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

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July 2, 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 were generally stronger for maternal exposure than paternal exposure. We present some evidence of household resources, neighborhood quality, and assortative matching as potential mechanisms. 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 are grateful to Taryn Dinkleman, Joe Kaboski, Marco Gonzalez-Navarro, Jean Lee 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), and LACEA-RIDGE conference. We thank Leah Plachinski for excellent research assistance.

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1 Introduction

One of the fundamental ways that nations have tried to advance economic development has been through investments in human capital. Many developing countries have embarked on educational reforms such as school construction projects in an effort to improve living standards. However, inadequate access to schools and poor school quality remains a pervasive problem in much of the developing world (UNESCO, 2015; UNICEF, 2018) and the building of schools remains a priority in many countries. There is now increasing recognition that in addition to improving economic outcomes, educational policies have the potential for producing important spillovers on other aspects of well-being such as health and may improve the socio-economic outcomes of future generations. However, few studies have been able to causally identify these spillovers, particularly in developing countries. 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 spillovers by studying a massive school construction program in Indonesia known as the INPRES program. Between 1974 and 1979, 61,000 elementary schools were constructed, 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 children exposed to these schools, while Martinez-Bravo (2017) documented the impacts on local governance and public good provision in Indonesia's main island (Java). Of course 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.[1]

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

[1] In contrast, Duflo (2001) studied the labor market effects of INPRES and therefore emphasized the traditional metric of the labor market return to an additional year of schooling. We find similar point estimates to Duflo (2001) (See Appendix Table A.1 in Mazumder et al. (2019)). However, our estimates are less precisely estimated owing to the smaller samples contained in our data, the IFLS and IFLS-E.

next generation.[2] 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 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*, instead of relying on children living in the household at only one point in time (cross-section). Furthermore, the rich set of questions in IFLS allows us to track several very useful markers of health for both exposed cohorts ("first generation") and their children ("second generation"), as well as 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 outcomes of first generation treated individuals. 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 produce 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 intergenerational impacts on the children of cohorts exposed to the rollout of INPRES, i.e. the second generation. We find that children born to women exposed to the INPRES program score 0.06 standard deviations higher in the national 9th grade examination, and the estimated effect is far larger (0.14 SD) if we only consider mothers who were younger than primary school age when the INPRES program began. We also find notable effects on the health of the second generation children. Specifically, maternal access to INPRES elementary schools increases height-for-age by 0.05 standard

[2] A contemporaneous paper to ours by Akresh et al. (2018) also examines the long-run health 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. One important difference is that their intergenerational analysis is limited to adult children who are co-resident with their parents. In contrast, we exploit longitudinal data which allows us to include *all children including those who no longer reside with their parents as adults*. This difference in sample coverage is important. We find that roughly 55 percent of 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 examine the impacts on participation in agriculture. However, none of these papers examine second generation outcomes.

deviations 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. Overall, we observe a 0.03 SD reduction in poor health as measured by a health index of these outcomes among the second generation. 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.

There are several pathways through which improved parental human capital (due to INPRES) could lead to better second-generation outcomes. One important potential channel is through greater parental investments in human capital. Unfortunately we do not have good measures to proxy for such investments. We consider several other pathways for which we can construct measures with available data. These include household resources, assortative mating, migration, fertility and neighborhood quality.[3] 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 better household resources as measured by per capita consumption and housing quality. Also, we observe that individuals exposed to INPRES live in communities with better access to health services. We find no significant evidence that fertility and migration responses drive the main results.

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 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 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 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

[3] General equilibrium effects may also influence both the first and second generations effects (Duflo, 2004).

of social interventions. Relatively fewer 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 ability to measure outcomes in two distinct generations and the ability to 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-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 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 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, but progress in expanding primary school access was particularly fast after 1973.

During the 1970's, 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 pro-

[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)

gram 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, reforestation, market, and road construction. 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 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 0.19 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, She shows that the increase in education among exposed individuals had a negative effect on the wages of older cohorts and increased their participation in the formal labor market.

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 mechanisms 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 School construction interventions in developing countries

Improving access to education through school building is a popular supply-side intervention in developing countries, where students may need to travel long distances to reach the closest school (Kazianga et al., 2013). Therefore, a number of studies have causally examined whether increasing school infrastructure improve school enrollment, and learning outcomes (Glewwe and Muralidharan, 2016), with few studies going beyond to analyze the effects on other human capital outcomes.[7]

Most of the evidence document contemporaneous impacts on school enrollment. Burde and Linden (2013) use a Randomized Control Trial (RCT) examine the impacts of village-based primary school openings on primary school-aged children in Afghanistan. While traditional schools are designed to serve numerous children from multiple villages and thus may be far away, village-based schools are smaller and designed to serve only the children living close by. After one year, they find significant effects on enrollment that are larger for girls (52 percentage points for girls and by 35 pp for boys). Alderman et al. (2003) also used an RCT in Pakistan to build new private girls' primary schools or to support existing ones. They find that the program increased the enrollment rates for girls in urban and rural areas (Glewwe and Muralidharan, 2016). Similarly, Kazianga et al. (2013) evaluate the effects of a primary school construction program that included girl-friendly amenities in Burkina Faso. Using a Regression Discontinuity Design based on a village score assignment, they find that

[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 and Bharati et al. (2018) who examine whether INPRES mitigates the effects of adverse weather shocks. However, neither of these papers examine second generation outcomes.

[7] There is large correlational evidence of the associations between increasing the availability of schools and enrollment. In this section, we review the main studies that rely on causal methods (Filmer, 2007).

the program increased enrollment for boys and girls about 3 years post implementation.

Some studies also evaluate the short and medium-term effects on educational achievement. Burde and Linden (2013) find that the Afghanistan village-based program improved children's test scores in math and language skills for girls and boys. Similarly, Kazianga et al. (2013) find that new girl-friendly schools in Burkina Faso increased test scores in mathematics and French. Focusing on access to preschools in Argentina, Berlinski et al. (2009) examine the medium-term effects of a large program that constructed preschool classrooms to accommodate children after making pre-primary education compulsory. Exploiting variation across regions and cohorts, they find that access to preschool education facilities increased children's test scores in third grade. Children also experienced improvements in socio-emotional skills, such as attention and effort.

Very few studies have examined whether the contemporaneous and medium-term effects of school construction programs translate into improvements in later outcomes, including educational attainment, economic well-being, and intergenerational impacts. Such studies include some that have analyzed the Indonesian INPRES primary school construction program, which we discussed above. Another study by Wantchekon et al. (2014) examines a historical shock that involved the construction of colonial schools in central and northern Benin in the early 20th century. Using information from schools and church archives and a natural experiment design, they find that individuals who attended missionary colonial schools had better living standards (i.e. having running water, assets, etc.), better social networks, and are less likely to be farmers. Also, they analyze the spillover from the first generation to their descendants and find positive effects on the descendants' education and living standards. An interesting finding from Wantchekon et al. (2014) is that the second generation effects are accrued not only from the individual/direct effects from parent's access to colonial schools but also from human capital externalities as children of non-treated individuals in villages that received schools also benefited from this intervention.

A set of studies have analyzed the effects of the Rosenwald Rural Schools Initiative in the US, which occurred between 1913 and 1932 in a similar context as those of developing countries and is the largest school construction program in the US in the 20thcentury. Using information of the location and the timing of the entry of the Rosenwald schools, Aaronson and Mazumder (2011) show that rural black students exposed to the Rosenwald schools experienced an increase in years of schooling by one year compared to rural black students with no access to those schools. In the aggregate, this magnitude explains around 40% of the decline in the racial gap in the South for cohorts born between 1910 and 1925. Also, they find that exposure to the program increased cognitive scores by 0.2 to 0.45 SDs. Aaronson et al. (2014) show that women exposed to the Rosenwald schools experienced a decline in early fertility, a delay in marriage, and an improvement in the quality of their chosen occupations, all of which is consistent with the idea that education raised the opportunity costs of fertility.

To the best of our knowledge, only one study has examined the effects of an education intervention that included a school building component on the second generation's health. Chou et al. (2010) examine the effects of the 1968 Taiwanese school reform that extended compulsory education from 6 to 9 years and increased the number of junior high schools by half. Exploiting geographic variation in the school openings and cohort variation in exposure, they find that the reform increased years of schooling by around 0.2 years for both mothers and fathers. Next, they examine the intergenerational effects on birth outcomes and infant health and find that the mother's schooling reduces the percentage of low birth weight by 5% of the mean, while it reduces neonatal and infant mortality between 8% and 19%.

We build on this small number of studies to provide the first evidence on the impacts of a massive primary school construction program on the long-term health status of the first generation. Given the documented effects of school construction programs on access to education, learning, and educational attainment and the complementarities between education and health (which we discuss below), we can expect that these gains may translate into health improvements. Also, we add to this set of studies by exploring the impacts of such programs on the next generation.

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 strong and robust association is 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. Theoretically, education can improve productive efficiency and allocative efficiency (Grossman, 1972; Kenkel, 1991). Through productive efficiency, higher education leads to a higher marginal product of 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. Recent reviews of the literature include (Cutler et al., 2011; Galama et al., 2018; Grossman, 2015; Mazumder, 2012).

