association with self-reported health. *Development Southern Africa 31*(1), 127–145.

Ashraf, N., N. Bau, N. Nunn, and A. Voena (2018). Bride price and female education.

Bailey, M. J. and A. Goodman-Bacon (2015). The war on poverty's experiment in public medicine: Community health centers and the mortality of older americans. *American Economic Review 105*(3), 1067–1104.

Baird, S., E. Chirwa, J. de Hoop, and B. Özler (2014). Girl power: Cash transfers and adolescent welfare: Evidence from a cluster-randomized experiment in malawi. In *African Successes, Volume II: Human Capital*, pp. 139–164. University of Chicago Press.

Baird, S., C. McIntosh, and B. Özler (2011). Cash or condition? evidence from a cash transfer experiment. *The Quarterly journal of economics 126*(4), 1709–1753.

Barham, T. (2012). Enhancing cognitive functioning: Medium-term effects of a health and family planning program in matlab. *American Economic Journal: Applied Economics 4*(1), 245–73.

Barham, T., K. Macours, and J. A. Maluccio (2013). More schooling and more learning?: Effects of a three-year conditional cash transfer program in nicaragua after 10 years. Technical report, Inter-American Development Bank.

Barr, A. and C. Gibbs (2018). Breaking the cycle? the intergenerational effects of head start.

Becker, G. S. (1964). 1993. human capital. *A Theoretical and Empirical Analysis with Special References to Education*.

Becker, G. S. and H. G. Lewis (1973). On the interaction between the quantity and quality of children. *Journal of political Economy 81*(2, Part 2), S279–S288.

Behrman, J. R. and M. R. Rosenzweig (2002). Does increasing women's schooling raise the schooling of the next generation? *American Economic Review 92*(1), 323–334.

Berlinski, S., S. Galiani, and P. Gertler (2009). The effect of pre-primary education on primary school performance. *Journal of public Economics 93*(1-2), 219–234.

Bettinger, E., M. Kremer, M. Kugler, C. Medina, C. Posso, and J. E. Saavedra (2017). Can educational voucher programs pay for themselves?

Bharati, T., S. Chin, and D. Jung (2016). Does education affect time preference?

Bharati, T., S. Chin, and D. Jung (2018). Recovery from an early life shock through improved access to schools: Evidence from indonesia.

Black, D., S. Sanders, E. Taylor, and L. Taylor (2015). The impact of the great migration on mortality of african americans: Evidence from the deep south. *American Economic Review 105*(2), 477 – 503.

Black, S. E., P. J. Devereux, and K. G. Salvanes (2016). Does grief transfer across generations? bereavements during pregnancy and child outcomes. *American Economic Journal: Applied Economics 8*(1), 193–223.

Booth, A. (1998). *The Indonesian economy in the nineteenth and twentieth centuries: A history of missed opportunities*. Springer.

Breierova, L. and E. Duflo (2004). The impact of education on fertility and child mortality: Do fathers really matter less than mothers?

Burde, D. and L. L. Linden (2013). Bringing education to afghan girls: A randomized controlled trial of village-based schools. *American Economic Journal: Applied Economics 5*(3), 27–40.

Campbell, F., G. Conti, J. J. Heckman, S. H. Moon, R. Pinto, E. Pungello, and Y. Pan (2014). Early childhood investments substantially boost adult health. *Science 343*(6178), 1478–1485.

Carneiro, P. and R. Ginja (2014). Long-term impacts of compensatory preschool on health and behavior: Evidence from head start. *American Economic Journal: Economic Policy 6*(4), 135–73.

Chetty, R. and N. Hendren (2018). The impacts of neighborhoods on intergenerational mobility ii: County-level estimates. *The Quarterly Journal of Economics 133*(3), 1163–1228.

Chetty, R., N. Hendren, and L. F. Katz (2016). The effects of exposure to better neighborhoods on children: New evidence from the moving to opportunity experiment. *American Economic Review 106*(4), 855–902.

Chou, S.-Y., J.-T. Liu, M. Grossman, and T. Joyce (2010). Parental education and child health: evidence from a natural experiment in taiwan. *American Economic Journal: Applied Economics 2*(1), 33–61.

Clark, D. and H. Royer (2013). The effect of education on adult mortality and health: Evidence from britain. *American Economic Review 103*(6), 2087–2120.

Conti, G. and J. J. Heckman (2013). The developmental approach to child and adult health. *Pediatrics 131*(Supplement 2), S133–S141.

Crespo, L., B. López-Noval, and P. Mira (2014). Compulsory schooling, education, depression and memory: New evidence from sharelife. *Economics of Education Review 43*, 36–46.

Currie, J. and J. Gruber (1996a). Health insurance eligibility, utilization of medical care, and child health. *The Quarterly Journal of Economics 111*(2), 431–466.

Currie, J. and J. Gruber (1996b). Saving babies: the efficacy and cost of recent changes in the medicaid eligibility of pregnant women. *Journal of political Economy 104*(6), 1263–1296.

Currie, J. and E. Moretti (2003). Mother's education and the intergenerational transmission of human capital: Evidence from college openings. *The Quarterly journal of economics 118*(4), 1495–1532.

Cutler, D. M., A. Lleras-Muney, and T. Vogl (2011). Socioeconomic status and health: Dimensions and mechanisms. *The Oxford Handbook of Health Economics*.

Daroesman, R. (1972). Finance of education. part ii. *Bulletin of Indonesian Economic Studies 8*(1), 32–68.

Deming, D. (2009). Early childhood intervention and life-cycle skill development: Evidence from head start. *American Economic Journal: Applied Economics 1*(3), 111–34.

DeSalvo, K. B., V. S. Fan, M. B. McDonell, and S. D. Fihn (2005). Predicting mortality and healthcare utilization with a single question. *Health services research 40*(4), 1234–1246.

Deutscher, N. (2019). Place, peers, and the teenage years: Long-run neighbourhood effects in australia. *forthcoming at American Economic Journal: Applied Economics*.

Dinsa, G. D., Y. Goryakin, E. Fumagalli, and M. Suhrcke (2012). Obesity and socioeconomic status in developing countries: a systematic review. *Obesity reviews 13*(11), 1067–1079.

Duflo, E. (1999). *Essays in empirical development economics*. Ph. D. thesis, Massachusetts Institute of Technology.

Duflo, E. (2001). Schooling and labor market consequences of school construction in indonesia: Evidence from an unusual policy experiment. *American economic review 91*(4), 795–813.

Duflo, E. (2004). The medium run effects of educational expansion: Evidence from a large school construction program in indonesia. *Journal of Development Economics 74*(1), 163–197.

Dupas, P. (2011). Health behavior in developing countries. *Annu. Rev. Econ. 3*(1), 425–449.

Dursun, B., R. Cesur, and I. R. Kelly (2017). The value of mandating maternal education in a developing country. Technical report, National Bureau of Economic Research.

East, C. N., S. Miller, M. Page, and L. R. Wherry (2017). Multi-generational impacts of childhood access to the safety net: Early life exposure to medicaid and the next generationâs health.

Falkner, F. and J. Tanner (1986). Human growth; a comprehensive treatise. v. 1: Developmental biology, prenatal growth.-v. 2: Postnatal growth, neurobiology.-v. 3: Methodology, ecological, genetic, and nutritional effects on growth.

Fernald, L. C., P. J. Gertler, and L. M. Neufeld (2008). Role of cash in conditional cash transfer programmes for child health, growth, and development: an analysis of mexico's oportunidades. *The Lancet 371*(9615), 828–837.

Frankenberg, E. and N. R. Jones (2004). Self-rated health and mortality: does the relationship extend to a low income setting? *Journal of health and social behavior 45*(4), 441–452.

Friedman, W., M. Kremer, E. Miguel, and R. Thornton (2016). Education as liberation? *Economica 83*(329), 1–30.

Fuchs, V. R. (1980). Time preference and health: an exploratory study.

Galama, T., A. Lleras-Muney, and H. van Kippersluis (2018). The effect of education on health and mortality: A review of experimental and quasi-experimental evidence. In *Oxford Research Encyclopedia of Economics and Finance.*

Glewwe, P. and M. Kremer (2006). Schools, teachers, and education outcomes in developing countries. *Handbook of the Economics of Education 2*, 945–1017.

Glewwe, P. and K. Muralidharan (2016). Improving education outcomes in developing countries: Evidence, knowledge gaps, and policy implications. In *Handbook of the Economics of Education*, Volume 5, pp. 653–743. Elsevier.

Grépin, K. A. and P. Bharadwaj (2015). Maternal education and child mortality in zimbabwe. *Journal of health economics 44*, 97–117.

Grosh, M., P. Glewwe, et al. (2000). Designing household survey questionnaires for developing countries. *World Bank Publications*.

Grossman, M. (1972). On the concept of health capital and the demand for health. *Journal of Political economy 80*(2), 223–255.

Grossman, M. (2006). Education and nonmarket outcomes. *Handbook of the Economics of Education 1*, 577–633.

Grossman, M. (2015). The relationship between health and schooling: Whatâs new? Technical report, National Bureau of Economic Research.

Group, W. W. et al. (1986). Use and interpretation of anthropometric indicators of nutritional status. *Bulletin of the World Health Organization 64*(6), 929.

Hahn, Y., K. Nuzhat, and H.-S. Yang (2018). The effect of female education on marital matches and child health in bangladesh. *Journal of Population Economics 31*(3), 915–936.

Hanushek, E. A. and L. Woessmann (2015). *The knowledge capital of nations: Education and the economics of growth*. MIT press.

Havnes, T. and M. Mogstad (2011). No child left behind: Subsidized child care and children's long-run outcomes. *American Economic Journal: Economic Policy 3*(2), 97–129.

Heckman, J. J., J. E. Humphries, and G. Veramendi (2018). The nonmarket benefits of education and ability. *Journal of human capital 12*(2), 282–304.

Hoynes, H., D. W. Schanzenbach, and D. Almond (2016). Long-run impacts of childhood access to the safety net. *American Economic Review 106*(4), 903–34.

Huebener, M. (2017). Intergenerational effects of education on risky health behaviours and long-term health.

Idler, E. L. and Y. Benyamini (1997). Self-rated health and mortality: a review of twenty-seven community studies. *Journal of health and social behavior*, 21–37.

Karachiwalla, N. and G. Palloni (2019). *Human capital and structural transformation: Quasi-experimental evidence from Indonesia*, Volume 1836. Intl Food Policy Res Inst.

Kazianga, H., D. Levy, L. L. Linden, and M. Sloan (2013). The effects of" girl-friendly" schools: Evidence from the bright school construction program in burkina faso. *American Economic Journal: Applied Economics 5*(3), 41–62.

Kenkel, D. S. (1991). Health behavior, health knowledge, and schooling. *Journal of Political Economy 99*(2), 287–305.

Kim, J. (2010). Womenâs education and fertility: An analysis of the relationship between education and birth spacing in indonesia. *Economic development and cultural change 58*(4), 739–774.

Kling, J. R., J. B. Liebman, and L. F. Katz (2007). Experimental analysis of neighborhood effects. *Econometrica 75*(1), 83–119.

Kose, E. (2019). The intergenerational health effects of head start.

Lindeboom, M., A. Llena-Nozal, and B. van Der Klaauw (2009). Parental education and child health: Evidence from a schooling reform. *Journal of health Economics 28*(1), 109–131.

Lundborg, P., A. Nilsson, and D.-O. Rooth (2014). Parental education and offspring outcomes: evidence from the swedish compulsory school reform. *American Economic Journal: Applied Economics 6*(1), 253–78.

