Review of Economics and Statistics Fellowship for NBER Summer Institute

Name: Maria Fernanda Rosales-Rueda

Email: maria.rosales@rutgers.edu

Affiliation: Rutgers University - Newark

Date of Ph.D.: 2014

Website: www.mariafrosales.com

Summary of current research activity: As an applied microeconomist, I am interested in studying the determinants of human capital formation across the life cycle. I am interested in understanding why human capital gaps between socio-economically diverse children occurred and how these gaps contribute to educational, health, and wage inequalities. Secondly, how public policies and economic investments can close those gaps? Through my scholarly work, I am contributing further insights into how policies that promote human capital accumulation can improve the wellbeing of vulnerable children and families worldwide and, thereby inform the development of future evidence-based social policy.

In particular, my main research area examines the impacts and interactions of family investments and social interventions in the process of human capital formation in the short and long-term, and whether these effects translate to generations to come. To this end, a recent paper with Bhash Mazumder and Maggie Triyana accepted at Journal of Human Resources study the long-term health and intergenerational effects of Indonesia's INPRES primary school program. Following this line of research, in the context of the US War on Poverty programs, I am currently collaborating with Marianne Bitler on a project that examines how exposure to health, nutrition and educational programs interact with one another across the life-cycle, and the channels through which these programs improve multiple human capital outcomes. Additionally, among my planned future research, I am working on new project entitled: Nurturing Parents and Children in Rural Indonesia: An Early-Stimulation Intervention (along with H. Baker, M. Triyana and E. Satriawan), where we will implement a cluster Randomized Control Trial (RCT) to explore complementarities between an evidence-based early childhood stimulation program (Reach-up and Learn- RL) and a component that targets father co-parenting engagement in poor families with children age 0-3 in Indonesia.

Another area of my research studies the link between environmental factors and human capital outcomes. Two published papers (at Journal of Health Economics and Journal of Human Resources) have examined the medium and long-term effects of early-life exposure to floods and air pollution on children's human capital and health outcomes in Ecuador and Indonesia.

Being a REstat Fellow will provide me with unique research exposure to connect with different top-notch academics working in similar topics and expand my network. I am located in a small department in a satellite campus and I do not have the usual possibilities of regular seminars to build and nurture an academic network. Therefore, I am always seeking for opportunities to attend and present at conferences like the NBER Summer Institute.

Summer Institute session(s) most interested in attending: Economics of Children, Health Economics, Development Economics, Education and Labor Economics

Maria Rosales-Rueda

CONTACT INFORMATION
maria.rosales@rutgers.edu
www.mariafrosales.com

ACADEMIC POSITIONS

Assistant Professor 2018 - present
Department of Economics
Rutgers University - Newark

Visiting Assistant Professor 2016-2018
Department of Economics and Center for Health and Wellbeing
Princeton University

Assistant Professor, School of Education, 2014-2018
and Department of Economics (by Courtesy)
University of California-Irvine

EDUCATION

University of Chicago, Harris School of Public Policy, Chicago, IL

Ph.D., Public Policy, June 2014

- Committee: Kerwin Charles (chair), Dan Black, James Heckman, and Robert LaLonde

Master of Public Policy, with honors, 2009

Universidad de los Andes, Bogota, Colombia

B.A and M.A. in Economics, 2005

RESEARCH FIELDS Labor Economics, Human Capital, Health, Development Economics

PUBLICATIONS

Articles in Peer-Reviewed Journals

"Social Interventions, Health and Well-being: The Long-term and Intergenerational Effects of a School Construction program", with Bhash Mazumder and Maggie Triyana. *Forthcoming at Journal of Human Resources*

"Reducing the Cost of Remoteness: Community-Based Health Interventions and Fertility Choices," with Catalina Herrera. *Journal of Health Economics*, September 2020, Volume 73, 102365.

"The Effects of Early-Life Exposure to Pollution on Children's Human Capital: the Case of Indonesia," with Maggie Triyana, *Journal of Human Resources*, Fall 2019, 54.4: 1037-1080.

"Intergenerational Human Capital Spillovers: Indonesia's School Construction and its Effects on the Next Generation," with Bhash Mazumder and Maggie Triyana *AEA Papers and Proceedings*, May 2019, Vol. 109.

"Impact of Early-Life Shocks on Human Capital Formation: Evidence from El Niño Floods in Ecuador," *Journal of Health Economics*, November 2018, Vol. 62, Pg. 13-44.

"Maternal Age and Child Achievement," with Greg Duncan, Ken Lee and Ariel Kalil, *Demography*, December 2018, Volume 55, Issue 6, pp 2229-2255.

"Family Investment Responses to Childhood Health Conditions: Intrafamily Allocation of Resources," *Journal of Health Economics*, September 2014, Volume 37: 41-57.

"Productivity and its Determinants: the Case of the Colombian Industry," with Juan Echavarria and Maria Arbelaez, Universidad de los Andes, *Revista Desarrollo y Sociedad*, 2006, No. 57: p77-122.

Chapters in Books

Cardenas, M., and M. Rosales (2006)."The Evolution of Financial Markets," Chapter 7 in " Introduction to the Colombian Economy," Textbook Alfaomega.

Cardenas, M., and M. Rosales (2006). "Public Finance in Colombia," Chapter 5 in "Introduction to the Colombian Economy," Textbook Alfaomega.

Working Papers

"Integrating Early-life Shocks and Human Capital Investments on Children's Education," with Valentina Duque and Fabio Sanchez. September 2020, Working Paper. *Under Review*

"The Returns of an Additional Year of Schooling: The Case of State-Mandated Kindergarten," with Jade Jenkins

"A Reanalysis of the Nurse Family Partnership Program: The Memphis Randomized Control Trial," with James Heckman, Maggie Holland, David Olds, and Rodrigo Pinto. December 2016, Working Paper, University of Chicago, Department of Economics.

"Financial Dependence, Formal Credit, and Informal Jobs: New Evidence from Brazilian Household Data," with Luis Catao and Carmen Pages, IZA Discussion Papers 4609, Institute for the Study of Labor.

Work in Progress

"The Life-Course Effects of Access to WIC," with Marianne Bitler and Esra Kose.

"Nurturing Parents and Children in Rural Indonesia: an Early-Stimulation Intervention," with Raquel Bernal, Helen Henningham, Elan Satriawan and Maggie Triyana.
Funding: USAID Development Innovation Ventures ($200,000) - pending administrative process. Status: Fieldwork in preparation. AEA RCT Registry (6360)

"Leveraging Technology to Promote Positive Family Behaviors and Child Wellbeing in Indonesia," with Gumilang Sahadewo, Elan Satriawan, Martin Siyaranamual and Maggie Triyana.
Funding: Asian Development Bank ($100,000). Status: Fieldwork in preparation.