The empirical evidence on the causal effects of education on health is mixed and appears to vary depending on the setting and the type of intervention. 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). Individuals exposed to these programs were less likely to be diagnosed with mental health problems in later life, and were less likely to exhibit risky health behaviors. On the other hand, studies that exploit compulsory schooling laws across various countries, affecting different age groups in different time periods provide only mixed evidence on the effects of education on various health outcomes Mazumder (2012). For example, Clark and Royer (2013) using a very credible identification strategy with very large samples in the U.K. are able to identify very precise null effects of education on a wide range of health outcomes. Even when examining specific health outcomes, the results are not always consistent across settings. For example, for mental health, evidence from the UK shows that education is associated with higher self-reported life satisfaction (Oreopoulos, 2007). 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). One possible explanation for the confusing array of results 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.[8] 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 examine the effects of education not only one's own health but also extend the analysis to analyze the effects on offspring's health in the next generation. Parental education, especially the mother's, is a strong predictor of children's outcomes (i.e. birth weight) (Currie and Moretti, 2003). However, changes in compulsory schooling in several countries have shown mixed evidence. Evidence from the UK shows little effects on child health (Lindeboom et al., 2009), while in other settings, parental education has been shown to improve children's health outcomes. In Taiwan, compulsory schooling and school

[8] 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)

construction led to improvements in low birth weight (birth weight under 2,500 grams), infant mortality (mortality in the first 12 months), and child mortality (mortality in the first 60 months)(Chou et al., 2010). Evidence from expanding access to secondary school in Zimbabwe also shows that higher maternal education is associated with lower child mortality (Grépin and Bharadwaj, 2015). Similarly, evidence from Turkey shows that increasing compulsory schooling from 5 to 8 years increases women's educational attainment, and this leads to better children's outcomes: lower incidence of low birth weight and child mortality (Dursun et al., 2017). In the context of the primary school building project in Indonesia, we extend earlier work by Breierova and Duflo (2004) that shows lower child mortality. Potential channels behind these effects may be 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.

3 Data

This paper uses data from the Indonesian Family Life Survey, 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. 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 regardless of whether they remained co-resident, instead of relying on children living in the household at only one point in time (cross-section). 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).

Indonesia is administratively divided into provinces, districts (regencies or cities), subdistricts, 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 519 districts.[9]

The IFLS contains rich information on individual, household, and community characteristics. Household characteristics include household size, household expenditure, and asset ownership. Individual characteristics include age, education, marital status, employment, as well as complete pregnancy history for women between the ages of 15 and 49.

Sample of interest We explore the long-term and intergenerational outcomes of first generation individuals who are 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 (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 in this sample comprises individuals partially exposed to the INPRES schools (those older than age 7 but younger than age 12 since only a part of their primary school ages occurred after the program onset) and those who are fully exposed (younger that age 7 at the time of the program). Following Duflo (2001), we also present estimates for a sample that comprises as treated those individuals fully-exposed (born between 1968 and 1972) and a non-exposed group that is closer in age (born between 1957 and 1962). We called this sample "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 completion, which is constructed using each individual's education history that included the highest level of education completed. Adult respondents were also asked to report their physical health through a series of questions on self-reported health status and chronic con-

[9]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).

ditions.[10] In order to observe the first generation when they are at least 40 years old, we use information from the IFLS-E in 2012 and IFLS-5 in 2014. Good self-reported health takes the value one if a respondent reported his or her self status as 'very healthy' or 'healthy'. 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.[11]

To assess mental health, respondents were administered a series of 10 questions on how frequently they experienced symptoms of depression using the Center for Epidemiologic Studies Depression Scale (CES-D), which has been clinically validated. 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).[12] We use the sum of the scores based on reported symptoms, where higher scores indicate a higher likelihood of having depression.

To summarize the multiple health outcomes, we construct at 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.

In addition, we separately explore objectively measured health outcomes based on anthropometric measures. The following measures were taken for adults: blood pressure, height,

[10]The IFLS also includes health care utilization, which could explain the health behavior associated with health outcomes. However, we exclude this outcome 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.

[11]These conditions include: hypertension, diabetes, tuberculosis, asthma, other respiratory conditions, stroke, heart disease, liver condition, cancer, arthritis, high cholesterol, depression/psychiatric condition.

[12]We recode feeling hopeful about the future and feeling happy to reflect the negative symptoms.

and weight. We calculate BMI (Body Mass index) using the person's weight in kilograms divided by height in meter squared, and define overweight as BMI higher than 25. We define hypertension as systolic pressure of greater than 130 or diastolic pressure of greater than 80 at the time of the measurement.[13]

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". 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, we focus on children aged between 8 and 18 in each wave of the IFLS. For children in this age group, the IFLS collects the following health measures: height and weight, hemoglobin count, and self-reported general health status (this was obtained from the primary caregiver for respondents under 15). Our 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. These outcomes capture children's general growth trajectory and are considered good proxies of long-term and cumulative nutritional status (Falkner and Tanner, 1986; Mason, 1990). We also include anemia to capture children's nutritional status and self-reported general health to capture overall health status.

Using children's height and age, we calculate height-for-age z-score using the WHO reference data.[14] 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. Similar to the 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 self-reported poor health (instead of self-reported good health), thus higher values represent poor health outcomes.

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 school (6th grade) and secondary school (9th grade) exams. The scoring of the national

[13]Using the old definition of hypertension of systolic pressure of greater than 140 or diastolic pressure of greater than 90 yields similar results. With the new definition, around 60% of IFLS respondents are classified as hypertensive, which is consistent with rates in the US (Khera et al., 2018).

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

examination changed between the 1980s and 2000s, so for consistency of comparison across the years, we create z-scores for each year of examination.[15]

INPRES Exposure variable We combine administrative data on the number of INPRES primary schools built between 1974-1978 at the district level[16] with the IFLS.[17] We assign geographic exposure to the INPRES program using the number of schools constructed on the first generation's district of birth, since using district of residence during respondent's primary school age would be endogenous with respect to the program.

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.[18]

Summary Statistics The analyzed first generation sample includes around 12,000 adults born between 1950 and 1972 with information on their place of birth. Descriptive statistics for the sample are reported in Table A.1 (Panel A).

The first generation sample is balanced across gender. About 60% of the sample were born between 1963 and 1972, 46% of the sample are Javanese. The number of INPRES schools built in the first generation's district of birth is 2.1, consistent with the national average. 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. Two thirds of adults in the first generation are hypertensive and 40% of adults are overweight.

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

[15]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.

[16]We thank Esther Duflo for providing us with these data.

[17]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.

[18]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.

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 about a third 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 the INPRES elementary school construction program in Indonesia forty years later. Following Duflo (2001), we use a natural experiment design that exploits two sources of variation: 1) cohort variation by comparing respondents who were older than primary school age at the start of the INPRES construction in 1974 (individuals born between 1950-1962) to those who were under 12 in 1974 (individuals born between 1963-1972); and 2) geographic variation from the district of birth by exploiting the geographic intensity in the number of schools built across districts, which was determined by the enrollment rates in 1971 (prior to the program).

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 between 1963 and 1972, and thus was exposed to the INPRES school construction since he/she was in primary school age when the INPRES schools were constructed in 1974. $INPRES_d$ 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.[19] Standard errors are clustered at the district of birth level. β measures the effect of one school built per 1,000. 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 could violate the identification assumption. Using data from the 1985 Intercensal survey (SUPAS 1985), we show the similar trends in primary school completion for cohorts who finished primary school before the program was implemented for high and low program intensity districts.[20] 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 "fake 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 reassuring 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", comprising of individuals born between 1957-1962 (comparison cohorts) or 1968-1972,

[19]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.

[20]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".

that excludes the partially exposed. Each panel 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 percentages 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 (the "restricted sample").

This first-order finding on education motivates the analysis of the long-term effects of program exposure on adult health as a measure of well-being since education is an input to the production of health as human capital (Grossman, 1972). We begin by examining self-reported general health, followed by chronic conditions (self-reported), and mental health in 2012 (IFLS-E) and 2014 (IFLS-5), about 40 years after the program.

Table 1 presents the estimated program effect for two outcomes: good self-reported health (if a respondent reported his or her self status as 'very healthy' or 'healthy') shown in columns 1-3 and the number of days of missed regular activities in columns 4-6. We find that adults who were exposed to the INPRES program have better self-reported health 40 years later. Specifically, an additional school per 1,000 children improves self-reported health by 3.9 percentage points (an increase of around 5% of the mean) for adults partially and fully exposed to INPRES (Panel A, column 1). Columns 2 and 3 show that the impact is stronger for women (statistically significant 6.2 percentage points for women vs. 1.7 percentage points for men and not significant). We also find that exposure to the INPRES primary school construction program decreased the number of days of missed activities by 0.17 days (almost 10% of the mean) for treated adults in the expanded sample (Panel A column 4), with larger effects for women. Table 1 Panel B shows that the impacts are similar among individuals who were fully exposed.

Table 2 presents results of the estimated long-term effects of INPRES primary school construction on adult chronic conditions. We find that an additional primary school per 1,000 children lowers the likelihood of reporting any chronic conditions as adults by 2.5 percentage points (6% of the mean) for individuals who were partially or fully exposed to the program. This impact is stronger among treated women, who are 3.9 percentage points less likely to report a diagnosed chronic condition (Panel A, columns 1-3). In terms of the number of diagnosed conditions (columns 4-6), 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 shows that these patterns and magnitudes are similar among those fully exposed to the program.