Mackenbach, J. P., I. Stirbu, A.-J. R. Roskam, M. M. Schaap, G. Menvielle, M. Leinsalu, and A. E. Kunst (2008). Socioeconomic inequalities in health in 22 european countries. *New England Journal of Medicine 358*(23), 2468–2481. PMID: 18525043.

Martinez-Bravo, M. (2017). The local political economy effects of school construction in indonesia. *American Economic Journal: Applied Economics 9*(2), 256–89.

Mason, J. (1990). Measuring children: the uses of anthropometry. *SCN News 5*, 8–19.

Mazumder, B. (2008). Does education improve health? a reexamination of the evidence from compulsory schooling laws. *Economic Perspectives 32*(2), 2–17.

Mazumder, B. (2012). The effects of education on health and mortality. *Nordic Economic Policy Review 1*(2012), 261–301.

Mazumder, B., M. Rosales-Rueda, and M. Triyana (2019). Intergenerational human capital spillovers: Indonesia's school construction and its effects on the next generation. *Forthcoming at AEA: Papers and Proceedings*.

Miilunpalo, S., I. Vuori, P. Oja, M. Pasanen, and H. Urponen (1997). Self-rated health status as a health measure: the predictive value of self-reported health status on the use of physician services and on mortality in the working-age population. *Journal of clinical epidemiology 50*(5), 517–528.

Miller, S. and L. R. Wherry (2018). The long-term effects of early life medicaid coverage. *Journal of Human Resources*, 0816_8173R1.

Nores, M. and W. S. Barnett (2010). Benefits of early childhood interventions across the world:(under) investing in the very young. *Economics of education review 29*(2), 271–282.

Oreopoulos, P. (2007). Do dropouts drop out too soon? wealth, health and happiness from compulsory schooling. *Journal of public Economics 91*(11-12), 2213–2229.

Oreopoulos, P. and K. G. Salvanes (2011). Priceless: The nonpecuniary benefits of schooling. *Journal of Economic perspectives 25*(1), 159–84.

Parker, S. W. and T. Vogl (2018). Do conditional cash transfers improve economic outcomes in the next generation? evidence from mexico. Technical report, National Bureau of Economic Research.

Perez-Alvarez, M. (2017). Returns to cognitive skills in 7 developing countries.

Persson, P. and M. Rossin-Slater (2018). Family ruptures, stress, and the mental health of the next generation. *American economic review 108*(4-5), 1214–52.

Psacharopoulos, G. and H. A. Patrinos* (2004). Returns to investment in education: a further update. *Education economics 12*(2), 111–134.

Razzaque, A., A. Mustafa, and P. K. Streatfield (2014). Do self-reported health indicators predict mortality? evidence from matlab, bangladesh. *Journal of biosocial science 46*(5), 621–634.

Rossin-Slater, M. and M. Wüst (2018). What is the added value of preschool? long-term impacts and interactions with an infant health intervention.

Schultz, T. W. (1961). Investment in human capital. *The American economic review*, 1–17.

Simes, R. J. (1986). An improved bonferroni procedure for multiple tests of significance. *Biometrika 73*(3), 751–754.

Sohn, K. (2015). The height premium in indonesia. *Economics & Human Biology 16*, 1–15.

Sohn, K. (2017). The fatter are happier in indonesia. *Quality of Life Research 26*(2), 393–402.

Strauss, J., F. Witoelar, B. Sikoki, and A. Wattie (2016). *User's Guide for the Indonesia Family Life Survey, Wave 5*. RAND Santa Monica.

Tadjoeddin, M. Z. and A. Chowdhury (2017). *Employment and Re-industrialisation in Post Soeharto Indonesia*. Springer.

Thomas, D., J. Strauss, and M.-H. Henriques (1991). How does mother's education affect child height? *The journal of human resources 26*(2), 183.

UNESCO (2015). *Education for All 2000-2015: achievements and challenges; EFA global monitoring report, 2015*. UNESCO publishing.

UNICEF, W. (2018). *DRINKING WATER, SANITATION AND HYGIENE IN SCHOOLS Global baseline report 2018*.

Walker, S. P., T. D. Wachs, S. Grantham-McGregor, M. M. Black, C. A. Nelson, S. L. Huffman, H. Baker-Henningham, S. M. Chang, J. D. Hamadani, B. Lozoff, et al. (2011). Inequality in early childhood: risk and protective factors for early child development. *The lancet 378*(9799), 1325–1338.

Wantchekon, L., M. Klašnja, and N. Novta (2014). Education and human capital externalities: evidence from colonial benin. *The Quarterly Journal of Economics 130*(2), 703–757.

World Bank (1989). Indonesia: Basic education study.

World Bank (1990). Indonesia: Strategy for a sustained reduction in poverty.

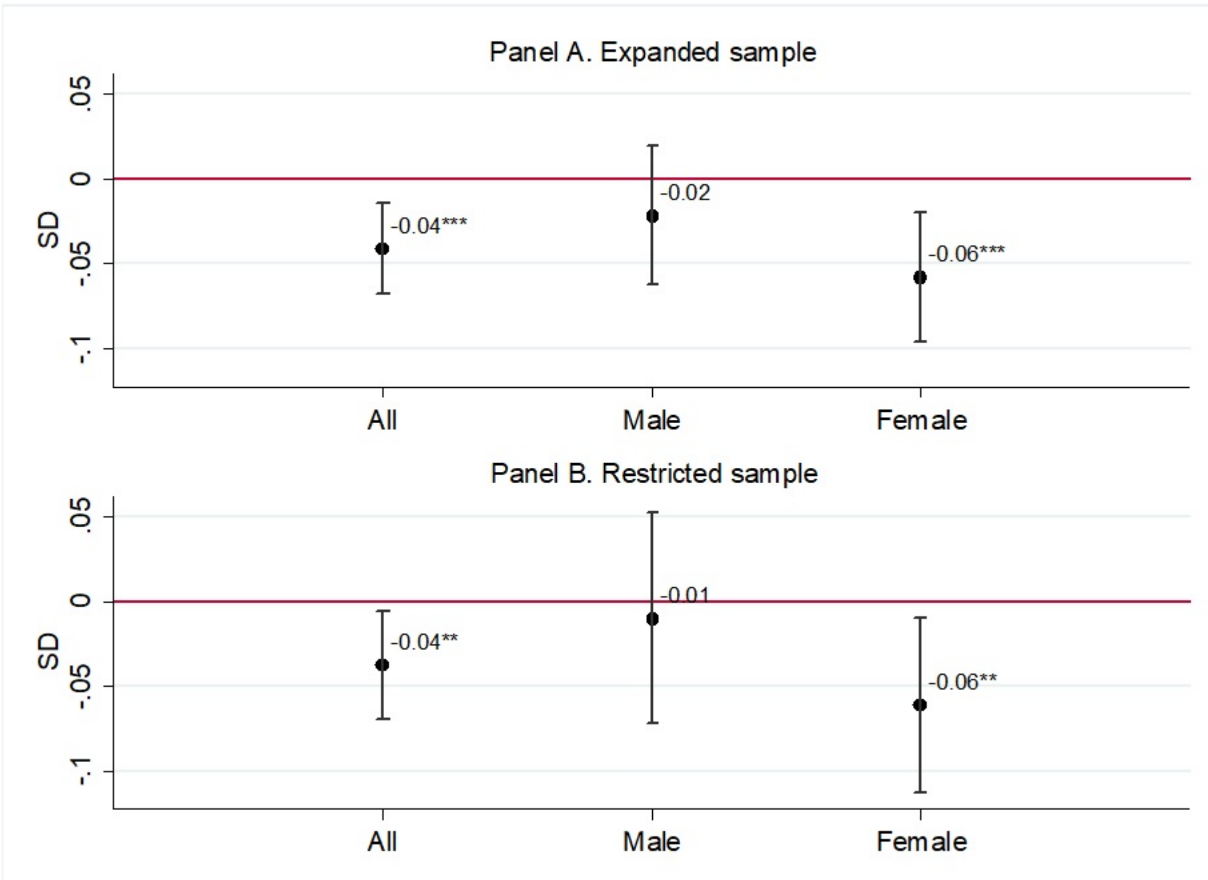
Yamauchi, F. (2012). Prenatal seasonality, child growth, and schooling investments: Evidence from rural indonesia. *Journal of Development Studies 48*(9), 1323–1341.

Zha, D. (2019). Schooling expansion and marriage market: Evidence from indonesia.

Tables and Figures

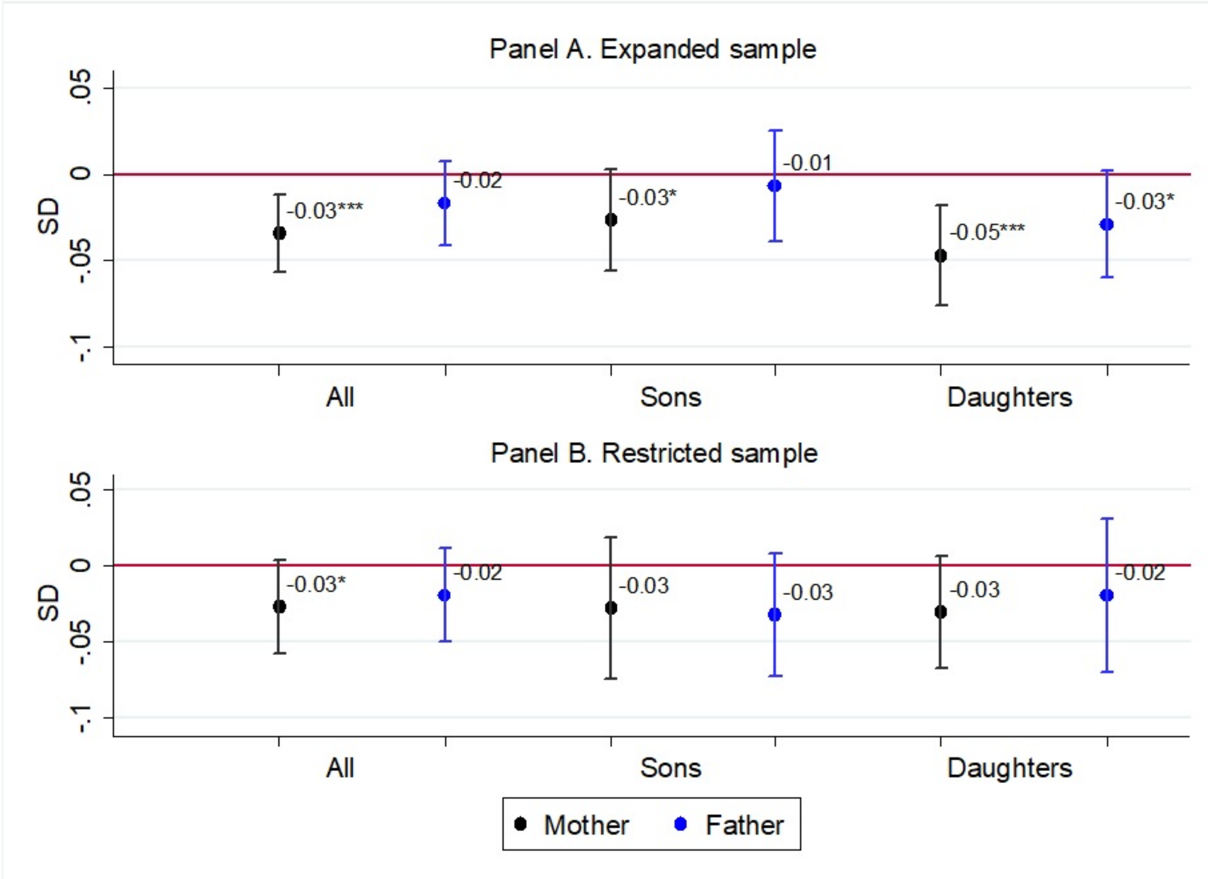
First generation estimations

Figure 1: Long-term outcome: program effect on poor health index



Notes: Overall health index corresponds to a summary index from the multiple self-reported health measures analyzed: self-reported poor general health, number of days missed, any chronic conditions, number of conditions, and mental health screening score where higher scores correspond to more symptoms of depression. The index has mean 0, SD 1 based on those born between 1950-1962 in low INPRES areas. Expanded sample includes those born between 1950-1972. Restricted sample includes those born between 1957-1962 or 1968-1972. Covariates include: district, year of birth, month of birth, ethnicity (Javanese dummy), birth-year interacted with: the number of school-aged children in the district in 1971 (before the start of the program), the enrollment rate of the district in 1971 and the exposure of the district to another INPRES program: a water and sanitation program. Robust standard errors clustered at district of birth. Bars indicate the 95% confidence intervals. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Figure 2: Intergenerational outcome: effect on poor health index