"Does Delivery of Primary Health Care Improve Birth Outcomes? Evidence from the roll-out of Community Health Centers" with Esra Kose and Siobhan O'Keefe

"The Long-Term Impacts of Conditional Cash Transfer Programs on Human Capital: What has been Accomplished Over the Last Two Decades?" with Valentina Duque and Fabio Sanchez

Grants, Honors and Awards

- R01 HD093898, National Institutes of Health (PI M. Bitler) 2018-2023
  Investments, Life Events, and Health Within and Across Generations
  (Rutgers Sub-award: $137,668)
- Research Council Grants, Rutgers University ($5,000) 2019-2021
- Global Grants, Rutgers University ($5,000) 2019-2021
- Spencer Foundation Small Research Grant 2018
  (Co-PI with Jade Jenkins, $50,000)
- Economic Self-Sufficiency Policy Research Institute (ESSPRI) Research Grant 2018
  (Co-PI with Jade Jenkins, $75,000)
- Economic Self-Sufficiency Policy Research Institute (ESSPRI) Seed Grant 2017
  (Co-PI with Jade Jenkins, $15,000)
- Overdeck Education Research Innovation Fund. Princeton University 2017

- World Bank. Invited to contribute to the regional study "Aggregated Shocks, Poverty and Ex-ante Risk Management in Latin America" 2015
- Hewlett Foundation/Population Research Bureau Research Grant (Co-PI with Catalina Herrera, $45,000) 2015-2017
- Development Bank of Latin America, CAF (Co-PI with Valentina Duque, $10,000) 2015
- 1^{st} Place – Ph.D Student Paper Competition, Illinois Economic Association 2013
- Center of Human Potential and Public Policy Pre-Dissertation and Dissertation Award 2012 2013
- Robert T. Michael Research Prize, NORC at University of Chicago 2010, 2011
- Field Qualifying Exam: Microeconometrics with distinction, Harris School 2010
- Harris School of Public Policy- University of Chicago, Ph.D Fellowship 2009-14
- Harris School of Public Policy-University of Chicago, M.P.P. Fellowship 2007-09

Research Experience

Summer Dissertation Fellow, Inter-American Development Bank Summer 2013
Research Assistant to Prof. James Heckman 2010-2014
Center for the Economics of Human Development, University of Chicago
Summer Intern, Inter-American Development Bank Summer 2008
Junior Professional Associate, The World Bank 2005 - 2007
Junior Researcher, Fedesarrollo, Colombia 2002 - 2005

Teaching Experience

Department of Economics, Rutgers University
- Health Economics (Undergraduate) 2018-present
- Principles of Microeconomics (Undergraduate) 2019-present

Department of Economics, Princeton University
- Health Economics (Undergraduate) 2017, 2018

School of Education, University of California, Irvine
- Statistics for Education Research (Undergraduate) 2015, 2016
- Educational, Social, and Behavioral Statistics (PhD) 2014, 2015

Invited seminars and conference presentations

2021: Allied Social Science Associations (ASSA) Annual Meetings, University of Florida (scheduled).

2020: Temple University, Eastern Economics Association, NBER Children Spring meetings, Rutgers, 2nd IUSSP Population, Poverty and Inequality Research Conference.

2019: ASSA Annual Meetings, Population Association of the America (PAA) Annual Conference, NBER Summer Institute, Southern Economic Association Conference, Fourth Annual PHS Research Workshop-UPenn.

2018: ASSA Annual Meetings, PAA Annual Conference, Paris Sorbonne Sustainable Development Seminar, Lafayette College, University of Wisconsin-Madison, NBER Development Meetings (Pre-conference), Third Annual PHS Research Workshop-Boston University.

2017: ASSA Annual Meetings, PAA Annual Conference, World Bank DIME, University of Delaware, Second Annual Population Health Science (PHS) Research Workshop-Boston University.

2016: ASSA Annual Meetings, Harvard GSE, Brandeis University, Association for Education Finance and Policy Annual Conference (AEFP), PAA Annual Conference, Universidad de los Andes, IZA/SOLE Transatlantic Meeting of Labor Economists, Association for Public Policy Analysis and Management (APPAM) International Conference, Princeton University.

2015: University of San Francisco, Pacific Conference on Development Economics - UCSD; PAA Annual Conference, CAF Research Program on Skills for Life and Work;

World Bank workshop: "Aggregated Shocks, Poverty and Ex-ante Risk Management in Latin America and the Caribbean", APPAM Annual Research Conference, University of Southern California.

2014: Society of Labor Economists (SOLE) Annual Meetings, Launch of the Center for the Economics of Human Development (University of Chicago), International Workshop on Education Quality -ICFES Colombia, APPAM Annual Research Conference.

2013: PAA Annual Conference, SOLE Annual Meetings, Illinois Economic Association, University of Chicago Center for Human Potential and Public Policy, Northeast Universities Development Consortium (NEUDC)-Harvard University.

2012: University of Chicago Center for Human Potential and Public Policy, CESifo Economic Studies and UCLS Conference, Latin America Economic Association (LACEA) Annual Meetings, APPAM Annual Research Conference.

Refereeing Activities

American Economic Journal: Applied Economics, AERA Open, Contemporary Economic Policy, Demography, Developmental Psychology, Economics and Human Biology, IZA Journal of Labor Policy, Journal of Health Economics, Journal of Human Resources, Journal of Policy Analysis and Management, Journal of Political Economy, Review of Economics and Statistics, Review of Economics of the Household.

Service Activities

Doctoral Committee Member: UC Irvine: Ken Lee (2015), Marcela Reyes (2016). Rutgers: Mriga Bansal (current).

Conference sessions organization: APPAM (2014), SEA(2019), ASHEcon(2020).

Conference discussant: NEUDC (2013), APPAM (2014), PAA (2016-2019), AEFP(2016), AEA(2021) (2017)

Mentoring: Workshop for female 3rd Year PhD students in economics (SEA 2020), APPAM Virtual Mentoring Program (2020), Session Participant at Association for Mentoring and Inclusivity in Economics - 1st Workshop in Applied Microeconomics, for PhD students (Scheduled)

Social interventions, health and wellbeing: The long-term and intergenerational effects of a school construction program[1]

Bhashkar Mazumder[2]
Maria Fernanda Rosales[3]
Margaret Triyana[4]

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 experience improved health and educational outcomes and that these effects are generally stronger for maternal exposure than paternal exposure. We find some evidence that household resources, neighborhood quality, and assortative mating may explain a portion of our results. Our findings highlight the importance of considering the long-run and multigenerational benefits when evaluating the costs and benefits of social interventions in a middle-income country.

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

JEL Codes: J13, O15, I38

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[2]Federal Reserve Bank of Chicago and University of Bergen, bhash.mazumder@gmail.com
[3]Rutgers University, maria.rosales@rutgers.edu
[4]Wake Forest University, mtriyana@alumni.uchicago.edu

1. Introduction

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

For developing countries that embark on large-scale school building initiatives, the potential societal gains may be significantly larger than current research may suggest. There is increasing recognition that in addition to improving economic outcomes, educational policies may produce important spillovers to other aspects of well-being, such as health (Oreopoulos and Salvanes, 2011; Galama, Lleras-Muney and van Kippersluis, 2018; Heckman, Humphries and Veramendi, 2018). Furthermore, human capital gains that are intergenerationally transmitted may lead to future benefits. However, causal evidence of such spillovers and intergenerational effects is limited, particularly in developing countries (Wantchekon, Klašnja and Novta, 2015). A failure to consider these potential benefits could dramatically understate the value of social interventions designed to improve human capital.

We focus on these non-pecuniary and intergenerational spillovers by studying a massive primary school construction program in Indonesia known as the INPRES program. In particular, we examine the effects of this program on the long-term health of individuals who were exposed to these schools (first generation) and the human capital outcomes of their offspring (second generation). Under INPRES, the Indonesian government constructed 61,000 elementary schools between 1974 and 1979, doubling the existing stock of schools, thus making schools available to

children where none or few had existed before. Our study builds upon earlier studies of this program. Duflo (2001), Duflo (2004), Breierova and Duflo (2004) and Zha (2019) show that INPRES affected the education, marriage market, fertility, and labor-market outcomes of individuals exposed to these schools, while Martinez-Bravo (2017) documents the impacts on local governance and public good provision on Indonesia's main island, Java. The main goal of INPRES was to increase primary school attendance, and in a companion paper (Mazumder, Rosales-Rueda and Triyana, 2019), we show that in fact, the program had large effects on primary school completion.[1]

We demonstrate 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 five waves between 1993 and 2014, and the Indonesian Family Life Survey-East (IFLS-E), which includes one wave in 2012. These data allow us to track the offspring of INPRES-exposed individuals and examine important effects on the human capital outcomes of the next generation. Like previous studies, we exploit variation in the rollout of INPRES across Indonesian districts. This enables us to utilize two sources of variation: 1) geographic variation across districts in the intensity of school building; and 2) cohort variation by comparing individuals who were of primary school age or younger to those who were older than primary school age at the inception of the program. As far as we are aware, we are the first paper to use the IFLS and IFLS-E to explore the long-term health and intergenerational effects of INPRES.