We then examine the effects of the program on mental health as an additional marker of well-being in adulthood (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, particularly those fully exposed, are less likely to report symptoms of depression. This effect is stronger and statistically significant for women who were fully exposed to the program, who experience a decline in the CES-D depressive symptoms index by 9% of the mean (Panel B, column 3).

Since we assess the impacts of INPRES primary schools on several measures of self-reported health outcomes, inference may be a concern. To address this, we use the health index, where higher values of the index 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. These results are similar in the expanded and restricted sample.[21]

Additionally, we analyze physical health measures taken by health workers during the survey: blood pressure and BMI. We find an increase in the incidence of having high blood pressure and being overweight among men, but not women in the expanded sample (Table A.3). Among the fully exposed, we find a higher incidence of being overweight, and this is significant for women. These results are aligned with evidence of a positive association between SES and BMI in low and lower-middle income countries including Indonesia (Dinsa et al., 2012; Sohn, 2017). In spite of poorer outcomes for these health measures, including these outcomes in the health index yields similar results (Table A.4).

Taken together, those exposed to the INPRES program 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 the INPRES school construction 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.[22] In addition, many of the health markers examined in this section have been shown to be highly correlated with adult

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

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

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. 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. We also 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 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 part of the paper, we evaluate the effects of parental exposure to the INPRES school construction program on their offspring's human capital. The longitudinal feature of the IFLS, as well as the fact that it follows split-off households allows us to match parents with children and obtain information on parental INPRES exposure.[23] We estimate the following equation:

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

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

where y_{itd} is the outcome of interest (eg. height-for-age) 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 the INPRES school construction program 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. We estimate the model for the children of adults in the first generation who were born between 1950 and 1972 in the expanded sample, and the children of adults who were born between 1957-1962 or 1963-1972 in the restricted sample. In this model, the coefficient β captures the effect of parental exposure to one INPRES school built per 1,000 on the second generation's outcomes.

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. 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 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

The INPRES program is associated with increased primary completion rates among men and women, and this may subsequently improve the intergenerational transmission of human capital. We first examine the intergenerational effects of the INPRES program on children's education. Since parental education is one input to children's health, we also examine the effect of parental program exposure to an education intervention on children's health outcomes.

Education 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. Previous evidence documents that children born to women who were exposed to the INPRES program perform better on the national primary school examination (Mazumder et al., 2019). We extend this analysis to examine the intergenerational effect on the secondary school national examination scores. We find that children born to women who were exposed to the INPRES program perform better on the national secondary school examination (Table 4). On average, maternal exposure to one INPRES school (per 1,000 children) increases their children's secondary test scores by 0.06 standard deviations. Although noisy, the estimated effects are similar for boys and girls (Columns 2-3). We find a similar, but not statistically significant effect resulting from father's exposure to the program (Columns 4-6).

In Table 4 Panel B, we analyze the children born to the restricted first generation sample (adults born between 1957-1962 or 1963-1972, where treated cohorts are the fully exposed only) and find that children whose mothers were fully exposed to the program score 0.14 standard deviations higher in the secondary school examination, about double the estimated effect of the expanded sample that combines partial and fully exposed cohorts as treated (in Panel A). The magnitudes of the effects are similar for boys and girls (Columns 2-3). We find smaller and not statistically significant effects resulting from paternal INPRES exposure (Columns 4-6). A comparison of the estimated effects on children achievement scores using the expanded and restricted samples suggests that the intergenerational effects are concentrated among parents who were fully exposed to the INPRES program.

We also examine children's secondary school completion and find small and non statistically significant effects of maternal and paternal INPRES exposure (Table A.7, Columns 1 and 4 respectively).[24]The estimated effects of maternal and paternal exposure are similarly small and not significant for boys and girls in the expanded and restricted samples (Panel A, Columns 2-3, 5-6 and Panel B respectively). These results may be 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. The majority of the second generation individuals are affected by the compulsory schooling laws, so the effect of parental education appears to be

[24]We also examine children's cognitive skills and find no significant effect.

on the intensive margin of test scores, and not the extensive margin of school completion.[25]

Health We analyze the intergenerational effects of the INPRES program on several measures of health status. We begin with two measures that capture the cumulative effects of health investments: children's height-for-age and stunting. Given the timing of the IFLS and the fact that second generation children are born between 1975 and 2005, 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 an additional school per 1,000 children built under the INPRES program is associated with improved height in the next generation. We find a 0.056 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 non statistically significant effect among children whose fathers were exposed to the program (Columns 4-6). The estimated effects are noisy, but qualitatively similar when we focus on the fully exposed as treated cohorts (Panel B, restricted sample). 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 among children born to women who were exposed to the INPRES program, 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 school construction program (one school per 1,000 children in the 1970s) are 2 percentage points less likely to be stunted, which corresponds to 6% of the mean. We find no significant effect among children born to men who were exposed to the INPRES program. Using the restricted sample yields qualitatively similar results. Given Indonesia's high stunting rates and the adverse effects of stunting, our results suggest that social interventions that improve access to education have a spillover effect in reducing stunting rates and potentially improving later life outcomes of the offsrping of the exposed generation. These results are consistent with earlier work that finds an effect on infant mortality among children born to women who were exposed to the INPRES program, but not men (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

[25] 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.

mothers exposed to INPRES program are 4.4 percentage points less likely to be anemic (12% of the mean, per one school per 1,000 children). Also, paternal exposure to INPRES has a similar impact on daughters' anemia status. Regarding self-reported health status, table 8 shows that children born to INPRES mothers experience an increase in their likelihood of being healthy, with similar impacts for 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 0.035 standard deviations for each additional primary school constructed (per 1,000 children).[26] This effect is similar across sons and daughters in both the expanded and restricted samples.[27]

Taken together, mothers exposed to the program have children with better health and educational outcomes, which suggests the importance of interventions that improve future mothers' education to improve their children's 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-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.

Magnitudes

In sum, our analyses indicate that childhood exposure to a massive school construction program generates improvements in the next generation's human capital as measured by achievement test scores and health outcomes. On average, 1.98 schools were built per 1,000 children, which implies that, on average, maternal exposure to INPRES schools increases their children secondary test scores between 0.12 and 0.26 SDs. These magnitudes are comparable to effect sizes from other education interventions. For the case of Conditional

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

[27] 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 of outcomes 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 the INPRES program (one school per 1,000 children in the 1970s) 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 (See Table A.5).

Cash Transfers (CCT), Baird et al. (2014, 2011) find that 2 years of exposure to a CCT program in Malawi fthat ocused 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 SD. 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 SD in grade 10-11.

Regarding offspring's health, we find that average maternal exposure to INPRES improves their children's height for age z-score (HAZ) by 0.12SDs and decreases their probability of being stunted by 0.05 percentage points (14% of the mean). As a point of comparison, Fernald et al. (2008) examine the medium term effects of Mexico's CCT program on child health by leveraging the experimental design in the roll-out of the program across rural municipalities, variation in the amount of the transfer from length of exposure, and the number of children eligible. They find that doubling the cumulative cash transfers to the household was associated with an increase of 0.16 standard deviations in HAZ and a 9% lower prevalence of stunting among children ages 10-14. In addition, Barham (2012) investigates the medium-term impacts of the Matlab Maternal and Child Health and Family Planning Program in Bangladesh on children's human capital at ages 8-14 and finds that exposure to the program increases HAZ by 0.2SDs. Also, Nores and Barnett (2010) summarize the impacts from Early Childhood Interventions conducted in developing countries and report that average effect sizes on long-term child health outcomes (ages 7 or above) are around 0.12 SD. Overall, the magnitudes of our findigs 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

In this section, we present additional checks to confirm that the results are robust to different specifications and not subject to omitted factors.

6.1 Alternative specifications

Intergenerational effects: including both parents' exposure In general, we find larger effects from maternal INPRES exposure compared to paternal INPRES exposure. One concern in the models that estimate each parent's exposure separately is that the comparison group may be contaminated because of the program effects on 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 exposed to INPRES). For robustness, we include both maternal and paternal program exposure in our analysis (Table 9).[28]

We find that the effects of maternal INPRES exposure on their children's education and health outcomes is robust to the inclusion of paternal exposure. When paternal exposure is included, the estimated effect of maternal INPRES exposure on the national secondary examination is 0.05 to 0.08 standard deviations, which is similar to our earlier estimates. We also find that children whose mothers were exposed to the program are on average taller, by 0.06 standard deviations in height for age, and the effect is stronger for girls. While noisy, we find a similar point estimate for the effect of maternal exposure on stunting. 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.[29]

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 in exposure to the program, which 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.8 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 those from our main specification.

[28]We estimate a similar equation to equation 2 for mother's program 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. Including the original covariates for father's exposure makes the model more demanding our our data and less precisely estimated.

[29]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.

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.9).

7 Potential mechanisms

We explore potential mechanisms by which access to education through a massive primary school construction program leads to improved health of the first generation and human capital and health spillovers on the second generation. Theoretically, an education intervention such as INPRES can improve productive efficiency through health and non-health resources (Grossman, 1972; Kenkel, 1991).[30] Thus, we explore the impact of the program on household resources as a potential mechanism that can improve health outcomes for the first generation and human capital outcomes for the second generation. Also, our findings of the effects of INPRES on several markers of maternal health constitute an example of health resources as a channel underlying the human capital gains for the second generation. In addition, improving education may affect marital outcomes, fertility choices, and migration. Another potential channel through which improving education can impact long-term and intergenerational outcomes is human capital externalities such as improvements in political participation and the provision of public goods (Martinez-Bravo, 2017; Wantchekon et al., 2014). Thus, we take advantage of the IFLS community survey to examine proxies of neighborhood quality as a potential mechanism behind our results.