Notes: Overall health index corresponds to a summary index from the following health measures for the second generation: being stunted, anemic and self-reported poor health. Sample corresponds to children born to first generation INPRES individuals. Expanded sample includes children born to adults born between 1950-1972. Restricted sample includes children born to adults born between 1957-1962 or 1968-1972. Covariates include the following FE: parent year of birth×1971 enrollment, parent year of birth×1971 number of children, parent year of birth×water sanitation program, child's gender, birth order, year and month of birth dummies, urban, and ethnicity (Javanese dummy). Because some children may be observed multiple times, robust standard errors in parentheses two-way clustered at the parent's district of birth and individual level. Bars indicate the 95% confidence intervals. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 1: First generation long-term outcomes: Self-reported health

	(1)	(2)	(3)	(4)	(5)	(6)
	All	Male	Female	All	Male	Female
		Healthy			Days missed	
			Panel A: Expanded sample			
Born bet. 1963-1972	0.039***	0.017	0.062***	-0.171**	-0.116	-0.205*
× INPRES	(0.010)	(0.013)	(0.014)	(0.068)	(0.086)	(0.112)
No. of obs.	10792	5296	5496	10420	5140	5280
Dep. var. mean	0.718	0.757	0.681	1.840	1.580	2.093
R-squared	0.08	0.13	0.10	0.06	0.09	0.08
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Male	Female	All	Male	Female
		Healthy			Days missed	
			Panel B: Restricted sample			
Born bet. 1968-1972	0.033***	0.006	0.060***	-0.079	-0.025	-0.102
× INPRES	(0.012)	(0.019)	(0.017)	(0.102)	(0.125)	(0.179)
No. of obs.	5999	2930	3069	5826	2861	2965
Dep. var. mean	0.743	0.785	0.704	1.765	1.504	2.017
R-squared	0.09	0.14	0.12	0.07	0.12	0.12

Notes: Healthy takes the value one if a respondent reports being 'Very healthy' or 'Healthy' (cols. 1-3). 'Days missed' corresponds to the number of days a respondent missed his or her activities in the past 4 weeks prior to the survey (cols. 4-6). Expanded sample includes those born between 1950-1972. Restricted sample includes those born between 1957-1962 or 1968-1972. Covariates include: district, year of birth, month of birth, ethnicity (Javanese dummy), birth-year interacted with: the number of school-aged children in the district in 1971 (before the start of the program), the enrollment rate of the district in 1971 and the exposure of the district to another INPRES program: a water and sanitation program. Robust standard errors clustered at district of birth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 2: First generation long-term outcomes: Chronic conditions

	(1)	(2)	(3)	(4)	(5)	(6)
	All	Male	Female	All	Male	Female
	Any condition			No. of conditions		
	Panel A. Expanded sample					
Born bet. 1963-1972	-0.025**	-0.015	-0.039***	-0.047**	-0.030	-0.075**
× INPRES	(0.011)	(0.020)	(0.014)	(0.019)	(0.026)	(0.034)
No. of obs.	10729	5266	5463	10729	5266	5463
Dep. var. mean	0.422	0.350	0.491	0.684	0.550	0.812
R-squared	0.09	0.10	0.10	0.10	0.12	0.11
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Male	Female	All	Male	Female
	Any condition			No. of conditions		
	Panel B. Restricted sample					
Born bet. 1968-1972	-0.014	0.003	-0.032*	-0.037	-0.009	-0.072*
× INPRES	(0.017)	(0.031)	(0.018)	(0.024)	(0.040)	(0.040)
No. of obs.	5940	2900	3040	5940	2900	3040
Dep. var. mean	0.397	0.326	0.464	0.626	0.478	0.766
R-squared	0.10	0.13	0.13	0.11	0.14	0.14

Notes: Expanded sample includes those born between 1950-1972. Restricted sample includes those born between 1957-1962 or 1968-1972. Covariates include: district, year of birth, month of birth, ethnicity (Javanese dummy), birth-year interacted with: the number of school-aged children in the district in 1971 (before the start of the program), the enrollment rate of the district in 1971 and the exposure of the district to another INPRES program: a water and sanitation program. Robust standard errors clustered at district of birth. Significance: *** p < 0.01, ** p < 0.05, * p < 0.10.

Table 3: First generation long-term outcomes: Mental health

	(1) All	(2) Male	(3) Female
	Panel A. Expanded sample		
Born bet. 1963-1972 × INPRES	-0.180	-0.109	-0.235
	(0.141)	(0.188)	(0.238)
No. of obs.	10244	4962	5282
Dep. var. mean	5.515	5.245	5.769
R-squared	0.09	0.12	0.12

	(1) All	(2) Male	(3) Female
	Panel B. Restricted sample		
Born bet. 1968-1972 × INPRES	-0.271*	0.015	-0.520**
	(0.153)	(0.281)	(0.214)
No. of obs.	5741	2751	2990
Dep. var. mean	5.550	5.278	5.800
R-squared	0.12	0.16	0.15

Notes: Mental health score is based on the following items: being bothered by things, having trouble concentrating, feeling depressed, feeling like everything was an effort, feeling hopeful about the future, feeling fearful, having restless sleep, feeling happy, lonely, and unable to get going. Higher scores correspond to poorer mental health. Expanded sample includes those born between 1950-1972. Restricted sample includes those born between 1957-1962 or 1968-1972. Covariates include: district, year of birth, month of birth, ethnicity (Javanese dummy), birth-year interacted with: the number of school-aged children in the district in 1971 (before the start of the program), the enrollment rate of the district in 1971 and the exposure of the district to another INPRES program: a water and sanitation program. Robust standard errors clustered at district of birth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Second generation estimations

Table 4: Intergenerational outcomes: National Secondary Test Scores

	(1) All	(2) Sons	(3) Daughters	(4) All	(5) Sons	(6) Daughters
	Panel A: Expanded sample					
Mother bet. 1963-72 × INPRES	0.083***	0.091	0.063			
	(0.031)	(0.056)	(0.044)			
Father bet. 1963-72 × INPRES				0.047	0.025	0.070
				(0.033)	(0.051)	(0.071)
No. of obs.	6819	3419	3400	5744	2846	2898
R-squared	0.11	0.16	0.16	0.11	0.17	0.17

	(1) All	(2) Sons	(3) Daughters	(4) All	(5) Sons	(6) Daughters
	Panel B: Restricted sample					
Mother bet. 1968-72 × INPRES	0.105*	0.042	0.185**			
	(0.059)	(0.105)	(0.073)			
Father bet. 1968-72 × INPRES				0.103	0.020	0.139
				(0.065)	(0.100)	(0.106)
No. of obs.	3512	1727	1785	2639	1294	1345
R-squared	0.14	0.23	0.23	0.17	0.27	0.30

Notes: Test scores standardized for each exam year. Sample corresponds to children born to first generation INPRES individuals. Expanded sample includes children born to adults born between 1950-1972. Restricted sample includes children born to adults born between 1957-1962 or 1968-1972. Covariates include the following FE: parent year of birth and district of birth fixed effects, parent year of birth×1971 enrollment, parent year of birth×1971 number of children, parent year of birth×water sanitation program, child's gender, birth order, year and month of birth dummies, ethnicity (Javanese dummy), examination year dummies. Robust standard errors in parentheses clustered at the parent's district of birth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 5: Intergenerational outcomes: Height-for-age

	(1) All	(2) Sons	(3) Daughters	(4) All	(5) Sons	(6) Daughters
	Panel A: Expanded sample					
Mother bet. 1963-72 × INPRES	0.056*	0.036	0.081**			
	(0.030)	(0.041)	(0.033)			
Father bet. 1963-72 × INPRES				-0.016	-0.037	-0.001
				(0.029)	(0.040)	(0.036)
No. of obs.	21382	10841	10534	19890	10079	9798
Dep. var. mean	-1.646	-1.653	-1.639	-1.613	-1.622	-1.605
R-squared	0.16	0.20	0.18	0.17	0.21	0.19

	(1) All	(2) Sons	(3) Daughters	(4) All	(5) Sons	(6) Daughters
	Panel B: Restricted sample					
Mother bet. 1968-72 × INPRES	0.044	0.008	0.074			
	(0.046)	(0.062)	(0.049)			
Father bet. 1968-72 × INPRES				-0.055	-0.057	-0.047
				(0.049)	(0.060)	(0.058)
No. of obs.	11464	5820	5632	10007	5108	4875
Dep. var. mean	-1.629	-1.634	-1.624	-1.606	-1.622	-1.591
R-squared	0.19	0.24	0.21	0.20	0.26	0.21

Notes: Sample corresponds to children born to first generation INPRES individuals. Expanded sample includes children born to adults born between 1950-1972. Restricted sample includes children born to adults born between 1957-1962 or 1968-1972. Covariates include the following FE: parent year of birth× 1971 enrollment, parent year of birth× 1971 number of children, parent year of birth× water sanitation program, child's gender, birth order, year and month of birth dummies, urban, and ethnicity (Javanese dummy). Because some children may be observed multiple times, robust standard errors in parentheses two-way clustered at the parent's district of birth and individual level. Significance: *** p < 0.01, ** p < 0.05, * p < 0.10.