Our first set of results documents significant and meaningful effects on the long-term health of individuals in the first generation. We create a summary index of several health measures: self-reported poor health, the number of days on which daily activities were missed due to health reasons, chronic conditions, and depressive symptoms. About 40 years after the program took

[1] Other recent papers that use the INPRES project include: Ashraf *et al. (*2020) who study bride price and female education; Bharati, Chin and Jung (2016) who analyze program impacts on adult time preferences; Bharati, Chin and Jung (2018) who examine whether INPRES mitigates the effects of adverse weather shocks; and Karachiwalla and Palloni (2019) who examine the impacts on participation in agriculture. We separately discuss, below, a recent paper by Akresh, Halim and Kleemans (2019) that also looks at long-term and intergenerational effects.

place, we find that health is improved by 0.04 to 0.06 standard deviations (SDs) per school built per 1,000 children relative to the comparison group. We also examine gender differences and find that these effects are generally stronger for women.

Our second set of results examines the impacts on the children of cohorts exposed to the rollout of INPRES, i.e., the second generation. Specifically, maternal access to INPRES elementary schools increases children's height-for-age by 0.06 SDs and reduces the likelihood of childhood stunting by 7% of the mean. We also find improvements in children's self-reported health. We construct an index of health outcomes for the second generation and find an improvement in health of about 0.03 SDs.

We also analyze children's test scores in the national 9th-grade examination and find that children born to women exposed to INPRES score between 0.08 and 0.10 SDs higher. In general, we find similar impacts for sons and daughters, and smaller and statistically insignificant effects from fathers' exposure to INPRES. Overall, our results for the first and second generations are robust to alternative specifications, and we show evidence from event study analyses and placebo regressions on the comparison group that validate the empirical strategy.

There are several pathways through which improved human capital (due to INPRES) could lead to better first- and second-generation outcomes. These include: assortative mating, fertility, household resources, neighborhood quality, and migration.[2] We find mixed evidence suggesting that women exposed to INPRES are more likely to marry better educated men. With respect to fertility, we find no evidence that INPRES changed the timing of the first birth or the spacing of births. While we find that women exposed to the program experience a decline in their total fertility, our "back-of-the envelope" calculations imply that reduced fertility only explains up to 8 percent of our second-generation effects. We also show that individuals exposed to INPRES have greater household resources as measured by per capita consumption and housing quality and that they are more likely to live in communities with better access to health services. Lastly, our

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

evidence suggests that migration responses do not drive our results.

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

The study closest to ours is Akresh, Halim and Kleemans (2019), henceforth "AHK", who also examine long-term and intergenerational effects of INPRES using data from the 2016 *Susenas*, a large-scale national survey. Our studies differ in two important ways. First, AHK consider different outcomes from us, which makes our studies complementary. For the first generation, they examine various measures of economic wellbeing (e.g. expenditures, housing and assets, employment) whereas we focus on a range of self-reported health outcomes. For the second generation, while they mainly focus on children's education, we highlight effects on several measures of health as well as a range of human capital outcomes (e.g. height, test scores).

Second, AHK use cross-sectional data in 2016 that only allow them to identify intergenerational links from children who are *co-resident with their parents during adulthood*. In contrast, the longitudinal aspect of the IFLS allows us to observe parents and children over many years, including children who form new households and no longer live with their parents. This is a critical distinction as we find that fewer than half of all children born to parents in the first-generation sample are still coresident with their parents by 2014 and, that they tend to be positively selected.[4] Therefore, although the *Susenas* has significantly larger samples, the IFLS has the advantage of providing a representative intergenerational sample.

[3] See Appendix C.1 for the details of our calculations.

[4] Recent studies have also shown that relying on coresident samples can lead to biased estimates of intergenerational mobility (Emran and Shilpi, 2018; Asher, Novosad and Rafkin, 2020). We also find that the cohorts exposed to INPRES are more likely to be co-resident with their parents than children born to comparison cohorts.

Our paper adds to several literatures. First, we contribute to research on the long-term effects of interventions on human capital. While most papers have studied high income countries, there is now emerging evidence from lower income countries that have mainly focused on demand-side educational interventions.[5] Our analysis is unique by examining the long-term effects of a common supply-side educational intervention, school construction.

Second, we provide new evidence on the causal effects of schooling on health in the context of a middle-income country. Most evidence thus far, relies on randomized variation in pre-school access or quasi-experiments that exploit changes to compulsory-schooling laws. Thus far, the research on the causal effects of education on health are mixed (e.g. Oreopoulos and Salvanes, 2011; Galama, Lleras-Muney and van Kippersluis, 2018).[6]

Third, we add to the small but growing literature on the causal intergenerational effects of policies most of which has been studied in high-income countries.[7] We are among the first to study the intergenerational impacts of school construction on post-infancy outcomes in a relatively lower-income setting. Documenting that there are intergenerational effects is highly policy relevant, as current debates about such social programs may underestimate their benefits.

The remainder of the paper is organized as follows. Section 2 provides an overview of the program, and 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. Section 5 describes the study's methods and results for the second generation. Section 6 presents additional robustness checks,

[5] Almond, Currie and Duque (2018) provide an extensive review of the evidence from high income countries. Studies that examine the long-term effects of demand-side interventions in low-income countries include conditional cash transfers (Parker and Vogl, 2021), vouchers (Bettinger et al., 2017) and compulsory schooling laws (Agüero and Ramachandran, 2020).

[6] Plausible mechanisms have been proposed for how education can improve health. One theoretical perspective hypothesizes that education can improve both productive and allocative efficiency (Grossman, 1972; Kenkel, 1991). Through productive efficiency, more education leads to a higher marginal product for a given set of health inputs. Through allocative efficiency, more highly educated individuals choose to put more efficient inputs into health investment. Examples of these efficiencies include greater financial resources, improved knowledge, better decision-making ability, and changes to time preferences.

[7] For example, there is evidence from Head Start and Medicaid in the US (East *et al.*, 2019; Barr and Gibbs, 2019), and compulsory education in Sweden (Lundborg, Nilsson and Rooth, 2014). Studies examining low- and middle-income countries include: Chou et al. (2010); Wantchekon, Klašnja and Novta (2015); Grépin and Bharadwaj (2015); and Agüero and Ramachandran (2020).

and Section 7 discusses some potential mechanisms. Section 8 concludes with cost-benefit calculations and policy implications.

2. Background

2.1 The Indonesian context and INPRES Program

Primary school enrollment in Indonesia was around 65 percent in the 1960s (Booth, 1998) and the illiteracy rate on the 1971 Census was 31 percent. In 1973, President Suharto took advantage of the oil boom and created the INPRES Primary School program (*Sekolah Dasar* INPRES, or SD INPRES) to expand primary school access[8], and other INPRES programs to promote regional economic development (Duflo, 2001).