Household resources We examine the role of household resources by analyzing per capita consumption, which is a widely used proxy for household income and well-being[31], and housing quality as a proxy of the household environment. Several empirical studies have shown that household income and resources are an important input for an individual's health

[30] We unfortunately are not able to analyze the role of allocative efficiency through improved knowledge due to data limitation. Although the IFLS includes questions on breast cancer awareness for women in the first generation, the response rate is low.

[31] 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).

and children's human capital (Aizer et al., 2016; Akee et al., 2018). We use the IFLS-5 (in 2014) and IFLS-E (in 2012) to construct log per capita expenditure (in 2012 *Rupiah*) and a housing quality index. Log per capita expenditure is based on weekly or monthly per capita food and non-food expenditure.[32] The housing quality index is based on the following poor housing items: poor toilet, poor floor, poor roof, poor wall, and high occupancy per room. 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). We standardize each poor housing 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. Thus, higher values of the index reflect poor housing quality.

We find that individuals who were exposed to the INPRES program have better household resources as measured by higher per capita expenditure and housing quality, which may explain the improvements in outcomes for both the first and second generations (Table 11).[33] On average, individuals exposed to the program have about 5% higher per capita expenditure (columns 1-3) and they also have a lower index of poor housing quality, by 0.03 to 0.04 standard deviations (columns 4-6) for each additional INPRES school per 1,000 children.[34]

Migration

First generation: We explore the possibility of the first generation migrating, which could confound the effect of the program and exposure to their new community. We use two indicators for migration and estimate equation 1 (Table A.11, Panel A). First, we create 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. Second, we create an indicator that compares the respondent's district of birth and his or her district of residence in 2012 (IFLS-E) or 2014 (IFLS-5). We find that exposure to the INPRES program has no significant or sizable effect on these indicators of migration. Additionally, we estimate equation1 on the sample of non-movers and find similar estimates on long-term health (Table A.16).

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

[33]We explore the role of asset ownership by creating an index based on the following asset ownership: savings, vehicle, land, TV, appliances, refrigerator, and house. Although noisy, the results are consistent with higher asset ownership among exposed individuals (Table A.12).

[34]We explore the role of each item of the index and find that the results are consistent across the housing items (Tables A.14-A.15).

Second generation: Additionally, we explore the possibility of parents migrating to a potentially better community before or after the birth of their child by estimating equation 2 (Table A.11, 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.16).

Neighborhood quality Earlier work by (Martinez-Bravo, 2017) examines whether a massive intervention that targeted access to education like INPRES increased the level of education of potential candidates to local leadership positions, which in turn can improve local governance and the provision of public goods. The positive evidence from this study constitutes an example of human capital externalities and motivates us to examine the role of neighborhood quality as a potential mechanism behind our results. There is evidence that children's human capital is shaped by the neighborhood where they grow up (Chetty and Hendren, 2018; Chetty et al., 2016). Specifically, we use the IFLS community survey (IFLS-5 in 2014 and IFLS-E in 2012) to create the community's access to education and health services by creating an education and health service indices. 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.

Access to education 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). 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.[35] Similarly, the health index includes the following:

[35] 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.

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 access to education (Table A.17, columns 1-3).[36] In contrast, we find that individuals who were exposed to the INPRES program 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 0.69 standard deviations, columns 7-9). These results are in line with the INPRES program improving public good provision.

Marriage and Fertility First generation individuals exposed to the INPRES primary school program experience gains in their educational attainment and this may subsequently change their marital and fertility outcomes.

There is a large body of literature that have documented the marital returns of education in the form of positive assortative mating (Anukriti and Dasgupta, 2017; Behrman and Rosenzweig, 2002; Hahn et al., 2018), which in turn can improve offspring's outcomes. 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 (that pools as treated both those partial and fully exposed).

Another potential channel is changes in fertility outcomes. Improving women's education may increase the opportunity cost of having children, thus delaying childbearing (Becker and Lewis, 1973; Hahn et al., 2018). Also, better educated women may make better fertility choices through contraceptives (Kim, 2010). We investigate women's fertility responses to INPRES exposure, however, we find no significant effects on several markers including: the age of first pregnancy, number of live births, or birth spacing between the first and second child (Table A.10).[37]

[36] 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.

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

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 the main mechanisms Finally, we examine the potential contribution of some of the mechanisms presented in explaining the effects of INPRES on the first generation's health and the second generation's human capital. To do this, we rely on back of the envelope calculations using: i) estimated associations between each mechanism and our outcome of interest in the comparison cohorts; and ii) our estimated effects of INPRES on each mechanism and outcome of interest. It is worth noting that for part i), those associations are not causal as we lack a strong research design. 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 matching. Our calculations imply that household resources, neighborhood quality, and assortative matching 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.[38]

For the second generation calculations, we focus on the following mechanisms: household resources, community health services quality, maternal and paternal education. Our back

[38]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 points (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.17) 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 matching, the association between spouse primary school completion and individual's self-reported health is very large (5 percentage points - 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.

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 household resources and maternal education playing an important role.[39] The remainder would then be attributed to the contributions of the program effect on maternal health[40], other human capital externalities from INPRES, and other mechanisms we could not explore, such as maternal bargaining power.

8 Conclusion

This paper evaluates whether improving access to education through a massive school construction program has spillover effects on other dimensions of human capital. We present evidence that individuals exposed to the INPRES primary school construction program have better health outcomes 40 years later: better self-reported health status, fewer mental health symptoms, and fewer chronic conditions. These findings provide new evidence on the complementarities between education and health, and the magnitudes of these effects are aligned with evidence from other types of education interventions.

We also examine the intergenerational spillovers of the INPRES program and find that the offspring of mothers exposed to INPRES had significantly higher test scores, are less likely to be stunted, and have better self-reported health. We present evidence that these effects are not driven by fertility changes or migration. We find some evidence of household resources and neighborhood quality as potential mechanisms. Our results demonstrate

[39]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.17) 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 0.08 SDs. 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 effect and paternal education explain between 4% and 6%. We did similar calculations for other second generation outcomes. For 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 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.

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

that there are important intergenerational human capital spillovers from a large-scale school building program that took place in a middle-income country. Thus, these findings point to one important way in which policy makers can potentially foster greater intergenerational transmission of human capital.

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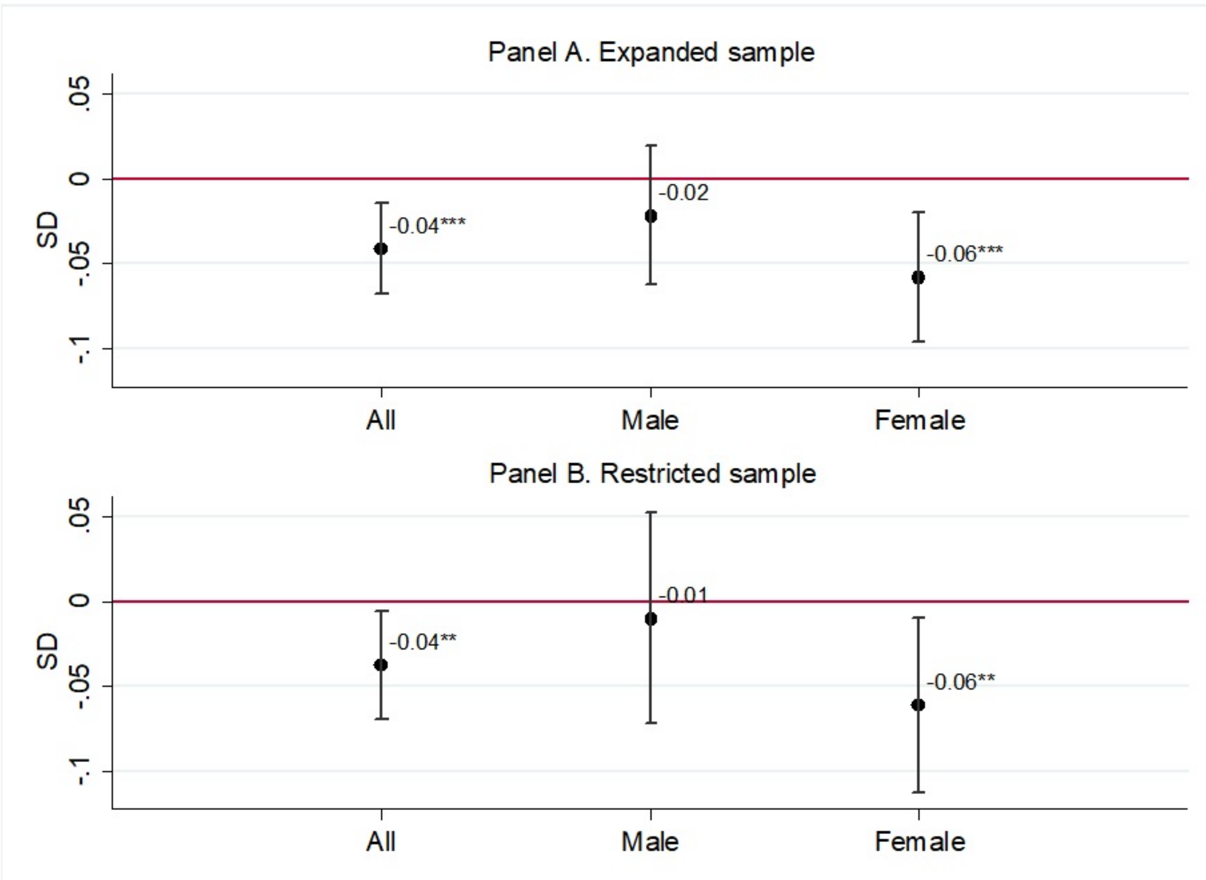
Tables and Figures