Table 6: Intergenerational outcomes: Stunting

	(1) All	(2) Sons	(3) Daughters	(4) All	(5) Sons	(6) Daughters
	Panel A: Expanded sample					
Mother bet. 1963-72 × INPRES	-0.025*	-0.033**	-0.018			
	(0.013)	(0.017)	(0.016)			
Father bet. 1963-72 × INPRES				-0.009	-0.005	-0.013
				(0.012)	(0.015)	(0.016)
No. of obs.	21382	10841	10534	19890	10079	9798
Dep. var. mean	0.365	0.376	0.354	0.357	0.368	0.346
R-squared	0.12	0.16	0.13	0.13	0.16	0.14

	(1) All	(2) Sons	(3) Daughters	(4) All	(5) Sons	(6) Daughters
	Panel B: Restricted sample					
Mother bet. 1968-72 × INPRES	-0.022	-0.021	-0.021			
	(0.020)	(0.023)	(0.026)			
Father bet. 1968-72 × INPRES				-0.007	-0.014	-0.005
				(0.017)	(0.019)	(0.025)
No. of obs.	11464	5820	5632	10007	5108	4875
Dep. var. mean	0.365	0.379	0.350	0.356	0.371	0.341
R-squared	0.14	0.19	0.16	0.15	0.20	0.17

Notes: Stunting takes the value one if a child's height for age is more than two standard deviations below the mean. Expanded sample includes children born to adults born between 1950-1972. Restricted sample includes children born to adults born between 1957-1962 or 1968-1972. Covariates include the following FE: parent year of birth× 1971 enrollment, parent year of birth× 1971 number of children, parent year of birth× water sanitation program, child's gender, birth order, year and month of birth dummies, urban, and ethnicity (Javanese dummy). Because some children may be observed multiple times, robust standard errors in parentheses two-way clustered at the parent's district of birth and individual level. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 7: Intergenerational outcomes: Anemia

	(1) All	(2) Sons	(3) Daughters	(4) All	(5) Sons	(6) Daughters
			Panel A: Expanded sample			
Mother bet. 1963-72 × INPRES	-0.010	0.015	-0.034**			
	(0.011)	(0.013)	(0.015)			
Father bet. 1963-72 × INPRES				-0.009	-0.002	-0.022*
				(0.010)	(0.014)	(0.012)
No. of obs.	18104	9233	8860	17535	8940	8580
Dep. var. mean	0.251	0.224	0.279	0.244	0.219	0.269
R-squared	0.07	0.10	0.09	0.07	0.11	0.09

	(1) All	(2) Sons	(3) Daughters	(4) All	(5) Sons	(6) Daughters
			Panel B: Restricted sample			
Mother bet. 1968-72 × INPRES	-0.011	0.006	-0.026			
	(0.014)	(0.018)	(0.021)			
Father bet. 1968-72 × INPRES				-0.018	-0.023	-0.034**
				(0.012)	(0.018)	(0.017)
No. of obs.	9994	5113	4867	9084	4674	4386
Dep. var. mean	0.247	0.225	0.271	0.239	0.214	0.266
R-squared	0.09	0.13	0.13	0.09	0.14	0.12

Notes: Measures of hemoglobin started in IFLS2. Expanded sample includes children born to adults born between 1950-1972. Restricted sample includes children born to adults born between 1957-1962 or 1968-1972. Covariates include the following FE: parent year of birth× 1971 enrollment, parent year of birth× 1971 number of children, parent year of birth× water sanitation program, child's gender, birth order, year and month of birth dummies, urban, and ethnicity (Javanese dummy). Because some children may be observed multiple times, robust standard errors in parentheses two-way clustered at the parent's district of birth and individual level. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 8: Intergenerational outcomes: Self-reported health

	(1) All	(2) Sons	(3) Daughters	(4) All	(5) Sons	(6) Daughters
	Panel A: Expanded sample					
Mother bet. 1963-72 ×INPRES	0.012**	0.015**	0.013*			
	(0.005)	(0.007)	(0.007)			
Father bet. 1963-72 × INPRES				0.004	0.003	0.004
				(0.005)	(0.008)	(0.007)
No. of obs.	18648	9499	9140	18171	9247	8905
Dep. var. mean	0.875	0.882	0.869	0.889	0.895	0.883
R-squared	0.31	0.34	0.32	0.25	0.27	0.26

	(1) All	(2) Sons	(3) Daughters	(4) All	(5) Sons	(6) Daughters
	Panel B: Restricted sample					
Mother bet. 1968-72 × INPRES	0.005	0.016**	0.000			
	(0.006)	(0.007)	(0.009)			
Father bet. 1968-72 × INPRES				-0.005	-0.004	-0.012
				(0.006)	(0.009)	(0.009)
No. of obs.	10319	5273	5029	9423	4833	4561
Dep. var. mean	0.884	0.890	0.877	0.904	0.908	0.900
R-squared	0.29	0.32	0.31	0.19	0.20	0.23

Notes: Self-reported health questions for children less than age 15 started in IFLS2. Expanded sample includes children born to adults born between 1950-1972. Restricted sample includes children born to adults born between 1957-1962 or 1968-1972. Covariates include the following FE: parent year of birth× 1971 enrollment, parent year of birth× 1971 number of children, parent year of birth× water sanitation program, child's gender, birth order, year and month of birth dummies, urban, and ethnicity (Javanese dummy). Because some children may be observed multiple times, robust standard errors in parentheses two-way clustered at the parent's district of birth and individual level. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 9: Intergenerational outcomes: Both parents' exposure

	(1)	(2)	(3)	(4)	(5)	(6)
	National secondary test scores			Health index		
	All	Sons	Daughters	All	Sons	Daughters
	Panel A: Expanded sample					
Mother Born bet. 1963-72	0.111***	0.145**	0.072	-0.037***	-0.020	-0.054***
× INPRES	(0.036)	(0.062)	(0.054)	(0.014)	(0.018)	(0.019)
Father Born bet. 1963-72	0.006	-0.018	0.004	-0.004	-0.019	0.011
× INPRES	(0.038)	(0.058)	(0.060)	(0.015)	(0.021)	(0.019)
No. of obs.	6619	3303	3316	19042	9771	9264
R-squared	0.14	0.21	0.20	0.17	0.20	0.19

	(1)	(2)	(3)	(4)	(5)	(6)
	All	Sons	Daughters	All	Sons	Daughters
	Panel B: Restricted sample					
Mother Born bet. 1968-72	0.110**	0.030	0.180**	-0.024	-0.017	-0.041*
× INPRES	(0.051)	(0.091)	(0.078)	(0.017)	(0.022)	(0.021)
Father Born bet. 1968-72	0.040	-0.006	-0.024	-0.010	-0.008	-0.009
× INPRES	(0.047)	(0.076)	(0.070)	(0.016)	(0.022)	(0.023)
No. of obs.	4381	2166	2215	13707	7073	6617
R-squared	0.17	0.28	0.27	0.17	0.20	0.20

Notes: Test scores standardized for each exam year. Overall health index corresponds to a summary index from the following health measures for the second generation: being stunted, anemic and reporting poor health. Sample corresponds to children born to first generation INPRES individuals. Alternative estimation of the effect of maternal INPRES exposure, where we include the father's cohort of birth (2 year bins), father's province of birth FE and the interaction of father's district of birth characteristics and father's cohort indicators. Additional covariates include the following FE: mother's year of birth and district of birth fixed effects, mother's year of birth×1971 enrollment, mother's year of birth×1971 number of children, mother's year of birth×water sanitation program, child's gender, birth order, year and month of birth dummies, urban, ethnicity (Javanese dummy). Analysis includes examination year dummies for test scores (cols. 1-3) and wave FE for health outcomes (cols. 4-6). Robust standard errors in parentheses clustered at the mother's district of birth for cols. 1-3 and two-way clustered at the mother's district of birth and individual level for cols. 4-6. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Potential mechanisms

Table 10: Potential mechanisms: Marriage outcomes of first generation

	(1)	(2)	(3)	(4)	(5)	(6)
	Female: Spouse			Male: Spouse		
	Primary completion	Secondary completion	Age difference Husband−wife	Primary completion	Secondary completion	Age difference Wife−husband
			Panel A: Expanded sample			
Female bet. 1963-72	0.006	0.018	0.197			
× INPRES	(0.013)	(0.016)	(0.271)			
Male bet. 1963-72				0.007	-0.025	0.079
× INPRES				(0.013)	(0.016)	(0.153)
No. of obs.	5884	5884	5443	5989	5989	5999
Dep. var. mean	0.78	0.48	4.93	0.80	0.47	-5.14
R-squared	0.19	0.24	0.11	0.22	0.26	0.09

	Female: Spouse			Male: Spouse		
	Primary completion	Secondary completion	Age difference Husband−wife	Primary completion	Secondary completion	Age difference Wife−husband
			Panel B: Restricted sample			
Female bet. 1968-72	0.032**	0.052**	0.326			
× INPRES	(0.016)	(0.024)	(0.362)			
Male bet. 1968-72				0.008	-0.012	-0.191
× INPRES				(0.019)	(0.021)	(0.218)
No. of obs.	3226	3226	3004	3281	3281	3290
Dep. var. mean	0.81	0.52	4.80	0.83	0.51	-4.89
R-squared	0.21	0.25	0.14	0.25	0.27	0.11

Notes: Primary completion corresponds to 6 years of education, secondary completion corresponds to 9 years of education, and age difference is in years. Expanded sample includes those born between 1950-1972. Restricted sample includes those born between 1957-1962 or 1968-1972. Covariates include: district, year of birth, month of birth, ethnicity (Javanese dummy), birth-year interacted with: the number of school-aged children in the district in 1971 (before the start of the program), the enrollment rate of the district in 1971 and the exposure of the district to another INPRES program: a water and sanitation program. Robust standard errors clustered at district of birth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 11: Potential mechanisms: Household resources

	(1)	(2)	(3)	(4)	(5)	(6)
	Per capita expenditure			Poor housing		
	All	Men	Women	All	Men	Women
	Panel A. Expanded sample					
Born bet. 1963-1972	0.046**	0.044*	0.051	-0.031*	-0.036	-0.031
× INPRES	(0.018)	(0.023)	(0.034)	(0.018)	(0.025)	(0.022)
No. of obs.	11941	6007	5934	11507	5785	5722
Dep. var. mean	12.860	12.912	12.806	-0.038	-0.045	-0.030
R-squared	0.15	0.15	0.19	0.38	0.38	0.42
	Panel B. Restricted sample					
Born bet. 1968-1972	0.054**	0.058*	0.060	-0.040*	-0.028	-0.070***
× INPRES	(0.026)	(0.033)	(0.042)	(0.020)	(0.036)	(0.023)
No. of obs.	6752	3408	3344	6502	3276	3226
Dep. var. mean	12.899	12.938	12.858	-0.042	-0.040	-0.044
R-squared	0.15	0.15	0.21	0.38	0.40	0.40

Notes: Total log per capita expenditure in 2012-14 based on weekly or monthly per capita food and non-food expenditure in 2012 *Rupiah*). We exclude annual non-food expenditure (which includes items like land/vehicle purchases). Index of poor housing quality in 2012-14 includes: poor toilet, poor floor, poor roof, poor wall, high occupancy per room. Poor toilet is captured by not having access to a toilet (including shared or public toilet). Poor floor includes board/lumber, bamboo, or dirt floor. Poor roof includes leaves or wood. Poor wall includes lumber/board and bamboo/mat. Occupancy per room is defined as more than two persons per room in the house (based on household size). Expanded sample includes those born between 1950-1972. Restricted sample includes those born between 1957-1962 or 1968-1972. Covariates include: district, year of birth, month of birth, ethnicity (Javanese dummy), birth-year interacted with: the number of school-aged children in the district in 1971 (before the start of the program), the enrollment rate of the district in 1971 and the exposure of the district to another INPRES program: a water and sanitation program. Robust standard errors clustered at district of birth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