INPRES was implemented beginning in 1974 with the goal of reaching at least 85 percent of primary-school-aged children, who usually start their six years of primary schooling between the ages of 6 and 7. To increase equity in access to basic education, INPRES targeted areas with low primary school enrollment such as poorer and more rural provinces outside the main island of Java (Duflo, 2001). By 1979, the program had constructed over 61,000 primary schools, increasing the number of schools available by 2 schools per 1,000 children (Duflo, 2001). This made INPRES one of the fastest primary school construction interventions on record (World Bank, 1989). By 1981 primary school enrollment reached 91 percent.

INPRES improved school quality in many dimensions. Schools were provided equipment and adequate water and sanitation (Duflo, 1999; World Bank, 1989). Double shifts, where some students would attend the morning session while others would attend the afternoon session, were eliminated, thereby increasing instruction time. The student-teacher ratio was also significantly improved [9] and there were increases in teacher salaries and improvements in teacher training through the construction of primary-teacher training schools (World Bank, 1989). In addition,

[8] In the rest of the paper, we simply refer to SD INPRES as INPRES.

[9] The student-teacher ratio was reduced to roughly 40 to 50 to one from 50 to 60 to one in the 1950s. https://unesdoc.unesco.org/ark:/48223/pf0000014169eng. Last accessed May 22, 2019.

primary school registration fees were eliminated in 1977.

A parallel INPRES program focused on improving water and sanitation and sought to build 10,500 piped-water connections and 150,000 toilets in villages. 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.

2.2 Previous evidence on school construction interventions

Previous evidence on INPRES

We summarize the literature evaluating INPRES in Appendix table B.1. The table breaks down the literature into 4 categories: first generation medium-term effects; first generation general equilibrium effects and human capital externalities; first generation long-term effects; and first generation long-term effects and intergenerational effects.

The literature begins with the seminal paper by Duflo (2001) who demonstrates that exposure to INPRES increased educational attainment and wages for men. A subsequent set of studies examines the impacts of INPRES on other medium-term outcomes. The program increased women's education (Duflo and Breinova, 2004), however these impacts are driven by women from bride price ethnic groups (Ashraf et al., 2020). INPRES also increased the likelihood of working in non-agricultural sectors for men (Karachiwalla and Palloni, 2019). A second set of studies has examined the general equilibrium effects of this large-scale program on the labor market (Duflo, 2004), the marriage market (Zha, 2019) and on human capital externalities (Martinez-Bravo, 2017). Martinez-Bravo (2017) found that INPRES led to a significant increase in the provision of public goods in villages with greater exposure, due to more educated village heads.

A set of recent studies that examine the long-term effects of INPRES finds persistent positive effects on socio-economic well-being outcomes such as household assets, expenditures, and tax payments (Akresh, Halim and Kleemans, 2019). Also, INPRES improved time preferences by increasing patience in adulthood (Bharati et al., 2016). We add to this literature by showing that INPRES improved health outcomes for the first generation 40 years later. As the first generation

becomes elderly, future research should examine the impacts of INPRES on mortality.

The evidence of long run effects of INPRES raises the question of whether these impacts persist to the subsequent generation. In a companion paper, we find that children born to mothers exposed to INPRES score higher in primary test scores (Mazumder, Rosales-Rueda and Triyana, 2019). Akresh, Halim and Kleemans (2019), who we discuss in greater detail in section 3, find that maternal exposure to INPRES increases the children's educational attainment. Our paper extends this emerging literature and shows that children born to mothers exposed to INPRES have better human capital outcomes such as stunting and secondary school test scores.

Other school construction interventions

Improving access to education through school building is a popular supply-side intervention in low- and middle-income countries, where students may need to travel long distances to reach the closest school (Glewwe and Kremer, 2006; Kazianga et al., 2013). Several studies examine whether improvements in school infrastructure have causal effects on enrollment and various short- and medium-term student outcomes such as test scores (for a recent review of the literature, see Glewwe and Muralidharan, 2016). A few studies of historical interventions have begun to explore longer-term outcomes, intergenerational effects and other dimensions of human capital such as health outcomes. Wantchekon, Klašnja and Novta (2015) find that colonial-era missionary schools built in Benin had human capital spillovers and intergenerational effects. Chou et al. (2010) find that the 1968 Taiwanese compulsory schooling reform, which included a school construction component, improved birth weight and infant health in the next generation.

A related literature has analyzed the effects of the Rosenwald Schools, which were built for Blacks living in rural parts of the American South largely during the 1920s. The institutional setting for these schools is similar to what many developing countries face today. Aaronson and Mazumder (2011) show that the schools led to significantly higher educational attainment and test scores. Aaronson et al. (2014) and Aaronson et al. (2020) show that the Rosenwald schools impacted fertility patterns and improved long-term health.

2.3 The effect of education on health

One of the most striking findings in the social sciences is the gradient in health and mortality by socioeconomic status (e.g Cutler et al., 2011). However, whether this association is truly causal and whether it can be influenced by educational polices is less clear and may depend on the context. If large-scale school-construction programs such as INPRES can improve not only educational attainment but also provide meaningful spillovers to health and have intergenerational health effects, then the case for such interventions becomes even more salient.

The empirical evidence of the causal effects of education on health appears to be mixed (see Galama et al. (2018) for a recent review).[10] One possible explanation for the mixed findings in the literature is that education may induce other behavioral changes, such as migration, and depending on the setting, these other behavioral changes can lead to worse health and therefore obscure the health-promoting aspects of education (Aaronson et al., 2020). Unlike much of the previous literature that has examined pre-school or compulsory schooling reforms, we provide evidence about the effect of expanding access to primary education on an individual's own physical and mental health in a lower-income setting.

A growing number of studies extend the analysis of the effects of education on the health of the offspring in the next generation. In general, parental education, especially that of the mother, has been shown to be a strong predictor of children's outcomes, such as birth weight (Currie and Moretti, 2003). However, changes in compulsory schooling laws in several countries have resulted in mixed evidence. Evidence from the UK shows few effects on child health (Lindeboom et al., 2009), while positive effects have been found in Taiwan (Chou et al., 2010), Zimbabwe (Grépin and Bharadwaj, 2015), Turkey (Dursun et al., 2017), and Indonesia (Breinova and Duflo, 2004). While these studies focus on children's outcomes up to age 5, we examine children's outcomes when they are older to evaluate the persistence of the health effects. Our study contributes to the

[10] Studies that exploit compulsory schooling laws across various countries (e.g., Clark and Royer, 2013; Oreopoulos, 2007), affecting different age groups in different time periods, provide mixed evidence about the effects of education on various health outcomes (Mazumder, 2012). For smoking and obesity, analyses from several high-income countries shows mixed evidence and some heterogeneity by gender and demographic characteristics (Galama et al., 2018).

small but growing literature on the intergenerational effects of large-scale education programs in lower-income countries.

3. Data

We use data from the Indonesian Family Life Survey (IFLS). Our IFLS sample 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.[11] In 1993, IFLS-1 included 7,224 households residing in 13 provinces, which covered more than 200 districts. The IFLS-E included 2,500 households residing in seven provinces in eastern Indonesia, which covered about 50 districts. Thus, the main IFLS and IFLS-E encompassed almost 300 of Indonesia's 514 districts. The IFLS is well suited for our analysis since the main IFLS is longitudinal. This characteristic allows us to not only track long-term outcomes of adults in the original survey, but to also track their offspring who can be observed at multiple points in time including adulthood as they form new households. As we show below, this is important for maintaining a representative sample of the families that were potentially affected by INPRES. The IFLS is also valuable as it includes a comprehensive set of socio-demographic measures of interest.

Sample of interest

Our first-generation individuals were born between 1950 and 1972. Those born between 1950 and 1962 were older than primary school age (age 12) at the time INPRES began (in 1974) and thus were not exposed, while those born between 1963 and 1972 were younger than age 12 during INPRES and thus could benefit from the primary school expansion. We call this sample the

[11] 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 effects (Mazumder, Rosales-Rueda and Triyana, 2019).