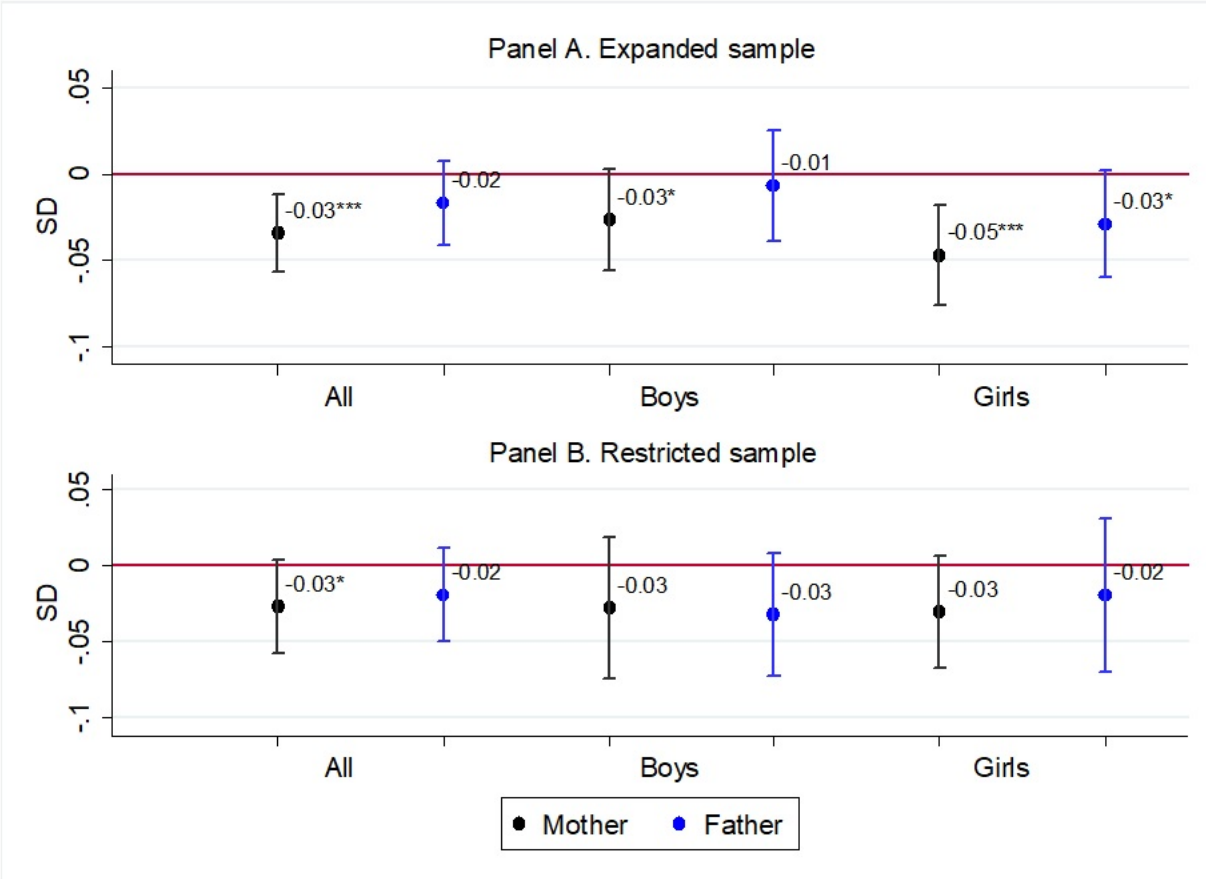
First generation estimations

Figure 1: 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 (higher scores correspond to more symptoms of depression). 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. District, year of birth, month of birth fixed effects included. 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.

Figure 2: INPRES intergenerational effects 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 reporting 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 birthx1971 enrollment, parent year of birthx1971 number of children, parent year of birthxwater sanitation program, child's gender, birth order, year and month of birth dummies, urban, and ethnicity. 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.

Table 1: Long-term outcomes: Self-reported health

	(1) All- Healthy	(2) Male	(3) Female	(4) All-Days missed	(5) Male	(6) Female
			Panel A: Expanded sample			
Born bet. 1963-1972	0.039***	0.017	0.062***	-0.171**	-0.116	-0.205*
X 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) All- Self-report	(2) Male	(3) Female	(4) All-Days missed	(5) Male	(6) Female
			Panel B: Restricted sample			
Born bet. 1968-1972	0.033***	0.006	0.060***	-0.079	-0.025	-0.102
X 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). 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. District, year of birth, month of birth fixed effects included. 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: Long-term outcomes: Chronic conditions

	(1)	(2)	(3)	(4)	(5)	(6)
	All-	Male	Female	All-	Male	Female
	Any condition			No. conditions		
	Panel A. Expanded sample					
Born bet. 1963-1972	-0.025**	-0.015	-0.039***	-0.047**	-0.030	-0.075**
X 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. conditions		
	Panel B. Restricted sample					
Born bet. 1968-1972	-0.014	0.003	-0.032*	-0.037	-0.009	-0.072*
X 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. District, year of birth, month of birth fixed effects included. 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: Long-term outcomes: Mental health

	(1) All- Score	(2) Male	(3) Female
	Panel A. Expanded sample		
Born bet. 1963-1972 X 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- Score	(2) Male	(3) Female
	Panel B. Restricted sample		
Born bet. 1968-1972 X 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 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. See Table 1 for covariates. Expanded sample includes those born between 1950-1972. Restricted sample includes those born between 1957-1962 or 1968-1972. Robust standard errors clustered at district of birth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Second generation estimations

Table 4: Intergenerational outcomes: National Secondary Test Scores

	(1) All	(2) Male	(3) Female	(4) All	(5) Male	(6) Female
	Panel A: Expanded sample					
Mother bet. 1963-72 X INPRES	0.057*	0.057	0.039			
	(0.032)	(0.054)	(0.053)			
Father bet. 1963-72 X INPRES				0.065	0.035	0.074
				(0.040)	(0.059)	(0.074)
No. of obs.	6484	3232	3252	5480	2703	2777
R-squared	0.13	0.18	0.19	0.12	0.17	0.19

	(1) All	(2) Male	(3) Female	(4) All	(5) Male	(6) Female
	Panel B: Restricted sample					
Mother bet. 1968-72 X INPRES	0.130**	0.127	0.128			
	(0.054)	(0.083)	(0.081)			
Father bet. 1968-72 X INPRES				0.091	0.019	0.159
				(0.073)	(0.104)	(0.103)
No. of obs.	3343	1638	1705	2506	1223	1283
R-squared	0.15	0.25	0.26	0.17	0.27	0.30