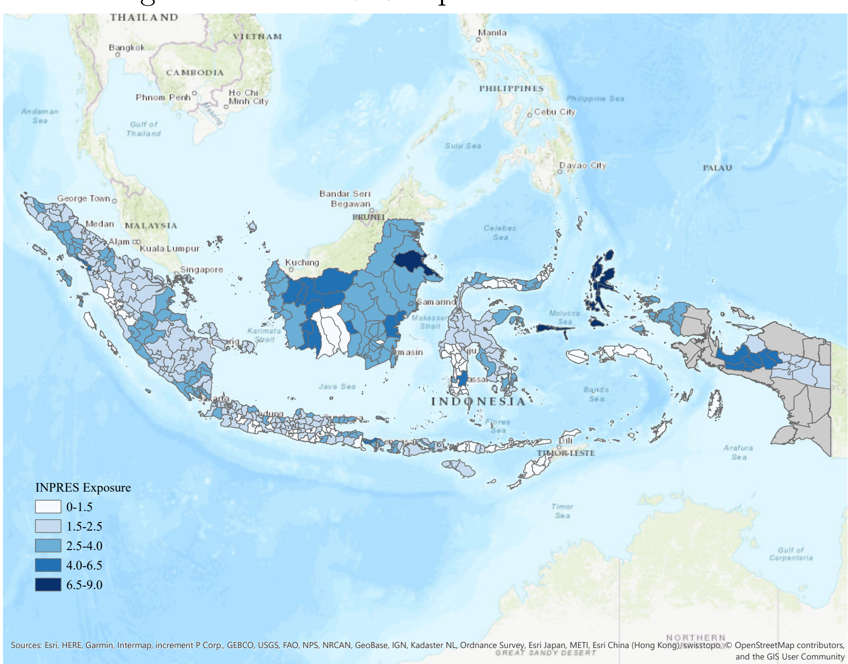
Appendix

A Data Appendix

A.1 Coverage of the IFLS and INPRES program

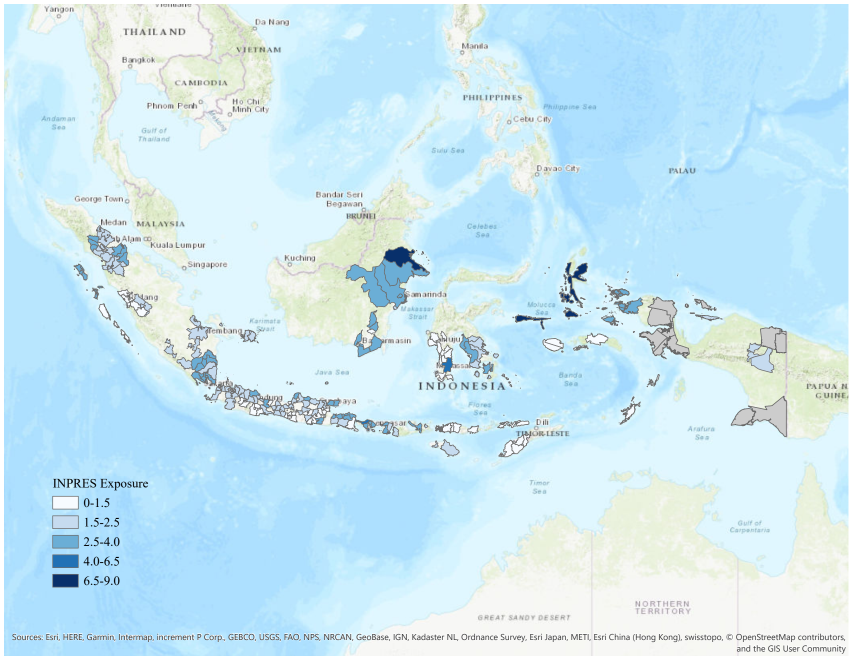
We compare the intensity of the INPRES school construction project in the IFLS and IFLS-E against the national record. The IFLS provinces include 13 out of Indonesia's 26 provinces in 1993. They include: North Sumatra, West Sumatra, South Sumatra, Lampung, Jakarta, West Java, Central Java, Yogyakarta, East Java, Bali, West Nusa Tenggara, South Kalimantan, and South Sulawesi. The IFLS-E provinces include the following 7 provinces in 2012: East Nusa Tenggara, East Kalimantan, Southeast Sulawesi, Maluku, North Maluku, West Papua, and Papua. The IFLS and IFLS-E include almost 300 of Indonesia's 519 districts. A.1shows the intensity of the INRES program at the national level while A.2shows the intensity of the INPRES program in the IFLS and IFLS-E districts. A comparison of Figures A.1andA.2 shows that the IFLS and IFLS-E include both high and low intensity program districts.

Figure A.1: INPRES exposure - All Indonesia



Source: Authors' calculations based on Duflo (2001)

Figure A.2: INPRES exposure in the IFLS and IFLS-E districts



Source: Authors' calculations based on the IFLS, IFLS-E, and Duflo (2001)

A.2 Data construction

Indonesia is administratively divided into provinces, districts (regencies or cities), sub-districts, and villages in rural areas or townships in urban areas. The IFLS over sampled urban and rural areas outside of the main island of Java. IFLS-1 included 7,224 households residing in 13 of Indonesia's 26 provinces in 1993. These households resided in approximately 200 districts, which corresponded to 321 enumeration areas in 312 communities. A community is defined as a village in rural areas and a township in urban areas. The IFLS-E includes 2,500 households residing in seven provinces in eastern Indonesia, which corresponded to about 50 districts and 99 communities. Households in the main IFLS and IFLS-E resided in almost 300 of Indonesia's 514 districts.

Date and district of birth

To obtain the sample of first generation individuals, we begin by identifying individuals who were born between 1950 and 1972 in the IFLS and IFLS-E. In each wave, the IFLS household roster includes information on date of birth (month and year). Also, the IFLS asks respondents over the age of 15 their place of birth in the wave in which they first join the survey. Indonesia experienced district proliferation over time, so we match each district to the 1993 district code in IFLS1. INPRES school construction in the district, water and sanitation program, enrollment in 1971, number of school-aged children in 1971: We obtain these variables from Duflo (2001).

Linking the first and second generation

To identify the second generation, who are the children of the first generation individuals, we use the household relationship in the household roster and women's birth history, matched to the household roster. In each wave, the survey includes an individual's relationship to the head of the household, and an identifier for an individual's mother and father if the mother and father are in the same household. The IFLS also includes a woman's birth history, which allows us to match mothers to their children, and subsequently to children's outcomes.

Long-term Outcomes for the first generation

Good self-reported health takes the value one if a respondent reported his or her self status as 'very healthy' or 'healthy'. The literature in epidemiology has established that self-reported health status is a valid and comprehensive health measure that is highly predictive of well-known health markers such as mortality (DeSalvo et al., 2005; Idler and Benyamini, 1997; Miilunpalo et al., 1997). This has also been demonstrated by some studies in developing countries that find that self-reported health status is predictive of mortality even after controlling for socio-demographic factors (Ardington and Gasealahwe, 2014; Razzaque et al., 2014). As additional adult health outcomes, we include the number of days a respondent missed his or her activities in the past 4 weeks prior to the survey. Respondents were also asked to report diagnosed chronic conditions, and we use an indicator for any condition as well as the number of conditions. These conditions include: hypertension, diabetes, tuberculosis, asthma, other respiratory conditions, stroke, heart disease, liver condition, cancer, arthritis, high cholesterol, depression/psychiatric condition.

To assess mental health, respondents were administered a series of 10 questions on how frequently they experienced symptoms of depression using the CES-D. The items include being bothered by things, having trouble concentrating, feeling depressed, feeling like everything was an effort, feeling hopeful about the future, feeling fearful, having restless sleep, feeling happy, lonely, and unable to get going. Each item includes 4 possible responses: rarely or none in the past week, 1-2 days, 3-4 days, 5-7 days. The intensity of each negative symptom is scored from 0 (rarely or none) to 3 (5-7 days a week). We recode feeling hopeful about the future and feeling happy to reflect the negative symptoms. We use the sum of the scores based on reported symptoms, where higher scores indicate a higher likelihood of having depression.

Intergenerational Outcomes

Using children's height and age, we calculate height-for-age z-score using the WHO reference data.[53] Stunting takes the value one if a child's height-for-age is more than two standard deviations below the mean. Using children's hemoglobin count, sex, and age, we identify children with anemia. Specifically, anemia is defined as having a count of less than 11.5 grams of hemoglobin per deciliter (gr/dL) for children under 12 years of age. For children between the ages of 12 and 15, the threshold is 12 gr/dL. The threshold is 12 gr/dL for girls over the age of 14 and 13 gr/dL for boys over the age of 14. Self-reported health takes the value one if the child is reported as being healthy or very healthy.

INPRES Exposure variable

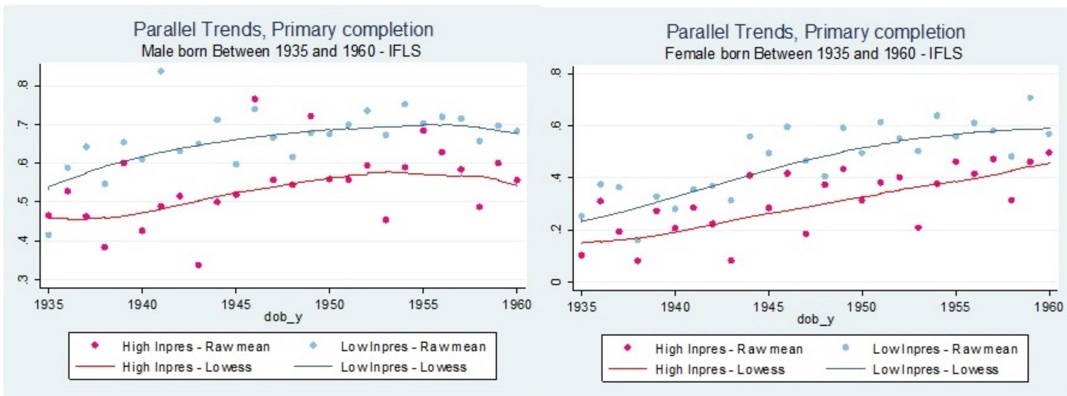
For the first generation, the IFLS asks respondents over the age of 15 their place of birth in the wave in which they first joined the survey. Additionally, in 2000, IFLS-3 asked all respondents over the age of 15 their district of birth. We combine both sources of information to obtain the respondents' district of birth.

For the second generation, we identify mother-child and father-child pairs based on the relationships within the household. We use mother-child (father-child) pairs by including respondents identified as the biological child of adult female (male) respondents who were born between 1950 and 1972. Additionally, in cases where the child's place and/or date of birth is missing from the household roster, we use women's pregnancy history to identify children born to women who were born between 1950 and 1972.

[53] We use the 2007 WHO growth chart, which is applicable to children between 0 and 19 years of age.