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

We believe it is useful to present results from both the expanded and restricted samples. The main advantage of the expanded sample is that it enables us to have greater statistical power, so we focus on this sample in our discussion of findings. The restricted sample has less statistical power, however it uses cohorts where the treatment was more targeted.

Long-term Outcomes

Our long-term outcomes use various measures of self-reported health. We use responses to a series of questions on self-reported health status and chronic conditions to construct the following outcomes: good self-reported health, the number of days a respondent missed activities due to health reasons in the past 4 weeks, any diagnosed chronic conditions (e.g., hypertension or diabetes), and the number of diagnosed conditions. Self-reported health is highly predictive of well-known health markers such as mortality in both high- and lower-income countries (DeSalvo et al., 2005; Idler and Benyamini, 1997; Razzaque et al., 2014).

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

having depression.[12] Because some of our outcomes of interest are only assessed for individuals older than 40, we use information from the IFLS-E in 2012 and IFLS-5 in 2014.

Our main outcome of interest is a summary index of health which we construct following Kling et al. (2007) and Hoynes et al. (2016). This approach also avoids the concern of multiple hypothesis testing. We standardize each health outcome by subtracting the mean and dividing by the standard deviation of the comparison group. We adjust the sign of the standardized outcomes so that higher values represent poorer health and then take a simple average of all the standardized outcomes. The components include self-reported poor health,[13] the number of days missing one's primary activity due to health reasons, any diagnosed chronic conditions, the number of chronic conditions, and the mental health score.[14] These outcomes are measured in 2012 (IFLS-E) and 2014 (IFLS-5) when individuals were at least 40 years old. In addition to the index, we also present results for each component.[15]

Intergenerational Outcomes

The second generation comprises of children born to our first generation sample of parents. They were born between 1975 and 2006. We use the following health measures: height, weight, hemoglobin count, and self-reported general health status (reported by the primary caregiver for those under 15). Given the timing of the IFLS survey years and the second generation's wide range of birth years, the oldest individuals in the second generation were 18 in 1993 (in IFLS-1) and the youngest were 8 in 2014 (in IFLS-5). Therefore, for the second generation's health outcomes, we focus on children aged between 8 and 18 in each wave of the IFLS.

[12] Details of the construction of these variables are available in Appendix A.
[13] This is an indicator for those who report not "very healthy" or "not healthy".
[14] We exclude health care utilization from our analysis due to the low incidence of preventive care in developing countries, including Indonesia. In our sample, only 8 percent of respondents reported obtaining at least one general health check-up in the 5 years prior to the survey.
[15] We also separately explore two other health outcomes that we did not include in our index: Body Mass Index (BMI) and high blood pressure. It is not clear a priori whether BMI is a positive or negative outcome, since there is evidence of a positive association between SES and BMI in developing countries, including Indonesia (Dinsa et al., 2012). High blood pressure (systolic pressure higher than 130 mm Hg or diastolic pressure higher than 80 mm) is only measured on the survey date, so no diagnosis of hypertension is made. We prefer to use self-reported diagnosed hypertension, which is one component of the chronic conditions included in the summary index discussed above.

Our health outcomes of interest for the second generation include height-for-age and stunting, defined as more than two standard deviations below the mean in height-for-age z-score.[16] These health measures capture children's general growth trajectory and proxy for long-term and cumulative nutritional status. We also include anemia from children's hemoglobin count as another proxy for children's nutritional status. To capture overall health status, we add self-reported general health. Similar to the first-generation long-term outcomes, we summarize these health outcomes for the second generation by creating a health index. The components include being stunted, being anemic, and having self-reported poor health (instead of self-reported good health); thus, higher values represent poorer health outcomes.[17]

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) examinations.[18] For consistency of comparison across the years, we create z-scores for each year of examination.[19] In this study, we focus on the secondary school examinations, since our companion paper (Mazumder et al., 2019) analyzes the intergenerational impacts of INPRES on primary test scores.

INPRES Exposure variable

We combine administrative data on the number of INPRES primary schools built between 1974 and 1979 at the district level with the IFLS.[20] We assign geographic exposure to the INPRES program using the number of schools constructed in the first generation's district of birth, since

[16] We use the 2007 WHO growth chart, which is applicable to children between 0 and 19 years of age.
[17] Details of the construction of these variables are available in Appendix A.
[18] While Indonesia has had national examinations since 1950s, the standardized national curriculum was only implemented in 1975 and the national examination after the curriculum standardization began in 1980. Thus, test scores are only available for the second generation. Indonesia implemented 6 years of compulsory schooling in 1984 and expanded that to 9 years of compulsory schooling in 1989, which corresponds to lower secondary. We are not able to examine the end of secondary school, which is 12th grade, examinations since most of the second children were too young in 2012 and 2014.
[19] The scoring of the national examination changed between the 1980s and 2000s.
[20] Appendix Figures A.1 and A.2 show the comparison between the national INPRES rollout and the provinces covered by the main IFLS and IFLS-E. We are not aware of other previous studies that have utilized the IFLS-E to study the effects of INPRES on these outcomes.

using district of residence during respondents' primary school age is potentially endogenous. For the second generation, we use the household roster to identify biological mother-child and father-child pairs born to the first-generation individuals, who were themselves born between 1950 and 1972.[21]

Summary Statistics

The first-generation sample includes around 12,000 adults born between 1950 and 1972 with information on their place of birth and observed in any wave of the IFLS.[22] For the first generation's long-term outcomes, the analyzed sample consists of around 10,200 individuals observed in the IFLS-E in 2012 or the IFLS-5 in 2014 (Table A.1, Panel A).

The average number of INPRES schools built in the first generation's district of birth is 2.1, consistent with the national average. The first-generation sample is balanced across gender. About 60 percent of the sample were born between 1963 and 1972, and 46 percent are Javanese, the main ethnic group in Indonesia. First-generation individuals score an average of 5.5 (out of 28) on the mental health screening questionnaire, 72 percent of adults in the sample reported being healthy, and 42 percent reported having at least one diagnosed chronic condition.

The second-generation sample consists of about 10,000 individuals who are the children of the members of the first-generation sample (Table A.1 Panel B). The sample is balanced across gender, and about 40 percent are first-born. The average year of birth is 1988. About half of the children have mothers who were born between 1963 and 1972, and similarly, about half have fathers who were born between 1963 and 1972. Children's health measures were taken in each wave, and we use observations between the ages of 8 and 18. The average height-for-age z-score

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

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

is -1.6 and 36 percent are stunted. About one fourth of the second generation has anemia, and almost 90 percent self-report good health.

The Benefit of Longitudinal Data for Intergenerational Analysis

A key advantage of the IFLS data is that the longitudinal nature allows us to observe offspring of the first generation at multiple points in time starting in 1993 as well as follows split-off households formed after children enter adulthood and leave their parents' household.[23] Therefore, our intergenerational sample is fairly representative of the universe of children born to parents in both comparison and INPRES exposed cohorts. In contrast, studies in developing countries often rely on cross-sectional data to form intergenerational samples, thereby selecting on children who are still coresident with their parents in adulthood. This is the case in Akresh, Halim and Kleemans (2019) ("AHK") who study the intergenerational effects of INPRES using adult children who live with their parents in the 2016 Susenas.[24] Studies have shown that relying on co-residency leads to biased estimates of intergenerational mobility (Asher et al., 2020; Emran and Shilpi, 2018).