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. Test scores standardized for each exam year. Covariates include the following FE: parent year of birth and district of birth fixed effects, parent year of birthx1971 enrollment, parent year of birthx1971 number of children, parent year of birthxwater sanitation program, child's gender, birth order, year and month of birth dummies, ethnicity (Javanese dummy), exam 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-HAZ	(2) Male	(3) Female	(4) All	(5) Male	(6) Female
	Panel A: Expanded sample					
Mother bet. 1963-72 X INPRES	0.056*	0.036	0.081**			
	(0.030)	(0.041)	(0.033)			
Father bet. 1963-72 X 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-HAZ	(2) Male	(3) Female	(4) All	(5) Male	(6) Female
	Panel B: Restricted sample					
Mother bet. 1968-72 X INPRES	0.044	0.008	0.074			
	(0.046)	(0.062)	(0.049)			
Father bet. 1968-72 X 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 birthx1971 enrollment, parent year of birthx1971 number of children, parent year of birthxwater sanitation program, child's gender, birth order, year and month of birth dummies, urban, and ethnicity. 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-stunting	(2) Male	(3) Female	(4) All	(5) Male	(6) Female
			Panel A: Expanded sample			
Mother bet. 1963-72 X INPRES	-0.025*	-0.033**	-0.018			
	(0.013)	(0.017)	(0.016)			
Father bet. 1963-72 X 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-stunting	(2) Male	(3) Female	(4) All	(5) Male	(6) Female
			Panel B: Restricted sample			
Mother bet. 1968-72 X INPRES	-0.022	-0.021	-0.021			
	(0.020)	(0.023)	(0.026)			
Father bet. 1968-72 X 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: 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 birthx1971 enrollment, parent year of birthx1971 number of children, parent year of birthxwater sanitation program, child's gender, birth order, year and month of birth dummies, urban, and ethnicity. 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) Male	(3) Female	(4) All	(5) Male	(6) Female
	Panel A: Expanded sample					
Mother bet. 1963-72 X INPRES	-0.010	0.015	-0.034**			
	(0.011)	(0.013)	(0.015)			
Father bet. 1963-72 X 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) Male	(3) Female	(4) All	(5) Male	(6) Female
	Panel B: Restricted sample					
Mother bet. 1968-72 X INPRES	-0.011	0.006	-0.026			
	(0.014)	(0.018)	(0.021)			
Father bet. 1968-72 X 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 were taken starting 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 birthx1971 enrollment, parent year of birthx1971 number of children, parent year of birthxwater sanitation program, child's gender, birth order, year and month of birth dummies, urban, and ethnicity. 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) Male	(3) Female	(4) All	(5) Male	(6) Female
	Panel A: Expanded sample					
Mother bet. 1963-72 X INPRES	0.012**	0.015**	0.013*			
	(0.005)	(0.007)	(0.007)			
Father bet. 1963-72 X 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) Male	(3) Female	(4) All	(5) Male	(6) Female
	Panel B: Restricted sample					
Mother bet. 1968-72 X INPRES	0.005	0.016**	0.000			
	(0.006)	(0.007)	(0.009)			
Father bet. 1968-72 X 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 to 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 birthx1971 enrollment, parent year of birthx1971 number of children, parent year of birthxwater sanitation program, child's gender, birth order, year and month of birth dummies, urban, and ethnicity. 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	Male	Female	All	Male	Female
Panel A: Expanded sample						
Mother Born bet. 1963-72	0.074**	0.090	0.031	-0.037***	-0.020	-0.054***
X INPRES	(0.036)	(0.065)	(0.059)	(0.014)	(0.018)	(0.019)
Father Born bet. 1963-72	0.034	0.032	0.033	-0.004	-0.019	0.011
X INPRES	(0.041)	(0.068)	(0.067)	(0.015)	(0.021)	(0.019)
No. of obs.	6298	3126	3172	19042	9771	9264
R-squared	0.15	0.22	0.23	0.17	0.20	0.19
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Male	Female	All	Male	Female
Panel B: Restricted sample						
Mother Born bet. 1968-72	0.095*	0.012	0.153*	-0.024	-0.017	-0.041*
X INPRES	(0.053)	(0.091)	(0.083)	(0.017)	(0.022)	(0.021)
Father Born bet. 1968-72	0.063	0.054	0.003	-0.010	-0.008	-0.009
X INPRES	(0.054)	(0.091)	(0.081)	(0.016)	(0.022)	(0.023)
No. of obs.	4159	2049	2110	13707	7073	6617
R-squared	0.17	0.28	0.28	0.17	0.20	0.20

Notes: 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 and the interaction of father's district of birth and cohort indicators. 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. Covariates include the following FE: parent year of birth and district of birth fixed effects, parent year of birthx1971 enrollment, parent year of birthx1971 number of children, parent year of birthxwater sanitation program, child's gender, birth order, year and month of birth dummies, urban, ethnicity (Javanese dummy), exam year dummies. Robust standard errors in parentheses clustered at the mother's district of birth.

Potential mechanisms

Table 10: Potential mechanisms: Maternal marriage outcomes of 1st generation

	(1)	(2)	(3)	(4)	(5)	(6)
	Female Spouse			Men Spouse		
	Primary	Secondary	Age diff Husband−wife	Primary	Secondary	Age diff Wife−husband
	Panel A: Expanded sample					
Female bet. 1963-72 X INPRES	0.006	0.018	0.197			
	(0.013)	(0.016)	(0.271)			
Men bet. 1963-72 X INPRES				0.007	-0.025	0.079
				(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			Men Spouse		
	Primary	Secondary	Age diff Husband−wife	Primary	Secondary	Age diff Wife−husband
	Panel B: Restricted sample					
Female bet. 1968-72 X INPRES	0.032**	0.052**	0.326			
	(0.016)	(0.024)	(0.362)			
Men bet. 1968-72 X INPRES				0.008	-0.012	-0.191
				(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: See Table 1 for covariates. Robust standard errors clustered at district of birth. Significance: *** p < 0.01, ** p < 0.05, * p < 0.10.

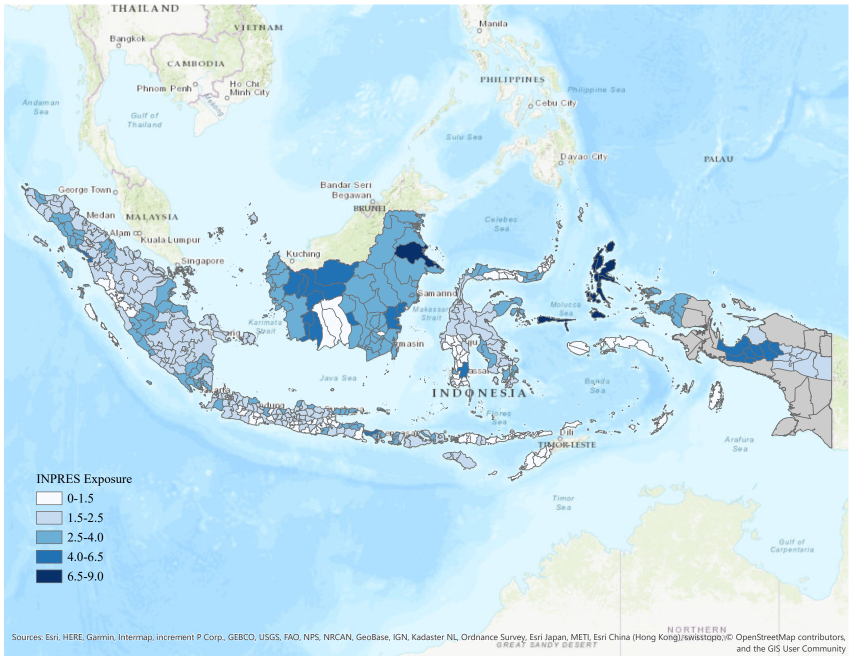
Table 11: Potential mechanisms: Household resources

	(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
X 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***
X 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: 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). See Table 1 for covariates. Robust standard errors clustered at district of birth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

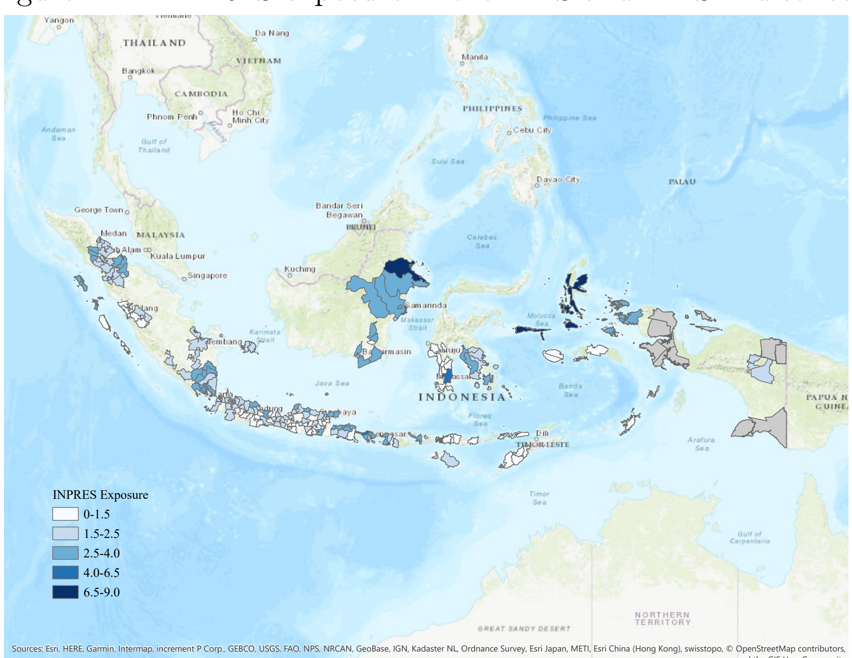
Appendix

Figure A.1: INPRES exposure - All Indonesia



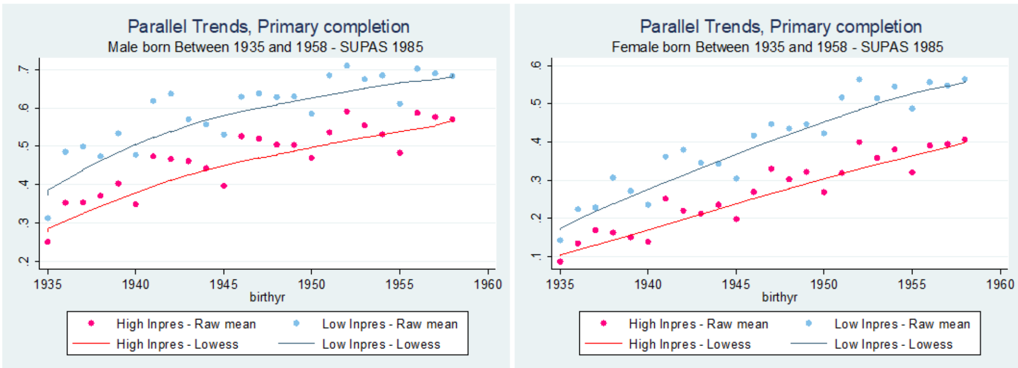
Source: Authors' calculations based on Duflo (2001)