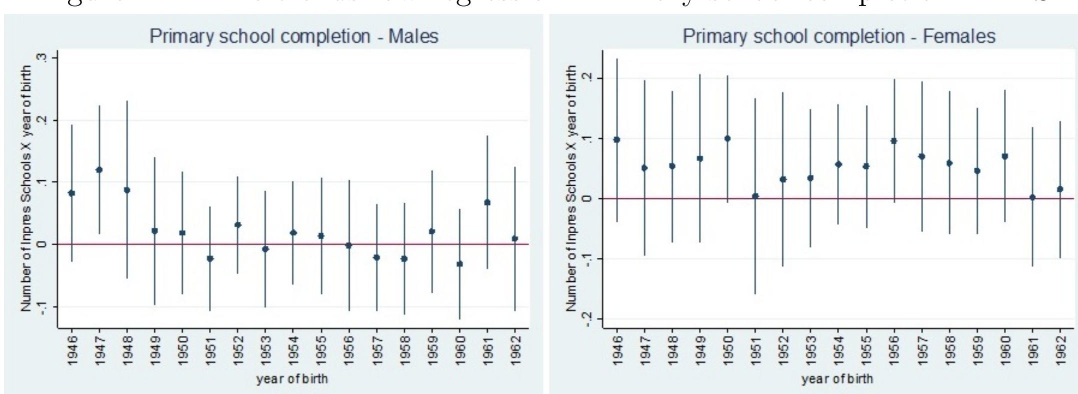
B Additional figures and tables

Figure A.3: Pre-trends raw data: Primary school completion - IFLS



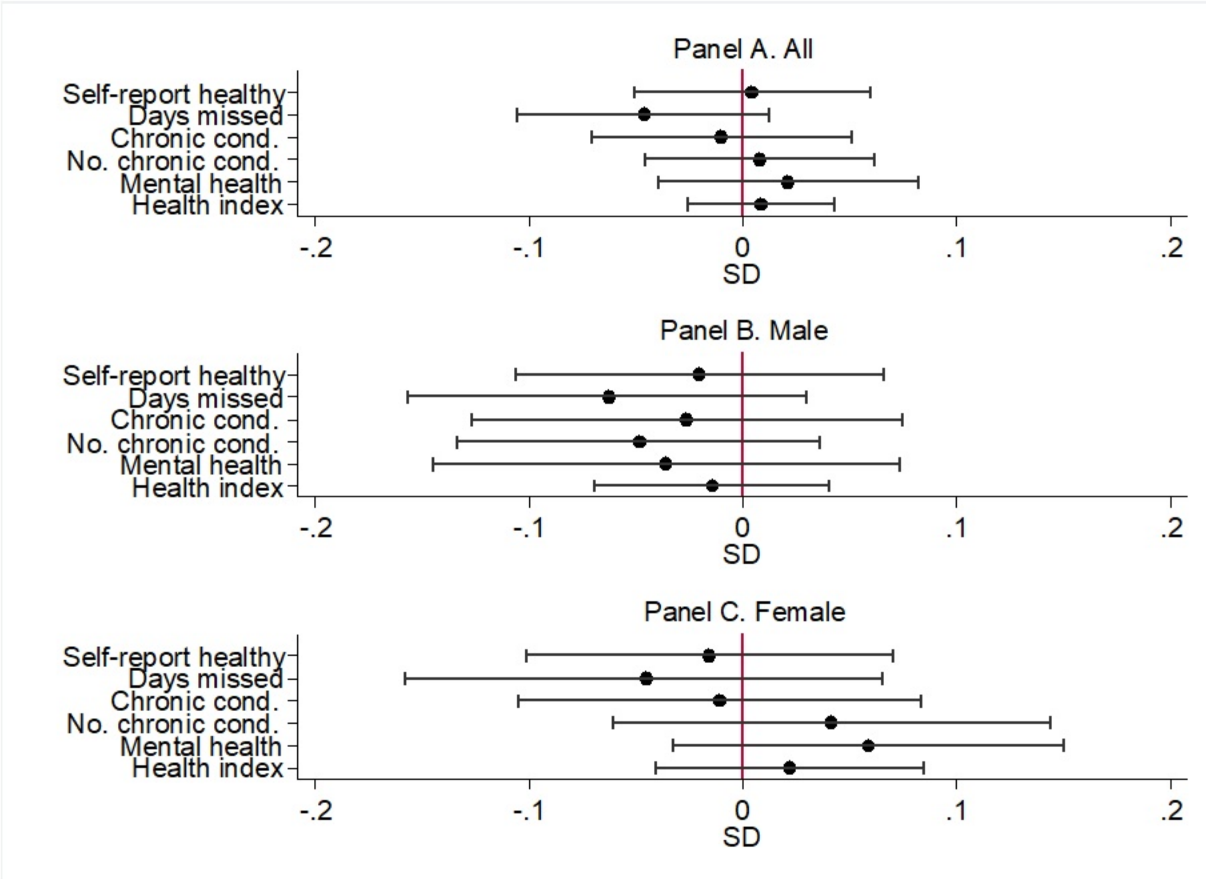
Notes:Primary completion rates for cohorts born between 1935 and 1958 from the main IFLS and IFLS-E.

Figure A.4: Pre-trends raw regression: Primary school completion - IFLS



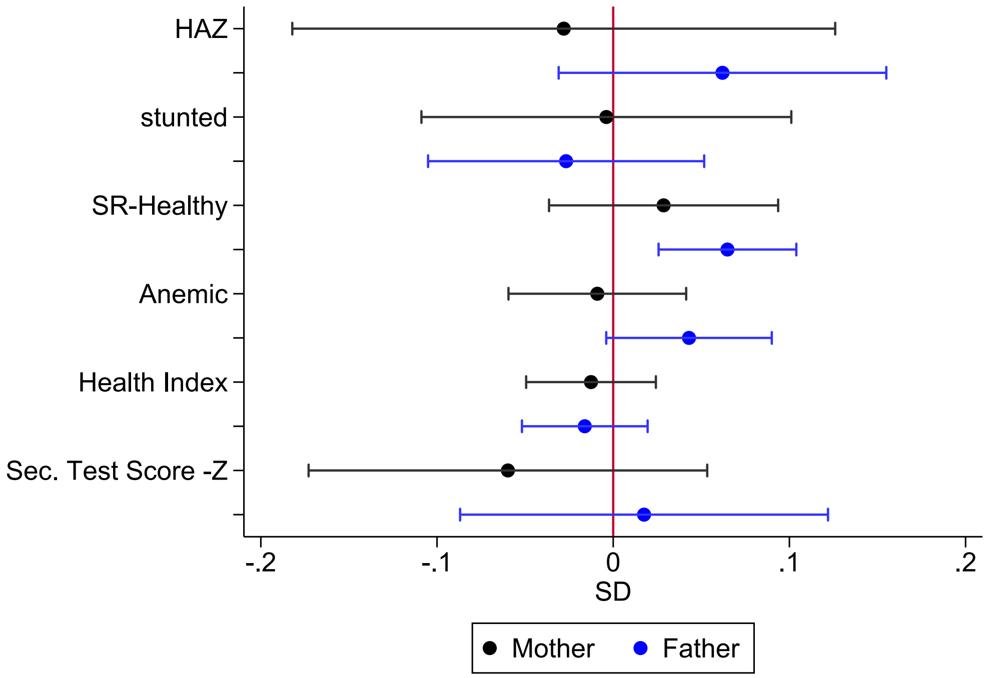
Notes: Coefficients from difference-in-differences model that interacts the number of INPRES schools and year of birth for cohorts born between 1935 and 1958. Bars indicate the 95% confidence intervals.

Figure A.5: Placebo regression: First generation



Notes: Sample includes individuals born between 1950 and 1962. "Placebo exposed group" for individuals born between 1957 and 1962. Coefficients reported in standard deviation units. Overall health index corresponds to a summary index from the multiple self-reported health measures analyzed: self-reported general health, days missed, if chronic conditions, number of conditions and mental health screening score. Health index has mean 0, SD 1 based on those born between 1950-1962 in low INPRES areas. Expanded sample includes those born between 1950-1972. Restricted sample includes those born between 1957-1962 or 1968-1972. Covariates include: district, year of birth, month of birth, ethnicity (Javanese dummy), birth-year interacted with: the number of school-aged children in the district in 1971 (before the start of the program), the enrollment rate of the district in 1971 and the exposure of the district to another INPRES program: a water and sanitation program. Robust standard errors clustered at district of birth. 95% confidence intervals are included.

Figure A.6: Placebo regression: Second generation



Notes: Sample includes the children of individuals born between 1950 and 1962. "Placebo exposed group" is defined as a dummy equal to one for children born to adults born between 1957 and 1962. Coefficients reported in standard deviation units. Overall health index corresponds to a summary index from the multiple health measures analyzed: height for age z-score, stunting, anemia, self-reported general health. Educational outcome is the z-score of the secondary school examination. The health index has mean 0, SD 1. Covariates include the following FE: parent year of birth×1971 enrollment, parent year of birth×1971 number of children, parent year of birth×water sanitation program, child's gender, birth order, year and month of birth dummies, urban, and ethnicity (Javanese dummy). Because some children may be observed multiple times, robust standard errors clustered two-way at parent's district of birth and individual level for health outcomes and clustered at parent's district of birth for test scores. 95% confidence intervals are included.

Table A.1: Summary statistics

	Mean	SD	N
Panel A: First generation			
Male	0.502	0.500	12,137
Born between 1963-1972	0.568	0.495	12,158
INPRES schools per 1,000	2.138	1.251	12,158
Javanese	0.455	0.498	12,158
Year of birth	1,963	6.392	12,158
Primary completion	0.674	0.469	12,158
Self-report: healthy	0.719	0.450	10,801
Days missed activities	1.841	3.267	10,429
Any chronic condition	0.422	0.494	10,738
No. of chronic conditions	0.683	1.007	10,738
Mental health screening score	5.513	4.694	10,254
Panel B. Second generation			
First child	0.406	0.491	10,396
Male child	0.495	0.500	10,337
Child's year of birth	1,988	6.554	10,396
Javanese	0.445	0.497	10,402
Mother born 1963-1972	0.445	0.497	10,396
Father born 1963-1972	0.484	0.500	10,396
Height for age z-score	-1.649	1.096	25,482
Stunted	0.366	0.482	25,482
Anemia	0.249	0.433	21,952
Self-reported health	0.879	0.326	22,748

Notes: Summary statistics for the expanded sample, which includes first generation individuals born between 1950-1972 and their children. The summary statistics for the health outcomes correspond to individuals observed in the Wave 5 of the IFLS and IFLS-E. Second generation height captures multiple observations per child as the IFLS measures height in all waves.

Table A.2: Replication: Primary completion

	(1)	(2)	(3)
	IFLS and IFLS-E		
	All	Male	Female
Panel A. Expanded sample (1950-1972)			
Born between 1963-72	0.028**	0.025*	0.030*
× INPRES	(0.014)	(0.014)	(0.017)
No. of obs.	13,856	6,991	6,865
Dep. var. mean	0.68	0.74	0.62
R-squared	0.252	0.232	0.290
Panel B. Restricted sample			
Born between 1968-72	0.044***	0.032*	0.052***
× INPRES	(0.014)	(0.017)	(0.018)
No. of obs.	7,650	3,869	3,781
Dep. var. mean	0.73	0.78	0.68
R-squared	0.256	0.271	0.290

Notes: Expanded sample includes those born between 1950-1972. Restricted sample includes those born between 1957-1962 or 1968-1972. Covariates include: district, year of birth, month of birth, ethnicity (Javanese dummy), birth-year interacted with: the number of school-aged children in the district in 1971 (before the start of the program), the enrollment rate of the district in 1971 and the exposure of the district to another INPRES program: a water and sanitation program. Robust standard errors in parentheses clustered at the district of birth. *** p<0.01, ** p<0.05, * p<0.1

Table A.3: Intergenerational outcomes: average poor health index

	(1) All	(2) Sons	(3) Daughters	(4) All	(5) Sons	(6) Daughters
			Panel A: Expanded sample			
Mother bet. 1963-72 × INPRES	-0.036***	-0.022	-0.055***			
	(0.013)	(0.016)	(0.017)			
Father bet. 1963-72 × INPRES				-0.039**	-0.025	-0.055***
				(0.017)	(0.021)	(0.021)
No. of obs.	9679	4943	4736	9504	4877	4627
R-squared	0.22	0.26	0.26	0.20	0.25	0.23