We investigate this further and find that the representativeness of our intergenerational sample is altered if we were to use only coresident children in 2014, the latest year in our data.[25] This roughly mimics the sample construction in AHK. We find that around 55 percent of children born to the first-generation sample are no longer coresident with their parents by 2014 in IFLS-5.[26] This highlights the fact that in our context, relying on coresidency of adult children with their parents leads to a dramatic loss of sample.

We compare the characteristics of coresident and non-coresident children in IFLS-5 (Table

[23] For the small subset of our intergenerational sample (less than 10 percent) that is formed using the IFLS-E, we only include children who were coresident with their parents as adults. Attrition rates across the five waves of the IFLS are relatively low: the original household re-contact rate was 92 percent in IFLS-5, and 87.8 percent of original households in IFLS-1 were either interviewed in all 5 waves or died (Strauss et al., 2016).
[24] Other examples of studies in developing countries that have formed intergenerational samples based on coresidence include Nimubona and Vencatachellum (2007) and Emran and Shilpi (2015).
[25] We do this exercise for the expanded sample.
[26] The corresponding number for our restricted sample is 54 percent.

A.2). Coresident children are more likely to be younger, come from households with a higher asset index,[27] their parents have a higher education level, and their mothers were older at the time of their first birth. Also, coresident children are more likely to be born to mothers in the INPRES exposed cohort, which means that this sample captures a lower fraction of children born to comparison cohorts.[28]

Overall, the IFLS data provide some clear advantages over cross-sectional samples such as that used by AHK. The main disadvantage of a survey like the IFLS, is the relatively small sample size, which can limit statistical power. Given the tradeoff between sample size and bias, it is useful to consider results from both the IFLS and other studies such as AHK that use larger samples but may suffer from bias due to selection on coresidence.

4. The First Generation

4.1 Estimation Strategy

In the first part of the analysis, we estimate the long-term effects of INPRES on outcomes measured roughly forty years after program implementation. Following Duflo (2001), we exploit variation in exposure to the primary schools by birth cohort and geography as described in Section 3. We estimate the intent-to-treat effects using the following equation [29]:

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

[27] The asset index includes the ownership of the following assets: savings, vehicle, land, TV, appliances, refrigerator, and house.

[28] We also compare the characteristics of coresident children in IFLS-5 to coresident children in the 2014 Susenas to show that coresident families in the IFLS are similar to the national sample (Table A.3). When we restrict the 2014 Susenas to all IFLS provinces, the coresident-child characteristics in the IFLS are similar to the national survey sample (Table A.3, columns 4-6). Additionally, when the 2014 Susenas is restricted to the main IFLS provinces, the coresident child characteristics are similar to the coresident children in IFLS-5 (Table A.3, columns 7-9).

[29] A recent paper by De Chaisemartin and d'Haultfoeuille (2018) proposes a fuzzy DID framework to examine treatment effects when the treatment is not sharp as there is no group completely untreated. We explored this alternative approach, but it is not feasible in our context as it requires a categorical classification of the treatment (for example, high- versus low-intensity INPRES exposure). We found that we did not have sufficient power with our sample sizes to implement this Wald-DID approach.

where y_{idt} is the outcome of interest for individual i born in district d in year t. $exposed_t$ is a dummy variable equals to 1 if individual i was born in the relevant birth cohorts exposed to INPRES. In the expanded sample, this indicator takes the value of 1 for cohorts born between 1963 and 1972.[30] $INPRES_d$ 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 schools and for other programs that could confound the INPRES school effects. X_{idt} is a set of individual characteristics: gender, ethnicity (Javanese indicator), and month-of-birth fixed effects.[31] Standard errors are clustered at the district of birth level. β measures the effect of one school built per 1,000 children. We estimate the models pooled and separately by gender.

The causal effect of exposure to INPRES schools is identified if 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 intensity districts, this might suggest that our identification assumption is not credible. Using the IFLS raw data, we show that there are similar trends in primary school completion for cohorts who finished primary school before the program was implemented in high and low program intensity districts (Figure B.1).[32]

To validate our empirical strategy, we also estimate a version of equation 1 for pre-program

[30] In the restricted sample, the exposed cohorts were born between 1968 and 1972.
[31] Our specification modestly improves upon Duflo (2001) in two ways. First, we use a dummy for whether the individual is Javanese, the largest ethnic group in Indonesia. Second, we include month of birth to control for seasonality (Yamauchi, 2012). In Mazumder Rosales-Rueda and Triyana (2019), we use the identical covariates as Duflo (2001) (i.e., excluding month of birth and ethnicity) and found similar effects on primary school completion.
[32] Following Duflo (2001), high intensity districts are defined as districts "where the residual of a regression of the number of schools on the number of children is positive".

cohorts to examine pre-trends for the health and education outcomes of interest in the first generation. We find no significant pre-trends for cohorts before the INPRES school program (Figures B.2 and B.3). Additionally, we perform placebo regressions using the IFLS data on comparison cohorts (individuals born between 1950 and 1962) for each health and education outcome (Figure B.4). These regressions define a "placebo-exposed group" as cohorts born between 1957 and 1962, while those born between 1950 and 1956 serve as the comparison group. We find small and statistically insignificant effects on all of our health outcomes, thereby providing reassuring evidence about the absence of pre-trends before program implementation.

4.2 First Generation Long-term Effects

In a previous paper, Mazumder, Rosales-Rueda and Triyana (2019), we showed that INPRES increased primary school completion, the margin that the program targeted. We replicate the estimates in Table B.2. Panel A presents estimates for the "expanded sample", comprising of individuals born between 1950 and 1972. The treated cohorts, those born between 1963 and 1972, pool partially and fully exposed individuals. Panel B presents estimates for the "restricted sample" that excludes the partially exposed. Recall, that this sample includes those born between 1968 and 1972 in the treated group, and those born between 1957 and 1962 in the comparison group. Each panel has three columns: all, male, and female. In our discussion, we focus on the expanded sample, but the results are robust to using the restricted sample. We find that exposure to INPRES raises the probability of completing primary school for both men and women. The effects range between 2.5 and 3 percentage points per primary school per 1,000 children.[33]

We next turn to analyzing the long-run health outcomes in the first generation. We begin by examining the poor-health index, which includes self-reported poor health, the number of days missing one's primary activity due to health reasons, any chronic conditions, the number of chronic

[33] To compare our results to a nationally representative sample, we also use the 2014 Socioeconomic Survey (2014 Susenas) to examine the program effect on primary completion (Table A.4). Using the 2014 Susenas, which is representative at the district level, the program effect on primary completion for all provinces ranges from 1.1 to 1.7 percentage points. When we restrict the Susenas to the IFLS and IFLS-E provinces, the program effect on primary completion ranges from 1.5 to 2.5 percentage points, similar to our findings using the IFLS and IFLS-E data. These results provide evidence of the representativeness of the IFLS and IFLS-E.

conditions, and the mental health score. Figure 1 and Table 1 (col. 1) show that adults exposed to the INPRES primary schools experience a decline in the poor-health index by 0.04 standard deviations for each additional primary school constructed (per 1,000 children), and the effects are similar for the restricted sample.

We also perform an event study analysis (Figure 2). We plot the relationship between the poor-health index and the first generation's age when INPRES was implemented by estimating an equation similar to equation 1. To reduce the noisiness in the data, we create three-year bins of birth cohorts. Among cohorts who were too old to be exposed to the program, we find no significant pre-trends as the coefficients bounce around zero. For exposed cohorts, the point estimate for each cohort group is negative. While the estimated coefficients using the event study framework are not statistically significant at conventional levels, they are consistent with our earlier estimate using the difference-in-differences framework (shown in the far right of Figure 2). These results show that individuals exposed to the INPRES program have better overall health about 40 years later compared to the comparison group.