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



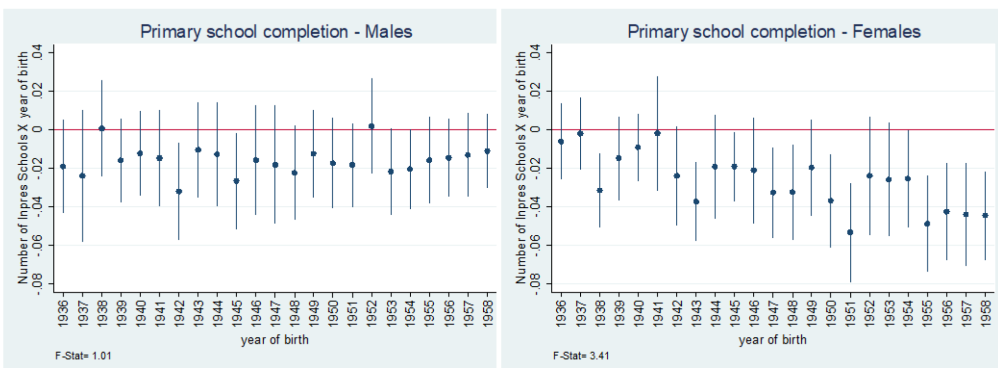
Source: Authors' calculations based on the IFLS, IFLS-E, and Duflo (2001)

Figure A.3: Pre-trends raw data: Primary school completion - SUPAS 1985



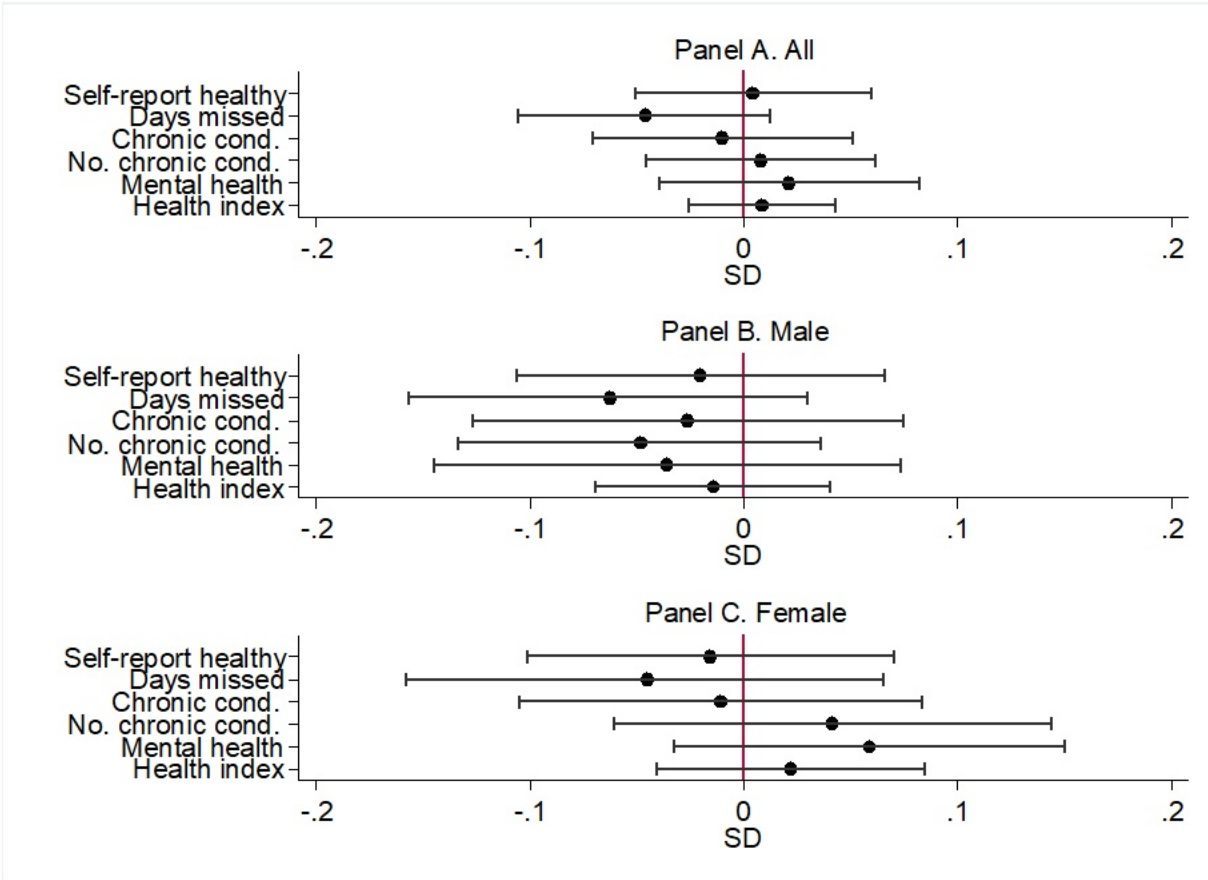
Notes:Primary completion rates for cohorts born between 1935 and 1958 from the 1985 Intercensal survey (SUPAS 1985).

Figure A.4: Pre-trends raw regression: Primary school completion - SUPAS 1985



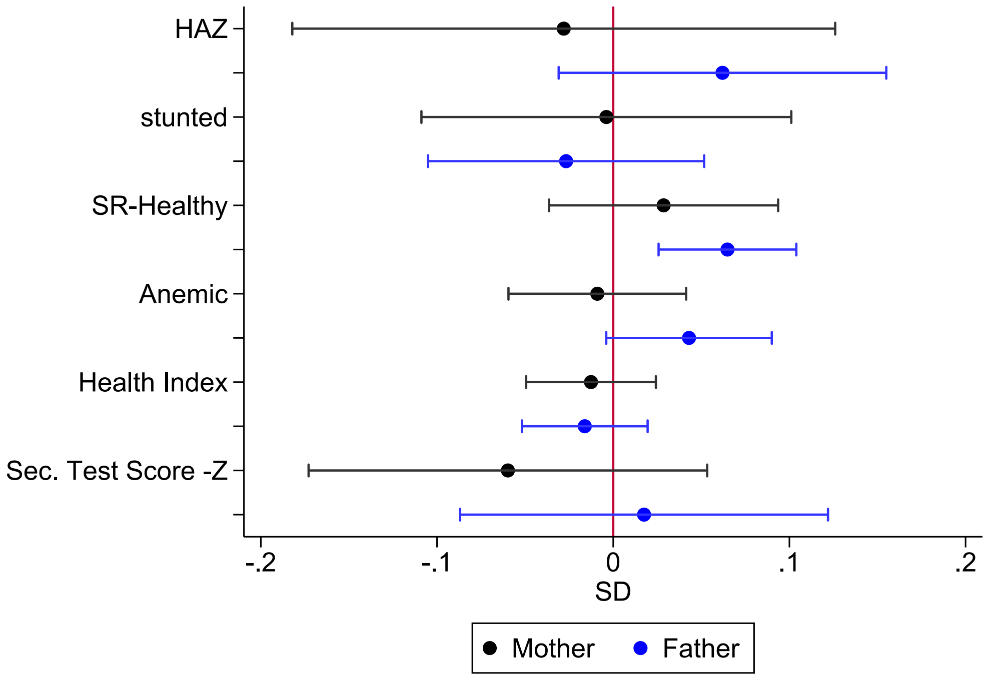
Notes: Coefficients from difference-in-differences model that interacts the number of INPRES schools and year of birth for cohorts born between 1935 and 1958

Figure A.5: Placebo regression: First generation



Notes: Sample includes individuals born between 1950 and 1962. "Fake INPRES" 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. Mean 0, SD 1 based on those born between 1950-1962 in low INPRES areas. District, year of birth, month of birth fixed effects included. 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.

Figure A.6: Placebo regression: Second generation



Notes: Sample includes the children of individuals born between 1950 and 1962. "Fake INPRES" for individuals 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. Mean 0, SD 1 based on those born between 1950-1962 in low INPRES areas. District, year of birth, month of birth fixed effects included. 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 parent's district of birth.

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
Average health index	-0.085	0.599	9,900
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
INPRES schools per 1,000	0.285	0.451	9,704
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 and Wave 6 of the IFLS. 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*
X 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 (1957-1962 or 1968-1972)			
Born between 1968-72	0.044***	0.032*	0.052***
X 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: Panel A includes individuals born between 1950 and 1972. Panel B includes individuals born between 1957-1962 or 1968-1972. Covariates include the following FE: year of birthx1971 enrollment, year of birthx1971 number of children, year of birthxwater sanitation program, year and month of birth dummies, ethnicity (Javanese dummy) and district of birth fixed effects. Robust standard errors in parentheses clustered at the district of birth. *** p<0.01, ** p<0.05, * p<0.1

Table A.3: Long term health: Blood pressure and BMI

	(1) All- High BP	(2) Male	(3) Female	(4) All- High BMI	(5) Male	(6) Female
	Panel A: Expanded sample					
Born bet. 1963-1972	0.002	-0.011	0.012	0.026**	0.032**	0.019
X INPRES	(0.009)	(0.014)	(0.012)	(0.012)	(0.015)	(0.017)
No. of obs.	10480	5072	5408	10046	5011	5035
Dep. var. mean	0.669	0.660	0.677	0.409	0.292	0.526
R-squared	0.07	0.09	0.11	0.12	0.11	0.10