	(1) All	(2) Sons	(3) Daughters	(4) All	(5) Sons	(6) Daughters
			Panel B: Restricted sample			
Mother bet. 1968-72 × INPRES	-0.043**	-0.038	-0.053**			
	(0.018)	(0.030)	(0.021)			
Father bet. 1968-72 × INPRES				-0.032	-0.035	-0.048
				(0.021)	(0.029)	(0.035)
No. of obs.	5397	2751	2646	4927	2569	2358
R-squared	0.24	0.29	0.32	0.20	0.26	0.26

Notes: Sample includes the first generation's children who are between ages 8 and 18. Sample corresponds to children born to first generation INPRES individuals. Expanded sample includes children born to adults born between 1950-1972. Restricted sample includes children born to adults born between 1957-1962 or 1968-1972. Covariates include the following FE: parent year of birth and district of birth fixed effects, parent year of birth×1971 enrollment, parent year of birth×1971 number of children, parent year of birth×water sanitation program, child's gender, birth order, year and month of birth dummies, ethnicity (Javanese dummy), examination year dummies. Average regressions weighted by the number of observations per child. Robust standard errors in parentheses clustered at the parent's district of birth. *** p<0.01, ** p<0.05, * p<0.1

Table A.4: Intergenerational outcomes: Complete secondary school

	(1) All	(2) Sons	(3) Daughters	(4) All	(5) Sons	(6) Daughters
Mother Born between 1963-72	-0.020	-0.002	-0.025			
× INPRES	(0.014)	(0.026)	(0.019)			
Father Born between 1963-72				0.004	-0.014	0.013
× INPRES				(0.018)	(0.031)	(0.031)
No. of obs.	11465	3881	3918	8896	3079	3130
Dep. var. mean	0.348	0.478	0.448	0.354	0.479	0.449
R-squared	0.10	0.19	0.19	0.11	0.22	0.22

	(1) All	(2) Sons	(3) Daughters	(4) All	(5) Sons	(6) Daughters
			Panel B: Restricted sample			
Mother Born between 1968-72	-0.029	-0.041	-0.018			
× INPRES	(0.024)	(0.042)	(0.029)			
Father Born between 1968-72				0.009	-0.081	0.094
× INPRES				(0.035)	(0.065)	(0.060)
No. of obs.	5645	1888	1952	3796	1327	1356
Dep. var. mean	0.357	0.480	0.475	0.367	0.491	0.477
R-squared	0.14	0.27	0.25	0.16	0.32	0.34

Notes: Sample corresponds to children born to first generation INPRES individuals. Expanded sample includes children born to adults born between 1950-1972. Restricted sample includes children born to adults born between 1957-1962 or 1968-1972. Covariates include the following FE: parent year of birth and district of birth fixed effects, parent year of birth×1971 enrollment, parent year of birth×1971 number of children, parent year of birth×water sanitation program, child's gender, birth order, year and month of birth dummies, ethnicity (Javanese dummy). Robust standard errors in parentheses clustered at the parent's district of birth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.5: Robustness: Alternative exposure based on schools built per year

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	First generation			Second generation			
	Health index			Test score		Health index	
	All	Male	Female	Maternal	Paternal	Maternal	Paternal
Born bet. 1963-1972 × INPRES	-0.032**	-0.027	-0.035*	0.115***	0.097**	-0.012	-0.014
	(0.013)	(0.019)	(0.021)	(0.039)	(0.046)	(0.013)	(0.014)
No. of obs.	9836	4785	5051	6484	5475	17851	17306
R-squared	0.10	0.12	0.11	0.13	0.12	0.17	0.15

Notes: The alternative exposure variable is based on the number of schools built per year for each cohort. Expanded sample includes children born to adults born between 1950-1972. Restricted sample includes children born to adults born between 1957-1962 or 1968-1972. See Table 1, Table 4, and Table 5 for covariates. Robust standard errors clustered at district of birth for cols. 1-3. Robust standard errors in parentheses clustered at the parent's district of birth for cols. 4-5. Robust standard errors in parentheses two-way clustered at the parent's district of birth and individual level for cols. 6-7. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.6: Robustness: Alternative cohorts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	First generation			Second generation			
	Health index			Test score		Health index	
	All	Male	Female	Maternal	Paternal	Maternal	Paternal
Panel A. Expanded sample							
Born bet. 1963-1975	-0.041***	-0.021	-0.058***	0.081***	0.058	-0.024**	-0.013
× INPRES	(0.013)	(0.021)	(0.019)	(0.031)	(0.041)	(0.012)	(0.011)
No. of obs.	9891	4817	5074	6996	5709	20341	19145
R-squared	0.10	0.12	0.11	0.12	0.11	0.16	0.14
Panel B. Restricted sample							
Born bet. 1968-1975	-0.038**	-0.010	-0.061**	0.160***	0.074	-0.014	-0.012
× INPRES	(0.016)	(0.032)	(0.026)	(0.046)	(0.070)	(0.015)	(0.014)
No. of obs.	5537	2668	2869	3855	2735	12334	10766
R-squared	0.12	0.15	0.14	0.13	0.16	0.16	0.13

Notes: Expanded sample includes those born between 1950-1975. Restricted sample includes those born between 1957-1962 or 1968-1975. See Table 1, Table 4, and Table 5 for covariates. Robust standard errors clustered at district of birth for cols. 1-3. Robust standard errors in parentheses clustered at the parent's district of birth for cols. 4-5. Robust standard errors in parentheses two-way clustered at the parent's district of birth and individual level for cols. 6-7. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.7: Potential mechanisms: Fertility

	(1) Age 1st preg.	(2) No. of births	(3) Spacing 1st and 2nd child
	Panel A: Expanded sample		
Female born between 1963-72	-0.179	-0.024	0.364
× INPRES	(0.192)	(0.077)	(1.513)
No. of obs.	5677	5661	4695
Dep. var. mean	23.07	3.20	52.98
R-squared	0.17	0.36	0.15

	(1) Age 1st preg.	(2) No. of births	(3) Spacing 1st and 2nd child
	Panel B: Restricted sample		
Female born between 1968-72	-0.125	-0.054	0.303
× INPRES	(0.270)	(0.077)	(1.963)
No. of obs.	3112	3095	2554
Dep. var. mean	23.05	2.88	53.29
R-squared	0.21	0.36	0.18

Notes: Expanded sample includes those born between 1950-1975. Restricted sample includes those born between 1957-1962 or 1968-1975. See Table 1 for covariates. Robust standard errors clustered at district of birth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.8: Potential mechanisms: Migration

	(1)	(2)	(3)	(4)
	Expanded sample		Restricted Sample	
	Ever moved	Moved by 2012-14	Ever moved	Moved by 2012-14
Panel A: First generation				
Born bet. 1963-72	-0.002	0.005	-0.000	0.001
× INPRES	(0.010)	(0.010)	(0.012)	(0.012)
No. of obs.	14049	13199	7818	7356
Dep. var. mean	0.342	0.296	0.345	0.297
R-squared	0.25	0.25	0.25	0.25
	(1)	(2)	(3)	(4)
	Expanded sample		Restricted Sample	
	Maternal mig. pre	Maternal mig. post	Mat. mig. pre	Mat. mig. post
Panel B: Second generation				
Mother Born bet. 1963-72	-0.006	0.014	0.022	0.033
× INPRES	(0.016)	(0.016)	(0.022)	(0.027)
No. of obs.	15464	15397	8117	8083
Dep. var. mean	0.285	0.285	0.280	0.271
R-squared	0.28	0.11	0.30	0.14

Notes: Ever moved is an indicator that takes the value one if the adult respondent's district of birth is different from the respondent's current district of residence. Moved by 2012-14 is indicator that compares the respondent's district of birth and his or her district of residence in 2012 (IFLS-E) or 2014 (IFLS). Maternal mig. pre is indicator that takes the value one if the mother's district of birth is different from the child's district of birth. Maternal mig. post is an indicator that takes the value one if the child's district of birth is different from the child's current district of birth. Expanded sample includes those born between 1950-1975. Restricted sample includes those born between 1957-1962 or 1968-1975. See Table 1 for covariates. Robust standard errors clustered at district of birth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.9: Long-term outcomes: Asset index

	(1) All	(2) Male	(3) Female
	Panel A. Expanded sample		
Born bet. 1963-1972	0.024	0.024	0.028
× INPRES	(0.028)	(0.034)	(0.035)
No. of obs.	11951	6007	5944
R-squared	0.20	0.22	0.22
	(1) All	(2) Male	(3) Female
	Panel B. Restricted sample		
Born bet. 1968-1972	0.037	0.024	0.074**
× INPRES	(0.029)	(0.043)	(0.035)
No. of obs.	6756	3407	3349
R-squared	0.19	0.21	0.24

Notes: Asset index includes the following asset ownership: savings, vehicle, land, TV, appliances, refrigerator, and house. Expanded sample includes those born between 1950-1972. Restricted sample includes those born between 1957-1962 or 1968-1972. See Table 1 for covariates. Robust standard errors clustered at district of birth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.10: Potential mechanism: Household resources

	(1) All	(2) Male	(3) Female	(4) All	(5) Male	(6) Female
		Food			Non-food	
	Panel A: Expanded sample					
Born bet. 1963-1972	0.040***	0.038*	0.041	0.053**	0.050	0.064
× INPRES	(0.015)	(0.022)	(0.027)	(0.026)	(0.034)	(0.043)
No. of obs.	11941	6007	5934	11925	5998	5927
Dep. var. mean	12.430	12.481	12.378	11.522	11.577	11.467
R-squared	0.14	0.15	0.17	0.16	0.16	0.20
	(1) All	(2) Male	(3) Female	(4) All	(5) Male	(6) Female
	Panel B: Restricted sample					
Born bet. 1968-1972	0.040*	0.052	0.031	0.069**	0.062	0.105**
× INPRES	(0.023)	(0.032)	(0.034)	(0.034)	(0.048)	(0.053)
No. of obs.	6752	3408	3344	6742	3403	3339
Dep. var. mean	12.458	12.499	12.417	11.584	11.622	11.545
R-squared	0.14	0.15	0.19	0.16	0.17	0.22

Notes: Log per capita expenditure in 2012-14 based on weekly or monthly per capita food and non-food expenditure in 2012 *Rupiah*). Expanded sample includes those born between 1950-1972. Restricted sample includes those born between 1957-1962 or 1968-1972. See Table 1 for covariates. Robust standard errors clustered at district of birth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.11: Potential mechanism: Housing quality (Expanded sample)

	(1) Drinking water	(2) Bad Toilet	(3) Bad Occupancy	(4) Bad Floor	(5) Bad Roof	(6) Bad Wall
Panel A. All						
Born between 1963-1972	0.010	-0.006	-0.015*	-0.006	0.003	-0.012
× INPRES	(0.009)	(0.007)	(0.008)	(0.006)	(0.003)	(0.008)
N	11835	11849	11934	12132	12121	12022
Dep. var. mean	0.437	0.086	0.092	0.163	0.033	0.234
R-squared	0.21	0.13	0.11	0.39	0.27	0.33
Panel B. Men						
Born between 1963-1972	0.010	0.001	-0.026**	-0.010	0.000	-0.009
× INPRES	(0.013)	(0.009)	(0.012)	(0.008)	(0.003)	(0.010)
N	5887	5891	5944	6042	6036	5986
Dep. var. mean	0.435	0.087	0.094	0.164	0.031	0.235
R-squared	0.24	0.15	0.14	0.43	0.27	0.36
Panel C. Women						
Born between 1963-1972	0.013	-0.011	-0.002	-0.002	0.004	-0.019*
× INPRES	(0.012)	(0.009)	(0.008)	(0.009)	(0.005)	(0.011)
N	5948	5958	5990	6090	6085	6036
Dep. var. mean	0.439	0.086	0.089	0.162	0.035	0.233
R-squared	0.22	0.15	0.12	0.38	0.31	0.34