We then analyze the components of the health index to examine each individual outcome separately (Table 1, cols. 2-6). We find that an additional school per 1,000 children improves good self-reported health by 3.9 percentage points, which is about 6% relative to the mean (col. 2). Also, exposure to INPRES decreased the number of days of missed activities by 0.17 days, which is almost 10% of the mean (col. 3). We find that an additional primary school per 1,000 children lowers the likelihood of reporting any chronic conditions by 2.5 percentage points (5% of the mean, col. 4). In terms of the number of diagnosed conditions (col. 5), INPRES exposure is associated with a 0.05 fewer diagnosed chronic illnesses for each additional primary school constructed (per 1,000 children). Finally, we examine the program effect on adult mental health as an additional marker of well-being (col. 6). We find that adults exposed to INPRES are less likely to report symptoms of depression. This effect is stronger and statistically significant in the restricted sample when we focus on individuals who were fully exposed to the program (an effect of 5% of the mean, Panel B). Overall, these findings are consistent with the health improvement shown by the health index.

To address concerns about inference due to multiple hypothesis testing, we find that the components of the index are jointly significant (p-value = 0.0003). We also adjust the standard errors for multiple hypothesis following Simes (1986) and most of our results are robust to this adjustment (Table B.3).

We also estimate the program effects separately for men and women (Table 2 and Figure 1). We find that the program impact is concentrated among women, for whom the effect corresponds to -0.06 standard deviations in the poor-health index. When we analyze the components separately, we find that the point estimates are generally significant for women. For self-reported health status, there is a statistically significant increase of 6.2 percentage points for women (col. 4), whereas for men, the point estimate is 1.7 percentage points and not statistically significant at conventional levels (col. 3). We test for the gender difference using the seemingly unrelated regression model and find a significant difference, suggesting a stronger effect for women. For the other health outcomes, the effects on the number of days of missed activities, any chronic condition, and the number of chronic conditions are significant for women, but the estimated gender differences are not statistically significant. These results suggest that the health improvement is generally stronger for women, which may be an important channel for the intergenerational effect of the program.[34]

Taken together, those exposed to INPRES have better self-reported physical and mental

[34] We also examine hypertension and Body Mass Index (BMI) to supplement our set of self-reported health measures (Table B.4). We find no significant effect in measured high blood pressure (cols. 1-3), but almost a 10% reduction in self-reported diagnosed hypertension (cols. 4-6), which is concentrated among women. The difference in findings between the measured high blood pressure and diagnosed hypertension may be the result of low utilization of preventive care and the fact that a one-time measure of high blood pressure is not a diagnosis. For BMI (cols. 7-9), we find an increase of about 0.2 (0.8% increase at the mean) from exposure. We also estimate the probability of being overweight, defined as BMI above 25, and find that the results are driven by high BMI. This is consistent with the positive association between SES and BMI in many developing countries, including Indonesia (Sohn, 2017). This finding is in line with evidence from a mandatory schooling reform in Turkey (Dursun et al., 2018). We also analyze the effects of INPRES on some health behaviors (Table B.5). We find no significant effect on ever smoking, but we find a 4.5% increase in the probability of smoking at the time of the survey, which is consistent with the SES gradient of smoking in Indonesia (Triyana et al., 2019). We find no significant effect on the intensive margin of daily cigarette consumption. Additionally, we find an increase in alcohol expenditure among men. For women, we find no significant change in teen pregnancy. The increase in risky behavior among men may partly explain why there are no significant health benefits for men from INPRES.

health. Our results are consistent with Bharati, Chin and Jung (2016), who find that INPRES increased patience in adulthood, which is associated with better health investments (Fuchs, 1980). Therefore, changes in time preferences are a potential mechanism for our findings on long-term health.[35]

Magnitudes

The average number of INPRES schools built in a district was 2.1 per 1,000 children. This implies that going from no exposure to the average level of exposure increases the probability of being "healthy" or "very healthy" by about 12% of the mean. Similarly, we find a 11% reduction in reporting any chronic conditions, a 12% reduction in the number of reported chronic conditions and a 10% reduction in mental health symptoms. Our findings are in line with previous studies in high income countries that have used changes in compulsory schooling laws (CSL) to estimate the impacts of education on similar outcomes. For example, Mazumder (2008) and Oreopoulos (2007) find that an additional year of schooling due to CSL in the US, UK, and Canada, reduces the probability of being in fair or poor health by around 20% of the mean. CSL in the UK is associated with an increase in overall life satisfaction by about 6% of the mean (Oreopoulos, 2007). Education reforms have also led to reductions in depression of about 7% in Austria, Germany, Sweden, the Netherlands, Italy, France and Denmark (Crespo, López-Noval and Mira, 2014).

5. The Second Generation

5.1 Estimation Strategy

In this section, we examine the effects of parental exposure to INPRES on second-generation human capital outcomes. Specifically, we estimate the following equation:

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

where y_{itd} is the outcome of interest for child i whose mother/father was born in district d and

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

year t. The interaction $ParentExposed_t \times INPRES_d$ captures parental exposure to INPRES based on parental district and year of birth. X_i is a set of child characteristics: gender, birth order, and year and month of birth dummies. α_d and τ_t are parent's district and year-of-birth fixed effects. The rest of the variables are as defined in equation 1. Standard errors are clustered at the parent's district of birth level. As in the previous section, we consider the effects separately for the expanded sample and the restricted sample. The coefficient β captures the effect of parental exposure to one INPRES school built per 1,000 (first-generation) children.

Our empirical strategy relies on the assumption that parental INPRES program exposure is not correlated with unobserved characteristics that vary across districts over time that also may affect the second generation's outcomes. To test the parallel trends assumption, we estimate equation 2 for children born to pre-program cohorts and find no significant pre-trends in human capital outcomes among second generation born to parents not exposed to INPRES (Figure B.5). Additionally, 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 B.6). These results suggest similar pre-trends in the outcomes across districts among children born to adults not exposed to the INPRES program.

We start by estimating the models separately for the mother's and father's INPRES exposure. These estimates provide the reduced form effects of INPRES exposure of any one parent separately but does not account for the possibility that both parents could have been exposed. One possible concern with that specification is that the comparison group may be contaminated because the program could affect the marriage market (Akresh, Halim and Kleemans, 2019; Ashraf et al., 2020; Zha, 2019). For example, a father in the comparison group (older than primary school age at INPRES rollout) could marry a woman in the treatment group and thus be indirectly exposed to INPRES. Therefore, we present estimates from specifications where we include both maternal and

paternal exposure.[36]

5.2 Intergenerational effects

Health

We now analyze the intergenerational effects of INPRES on several measures of children's health. For each outcome we show three sets of results: mother's exposure only, father's exposure only and both parents' exposure. Our discussion focuses on the results for the expanded sample, which are shown in the top panels of the tables and figures. The results for the restricted sample are shown in the bottom panels.