	(1) All- High BP	(2) Male	(3) Female	(4) All- High BMI	(5) Male	(6) Female
	Panel B: Restricted sample					
Born bet. 1968-1972	-0.024**	-0.025	-0.015	0.027**	0.024	0.050**
X INPRES	(0.012)	(0.018)	(0.017)	(0.013)	(0.019)	(0.022)
No. of obs.	5837	2807	3030	5639	2781	2858
Dep. var. mean	0.640	0.632	0.647	0.421	0.302	0.537
R-squared	0.08	0.12	0.13	0.13	0.12	0.13

Notes: Expanded sample includes those born between 1950-1972. Restricted sample includes those born between 1957-1962 or 1968-1972. High blood pressure defined as systolic pressure greater than 130 or diastolic pressure greater than 80. High BMI defined as BMI greater than 25. District, year of birth, month of birth fixed effects included. 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 A.4: Long-term outcomes: Alternative health index

	(1) All	(2) Male	(3) Female
	Panel A. Expanded sample		
Born bet. 1963-1972 X INPRES	-0.021**	-0.008	-0.034**
X INPRES	(0.010)	(0.015)	(0.015)
No. of obs.	9891	4817	5074
R-squared	0.12	0.12	0.12
	(1) All	(2) Male	(3) Female
	Panel B. Restricted sample		
Born bet. 1968-1972	-0.024	-0.006	-0.029
X INPRES	(0.015)	(0.025)	(0.020)
No. of obs.	5537	2668	2869
R-squared	0.13	0.13	0.15

Notes: The alternative health index includes self-reported poor general health, days missed, any chronic conditions, number of conditions, mental health screening score, high blood pressure, and high BMI. See Table 1 for covariates. Expanded sample includes those born between 1950-1972. 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.5: Intergenerational: average poor health index

	(1) All-health index	(2) Male	(3) Female	(4) All	(5) Male	(6) Female
			Panel A: Expanded sample			
Mother bet. 1963-72 X INPRES	-0.036***	-0.022	-0.055***			
	(0.013)	(0.016)	(0.017)			
Father bet. 1963-72 X 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-health index	(2) Male	(3) Female	(4) All	(5) Male	(6) Female
			Panel B: Restricted sample			
Mother bet. 1968-72 X INPRES	-0.043**	-0.038	-0.053**			
	(0.018)	(0.030)	(0.021)			
Father bet. 1968-72 X 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: Children ages 8-18. FE: Parent's year of birthx1971 enrollment, year of birthx1971 number of children, year of birthxwater sanitation program, Javanese, month of birth. Child's gender, year of birth and age at survey. Average regressions weighted by the number of observations per child. Robust standard errors in parentheses clustered at the maternal district of birth. *** p<0.01, ** p<0.05, * p<0.1

Table A.6: Intergenerational outcomes: Robustness anemia and self-reported health

	(1) All-healthy	(2) Male	(3) Female	(4) All-anemic	(5) Male	(6) Female
	Panel A: Expanded sample					
Mother bet. 1963-72 X INPRES	0.012**	0.011	0.018**	-0.014	0.002	-0.030*
	(0.006)	(0.009)	(0.008)	(0.011)	(0.015)	(0.016)
Father bet. 1963-72 X INPRES	-0.001	0.006	-0.010	-0.006	-0.011	-0.001
	(0.006)	(0.009)	(0.007)	(0.010)	(0.014)	(0.013)
No. of obs.	19852	10167	9679	19217	9866	9343
Dep. var. mean	0.880	0.886	0.874	0.245	0.221	0.271
R-squared	0.29	0.31	0.31	0.07	0.11	0.10

	(1) All-healthy	(2) Male	(3) Female	(4) All-anemic	(5) Male	(6) Female
	Panel B: Restricted sample					
Mother bet. 1968-72 X INPRES	0.006	0.009	0.008	-0.018	-0.012	-0.030
	(0.008)	(0.010)	(0.009)	(0.013)	(0.018)	(0.019)
Father bet. 1968-72 X INPRES	-0.004	0.000	-0.009	-0.008	-0.004	-0.010
	(0.007)	(0.009)	(0.009)	(0.011)	(0.016)	(0.017)
No. of obs.	14303	7366	6922	13821	7137	6666
Dep. var. mean	0.892	0.898	0.886	0.242	0.221	0.265
R-squared	0.26	0.27	0.29	0.09	0.13	0.13

Notes: Sample corresponds to children born to first generation Inpres individuals. Covariates include the following FE: parent year of birthx1971 enrollment, parent year of birthx1971 number of children, parent year of birthxwater sanitation program, child's gender, birth order, year and month of birth dummies, urban, ethnicity, and religion. Robust standard errors in parentheses clustered at the parent's district of birth

Table A.7: Intergenerational outcomes: Complete secondary school

	(1) All	(2) Male	(3) Female	(4) All	(5) Male	(6) Female
Mother Born between 1963-72	-0.020	-0.002	-0.025			
X INPRES	(0.014)	(0.026)	(0.019)			
Father Born between 1963-72				0.004	-0.014	0.013
X 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) Male	(3) Female	(4) All	(5) Male	(6) Female
			Panel B: Restricted sample			
Mother Born between 1968-72	-0.029	-0.041	-0.018			
X INPRES	(0.024)	(0.042)	(0.029)			
Father Born between 1968-72				0.009	-0.081	0.094
X 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. Test scores standardize for each exam year. Covariates include the following FE: parent year of birth and district of birth fixed effects, parent year of birthx1971 enrollment, parent year of birthx1971 number of children, parent year of birthxwater sanitation program, child's gender, birth order, year and month of birth dummies, ethnicity (Javanese dummy), exam year dummies. Robust standard errors in parentheses clustered at the parent's district of birth

Table A.8: Robustness: Alternative exposure based on schools built per year

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	First gen: Health index			Second gen			
				Test score		Health index	
	All	Male	Female	Maternal	Paternal	Maternal	Paternal
Born bet. 1963-1972 X 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. See Table 1 for covariates. Expanded sample includes those born between 1950-1972. Robust standard errors clustered at district of birth (parent's district of birth for the second generation). Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.9: Robustness: Alternative cohorts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	First gen: Health index			Second gen			
				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
X 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
X 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 for covariates. Robust standard errors clustered at district of birth (parent's district of birth for the second generation). Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.10: Potential mechanisms: Fertility

	(1) Age 1st preg.	(2) N of births	(3) Spacing 1st and 2nd child
	Panel A: Expanded sample		
Female born between 1963-72	-0.179	-0.024	0.364
X 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) N of births	(3) Spacing 1st and 2nd child
	Panel B: Restricted sample		
Female born between 1968-72	-0.125	-0.054	0.303
X 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: 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 mechanisms: Migration

	(1)	(2)	(3)	(4)
	Expanded sample		Restricted Sample	
	Ever moved	Moved by 2012-14	Ever moved	Mig. 2012-14
Panel A: First generation				
Born between 1963-72	-0.002	0.005	-0.000	0.001
X 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 between 1963-72	-0.006	0.014	0.022	0.033
X 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. 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: Long-term outcomes: Asset index

	(1) All- Score	(2) Male	(3) Female
	Panel A. Expanded sample		
Born bet. 1963-1972	0.024	0.024	0.028
X INPRES	(0.028)	(0.034)	(0.035)
No. of obs.	11951	6007	5944
R-squared	0.20	0.22	0.22
	(1) All- Score	(2) Male	(3) Female
	Panel B. Restricted sample		
Born bet. 1968-1972	0.037	0.024	0.074**
X 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. See Table 1 for covariates. Expanded sample includes those born between 1950-1972. 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: Potential mechanism: Household resources

	(1) All- Food	(2) Male	(3) Female	(4) All- Non-food	(5) Male	(6) Female
	Panel A: Expanded sample					
Born bet. 1963-1972	0.040***	0.038*	0.041	0.053**	0.050	0.064
X 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- Food	(2) Male	(3) Female	(4) All- Non food	(5) Male	(6) Female
	Panel B: Restricted sample					
Born bet. 1968-1972	0.040*	0.052	0.031	0.069**	0.062	0.105**
X 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*). See Table 1 for covariates. Expanded sample includes those born between 1950-1972. 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.14: 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
X 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
X 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*
X 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: See Table 1 for covariates. Expanded sample includes those born between 1950-1972. Robust standard errors clustered at district of birth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.15: 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**
X 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**
X 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
X 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 1 for covariates. 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.16: Robustness: Non-movers only

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	First gen: Health index			Second gen			
				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**
X 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*
X 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: See Table 1 for covariates. Expanded sample includes those born between 1950 and 1972 and remained in their district of birth in 2012-14. Restricted sample includes those born between 1957-1962 or 1968-1972 and remained in their district of birth in 2012-14. Robust standard errors clustered at district of birth (parent's district of birth for the second generation). Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.17: Potential mechanism: Neighborhood quality

	(1) All-Edu	(2) Male	(3) Female	(4) All-Health	(5) Male	(6) Female	(7) All-Poor	(8) Male	(9) Female
Born bet. 1963-1972	0.014	0.059	-0.003	0.024	0.036	0.014	-0.003	0.015	-0.022
X 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) All-Edu	(2) Male	(3) Female	(4) All-Health	(5) Male	(6) Female	(7) All-Poor	(8) Male	(9) Female
Born bet. 1968-1972	-0.001	0.022	0.008	0.055**	0.036	0.079*	-0.012	0.017	-0.069*
X 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*). See Table 1 for covariates. Expanded sample includes those born between 1950-1972. Restricted sample includes those born between 1957-1962 or 1968-1972. Robust standard errors clustered at district of birth and community. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.