Notes: Poor toilet is captured by not having access to a toilet (including shared or public toilet). Poor floor includes board or lumber, bamboo, or dirt floor. Poor roof includes leaves or wood. Poor wall includes lumber or board and bamboo or mat. High occupancy per room is defined as more than two persons per room in the house (based on household size). Expanded sample includes those born between 1950-1972. See Table 1 for covariates. Robust standard errors clustered at district of birth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.12: Potential mechanism: Housing quality (Restricted sample)

	(1) Drinking water	(2) Bad Toilet	(3) Bad Occupancy	(4) Bad Floor	(5) Bad Roof	(6) Bad Wall
Panel A. All						
Born between 1968-1972	0.016	-0.003	-0.012	-0.006	0.002	-0.020**
× INPRES	(0.013)	(0.008)	(0.010)	(0.008)	(0.004)	(0.009)
N	6696	6698	6760	6867	6861	6795
Dep. var. mean	0.449	0.085	0.092	0.161	0.035	0.232
R-squared	0.22	0.14	0.11	0.39	0.27	0.34
Panel B. Men						
Born between 1968-1972	0.006	-0.002	-0.037***	-0.015	0.005	-0.026**
× INPRES	(0.016)	(0.010)	(0.013)	(0.010)	(0.005)	(0.011)
N	3319	3319	3363	3408	3405	3371
Dep. var. mean	0.448	0.087	0.096	0.160	0.032	0.231
R-squared	0.25	0.17	0.15	0.43	0.27	0.37
Panel C. Women						
Born between 1968-1972	0.043**	-0.006	0.013	0.000	-0.001	-0.020
× INPRES	(0.020)	(0.012)	(0.014)	(0.012)	(0.007)	(0.014)
N	3377	3379	3397	3459	3456	3424
Dep. var. mean	0.450	0.082	0.088	0.162	0.037	0.234
R-squared	0.24	0.17	0.14	0.40	0.33	0.35

Notes: See Table A.11 for notes. Restricted sample includes those born between 1957-1962 or 1968-1972. Robust standard errors clustered at district of birth. Significance: *** p < 0.01, ** p < 0.05, * p < 0.10.

Table A.13: Robustness: Non-movers only

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	First generation			Second generation			
	Health index			Test score		Health index	
	All	Male	Female	Maternal	Paternal	Maternal	Paternal
Panel A. Expanded sample							
Born bet. 1963-1972	-0.039*	0.008	-0.074***	0.087**	0.048	-0.037**	-0.030**
$\times$ INPRES	(0.023)	(0.033)	(0.028)	(0.042)	(0.043)	(0.014)	(0.015)
No. of obs.	4779	2271	2508	4496	3613	12738	11568
R-squared	0.12	0.15	0.14	0.15	0.16	0.18	0.16
Panel B. Restricted sample							
Born bet. 1968-1972	-0.026	0.019	-0.052	0.165**	0.117	-0.044**	-0.032*
$\times$ INPRES	(0.025)	(0.047)	(0.032)	(0.067)	(0.080)	(0.017)	(0.016)
No. of obs.	2662	1275	1387	2357	1690	6987	5941
R-squared	0.13	0.16	0.18	0.17	0.20	0.18	0.16

Notes: Expanded sample includes those born between 1950-1975. Restricted sample includes those born between 1957-1962 or 1968-1975. See Table 1, Table 4, and Table 5 for covariates. Robust standard errors clustered at district of birth for cols. 1-3. Robust standard errors in parentheses clustered at the parent's district of birth for cols. 4-5. Robust standard errors in parentheses two-way clustered at the parent's district of birth and individual level for cols. 6-7. Significance: *** p < 0.01, ** p < 0.05, * p < 0.10.

Table A.14: Potential mechanism: Neighborhood quality

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All-Edu	Male	Female	All-Health	Male	Female	All-Poor	Male	Female
Born bet. 1963-1972	0.014	0.059	-0.003	0.024	0.036	0.014	-0.003	0.015	-0.022
× INPRES	(0.021)	(0.038)	(0.029)	(0.022)	(0.030)	(0.032)	(0.022)	(0.035)	(0.033)
No. of obs.	9329	4526	4776	9078	4399	4651	8015	3920	4067
R-squared	0.56	0.57	0.58	0.48	0.50	0.49	0.50	0.51	0.53
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All-Edu	Male	Female	All-Health	Male	Female	All-Poor	Male	Female
Born bet. 1968-1972	-0.001	0.022	0.008	0.055**	0.036	0.079*	-0.012	0.017	-0.069*
× INPRES	(0.031)	(0.042)	(0.046)	(0.027)	(0.035)	(0.044)	(0.027)	(0.036)	(0.040)
No. of obs.	5118	2464	2617	4974	2389	2549	4377	2122	2218
R-squared	0.58	0.59	0.60	0.48	0.51	0.50	0.51	0.53	0.53

Notes: Education index includes the number of primary, junior high, and high schools used by the community. Health index includes the following: an indicator for having a majority of the residents using piped water, an indicator for having a majority of the residents using private toilet, the number of community health centers, and the number of midwives available to the community. Poverty index includes the fraction of households in the community in the subsidized rice program (*Raskin*), subsidized national health insurance (*Jamkesmas*), subsidized regional (province or district) health insurance (*Jamkesda*). Expanded sample includes those born between 1950-1972. Restricted sample includes those born between 1957-1962 or 1968-1972. See Table 1 for covariates. Robust standard errors clustered at district of birth and community. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

C Cost-Benefit Analysis

Cost

We follow Duflo (2001) in our cost estimation. The cost of the school construction and the number of schools built each year came from Duflo (2001). We assume the schools were built between 1973 and 1977 and were operational for 20 years, so the school's last year of operation is 1997. Each school was designed with 3 classrooms and 3 teachers. Teacher's salary was USD 360 (in 1990 USD) in 1974 and USD 2467 (in 1990 USD) in 1995, and we assume linear growth between 1974 and 1995. We assume each teacher would require training, and that would cost a third of the salary. The maintenance cost is assumed to be 25% of the wage bill.

Cohort size

We estimate the number of first generation individuals exposed to the program, starting from those born in 1963. Assuming children start school at 6, the last cohort to benefit is born in 1989. We use the 1971, 1980, and 1990 Census to obtain the population of the cohort born between 1963 and 1989. We then estimate the number of students enrolled based on the enrollment rates between 1970 and 1995 from the World Development Indicators.

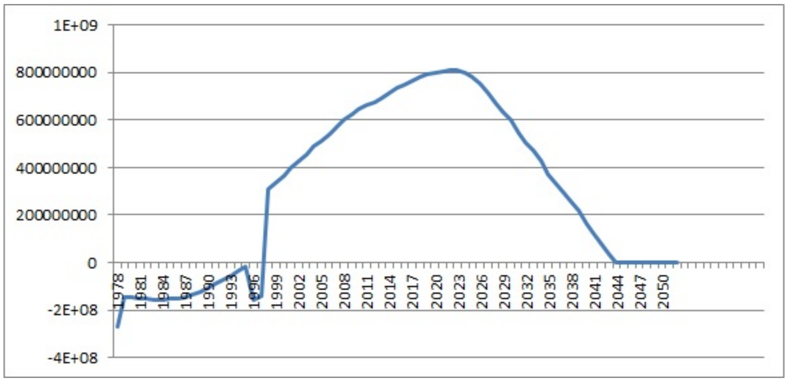
The program sought to attain a teacher student ratio of 40 students per teacher. Each school would typically hold a morning and afternoon session, with 3 classes in each session, so each school served 240 students.[54] To obtain the INPRES coverage for each year, we use the number of INPRES schools in each year divided by the number of primary school-aged children (6-12 year olds) enrolled in primary school. The INPRES exposure for each cohort is given by the average INPRES coverage for the cohort's primary schooling (6 years).

First generation benefits

We follow the literature and assume that returns to primary education is 20% (Psacharopoulos and Patrinos*, 2004). To calculate the base earnings on which to apply the return to primary schooling, we assume the Indonesian population will be in the labor force between the ages of 16 and 55, which is the official age of retirement up to the early 2000s. We calculate the mean earnings of individuals aged 16 to 55 in IFLS-1. We then use the CPI to deflate earnings to 1990 USD. The estimated lifetime earnings is about USD 57000 (in 1990 USD).

[54] Conversations with Bappenas and former Bappenas officials.

Figure A.7: Cost Benefit Analysis



Notes: We assume the benefits are derived solely from the earnings gain of the first generation individuals. Cohorts born in 1963 to 1989 benefit from the program. Benefits accrue from 1979, the first year that the first cohort entered the labor market, and end in 2052, when the last cohort served by the program would retire.

We include the gains from health based on the relationship between poor health and mortality at older ages. We follow the literature and assume that self-reported poor health is associated with a 2.73 odds ratio among those 50 and above in Indonesia (Frankenberg and Jones, 2004). We then combine this with mean earnings between the ages of 50 and 55 (in 1990 USD) and estimated survival probability for those ages from Statistics Indonesia.[55]

Second generation benefits

To obtain the cohort size in the second generation, we assume each first generation individual has 1.2 children at age 22.[56] We assume second generation individuals have 20% higher lifetime earnings compared to the first generation individuals and the second generation would be in the labor market between the ages of 16 and 55. The effect of INPRES on the second generation's height is 0.056 standard deviations. With about 6 centimeters standard deviation in height, this would correspond to about 0.366 centimeters height increase. With an 8% gain in earnings resulting from the height premium (Sohn, 2015), the program effect would then translate to a 0.26% gain in lifetime earnings for the second generation. For the second generation gain in education, we use literacy as a proxy

[55] Pengembangan Model Life Table Indonesia (2011). Last accessed July 15, 2019.

[56] Indonesia's total fertility rate in 2000 is 2.4 per woman, so we assume a fertility rate of 1.2 for the first generation individuals.

Table A.15: Internal Rate of Return Estimates

		Internal Rate of Return
First generation	Earnings returns to Primary Completion	7.91%
	+ earnings gains from better health and lower mortality	8.75%
Second generation	1st gen. gains + returns to height	14.53%
	1st gen. gains + returns to test scores	21.48%
	1st gen. gains + returns to height and test scores if independent	24.76%

Notes: First generation earnings based on returns to primary completion. First generation health gains based on earnings gains between 50 and 55 from program reduction in poor health that is associated with mortality improvement. Test score gains based on earnings gain from improved secondary test score. Health gains based on the height premium. Test score and health are assumed to be independent.

for gains in test score. Following the literature, we assume a one standard deviation increase in literacy would increase earnings by 8.5% (Perez-Alvarez, 2017). The program effect would then translate to a 0.68% gain in lifetime earnings for the second generation.

Scenarios

We present calculations based on several scenarios (Table A.15). First, we assume the gains for the first generation came from earnings only. Next, we assume the gains for the first generation came from earnings and mortality gains. We then add the gains from the second generation's test score alone, the second generation's health alone, and finally, we assume the gains from health and test scores are independent and combine the gains.