We begin by analyzing the poor health index (Table 3 and Figure 3), which summarize the following outcomes: stunting, self-reported poor health, and anemia status. These outcomes are potentially observed multiple times in different waves of the IFLS when children are between the ages of 8 and 18.[37] Table 3 column 1 presents the effects of maternal exposure and shows that second-generation children born to mothers exposed to INPRES experience a decline in the poor health index by 0.03 standard deviations for each additional primary school constructed per 1,000 children. Table 3 column 2 finds that paternal exposure to INPRES also decreases the poor health index but the effects are smaller and statistically insignificant. Table 3 column 3, which includes both parents' exposure, corroborates our previous findings: second-generation children born to mothers exposed to INPRES experience a decline in the poor health index by 0.04 standard

[36] Including the full set of mother's and father's district of birth and year-of-birth fixed effects as well as the interactions between these districts of birth fixed effects and districts' covariates in the specifications for both parents' exposure is very demanding for our small sample sizes. Also, when estimating each parent separately, we find larger effects from maternal than paternal exposure to INPRES (see below). Therefore, we estimate models that include both the mother's and father's exposure and the full set of covariates for the mother. For the father's covariates, instead of father's district of birth and year-of-birth fixed effects, we use province of birth and two-year bins for year-of-birth fixed effects. Also, we include the interactions between the father's year of birth (in two-year bins) and the father's district-level covariates. Standard errors are clustered two-ways at the mother's and father's district of birth. In general, specifications using both parents are much more demanding on our data and lead to less precise estimates.

[37] We add wave fixed effects to these specifications.

deviations, while paternal exposure has smaller and insignificant effects.[38] We also examine the effects by the child's gender and find that the impacts are similar across sons and daughters (Table B.6).

Figure 4 shows an event study analysis (Panel A for maternal exposure and Panel B for paternal exposure) and plot the relationship between the second generation's poor health index against parental age at the time INPRES was implemented. To increase precision, we group parents into three-year age bins. The dots represent the point estimates of the coefficients of each parental age bin interacted with the number of INPRES schools, similar to equation 2. For mothers and fathers who were too old to be exposed to INPRES primary schools, the point estimates bounce around zero, providing evidence of similar pre-trends among non-exposed cohorts. In contrast, we see that children born to mothers exposed to the program at primary school age or younger exhibit a decline in their poor health index. Some of these estimated coefficients are noisy but they are negative and consistent with our earlier estimate using the difference-in-differences framework (shown on the far right). For fathers in the exposed cohorts, the effects are smaller and noisier. Taken together, our findings suggest that maternal exposure to INPRES improves health outcomes of children.

We next examine each of the components separately (Table 4). We begin with two measures that capture the cumulative effects of health investments: children's height-for-age (cols. 1-3) and stunting (cols. 4-6).[39] We find a 0.06 standard deviation increase in children's height-for-age z-score among those whose mothers were exposed to the program (col. 1 and col. 3). We find small and statistically insignificant effects among children whose fathers were exposed to the program (cols. 2 and 3). We also find a reduction in stunting rates among children born to INPRES-exposed mothers, which is consistent with the estimated increase in height-for-age among these children

[38] The use of multiple observations per individual may be a concern since a child in the IFLS may be between ages 8 and 18 in several waves of the survey. To address this, we estimate equation 2, using the average outcome across waves and restricting the sample to one observation per child, weighted by the number of observations per child. The estimated effect is similar to our earlier findings. Children whose mothers were exposed to INPRES have 0.03 standard deviations lower average poor health index (Table B.9).

[39] We did not analyze weight indicators as those are considered more short-term health measures rather than a long-term cumulative indicator such as height.

(cols. 4-6 of Table 4). These children are 2 percentage points less likely to be stunted, which corresponds to 7% of the mean. We find no significant effect through paternal exposure. Given Indonesia's high stunting rates and the adverse effects of stunting, this is a potentially important finding.

Turning to children's self-reported health status, we show that maternal exposure to INPRES increases the likelihood of being healthy by 1.2 percentage points (cols. 7-9). With respect to anemia, while the point estimates are negative for both mother's and parent's exposure, they are not statistically significant.

Overall, these results are consistent with earlier work that finds an effect on infant mortality through maternal but not paternal exposure to INPRES (Breierova and Duflo, 2004). To address multiple hypothesis testing, we test for the joint significance of the components of the index. We find that for maternal exposure, they are jointly significant in the expanded sample. We also adjust the standard errors for multiple hypothesis following Simes (1986) and most of our results are robust to this adjustment (Table B.8).

Education

We also examine whether parental exposure to INPRES resulted in improved educational outcomes in the next generation. Previous evidence documents that children born to women who were exposed to INPRES perform better on the national primary school examination (Mazumder, Rosales-Rueda and Triyana, 2019). We now extend the analysis to test scores on the national 9th grade examination. We find that children of mothers who were exposed to INPRES perform significantly better on the national secondary school examination (Table 5). On average, maternal exposure to one INPRES school (per 1,000 children) increases their children's secondary test scores by 0.08 standard deviations in the expanded sample (Panel A). The estimated effects are similar for sons and daughters. (Table B.7). Turning to fathers' INPRES exposure, the estimates are typically a little smaller and in no case are they statistically significant (cols. 4-6). These findings are also consistent with our event study analysis (Figure 4).

We also examine children's lower secondary school completion (up to 9th grade). Here we find small and statistically insignificant effects of maternal and paternal INPRES exposure (Table B.10, cols. 1 and 2 respectively).[40] The estimated effects of maternal and paternal exposure are similarly small and not significant. Since the majority of the second-generation individuals in our sample are subject to Indonesia's compulsory schooling laws, this could explain why parental education has no effect on school completion, but does appear to affect test scores.[41]

Magnitudes

We now discuss the effect sizes for our intergenerational estimates. Under INPRES, an average of 2.1 schools were built per 1,000 in each district, which implies that, on average, maternal exposure to INPRES raises height-for-age z-score (HAZ) by 0.13 SDs and reduces stunting by 5 percentage points (14% of the mean). Ideally, we would like to compare these effect sizes to the impacts of other developing-country interventions on similar intergenerational outcomes. However, we are unaware of any comparable studies. Therefore, we compare our effect sizes to those of other interventions on individuals *directly exposed in the first generation*. Fernald, Gertler and Neufeld (2008) find that a doubling of Mexico's conditional cash transfers (CCT) program led to a 0.16 SDs increase in HAZ and reduced stunting by 9% among children ages 10-14. Similarly, Barham (2012) finds that exposure to the Matlab Maternal and Child Health and Family Planning Program in Bangladesh increases HAZ by 0.2 SDs. Nores and Barnett (2012) summarize the impacts of early childhood interventions in developing countries and report that average effect sizes on long-term child health outcomes (ages 7 or above) are around 0.12 SDs.

At the average level of exposure, maternal INPRES exposure increases secondary test scores between 0.17 and 0.22 SDs. In comparison, Baird, McIntosh and Özler (2011) and Baird et al. (2014) find that 2 years of exposure to a CCT program in Malawi increased English and math

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

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

test scores of 13-22 year old women by between 0.12-0.16 SDs. Similarly, Barham, Macours and Maluccio (2013) find that a Nicaraguan CCT program in primary school increases boys' test scores by 0.2 SDs ten years later. Friedman et al. (2016) examine the effects of a merit-based scholarship program in rural Kenya that targeted girls in grade 6 and find that their test scores increase by 0.2 SDs in grades 10-11. Taken together, the effects of INPRES on second-generation test scores are comparable to the effect sizes of interventions like CCTs. Overall, the magnitudes of our findings on the second generation are similar to estimates from studies that evaluate other social interventions in developing countries.

6. Robustness

Alternative exposure variable

Following Duflo (2001), our main specifications use the total number of INPRES primary schools per 1,000 children in the district of birth that were built between 1973-74 and 1978-79 as a measure of exposure. This assumes that all individuals are exposed to the complete "stock" of schools at the end of the program. As a robustness check, we create an alternative measure of the number of schools constructed during an individual's primary school years (ages 6 to 11) in their birth district. Table B.11 illustrates that the estimated effects are similar in magnitude and statistical significance to those from our main exposure measure.

Alternative sample

In addition, following Duflo (2001), our sample uses men and women born between 1950 and 1972 (and their offspring), which combines non-exposed, partially exposed, and fully exposed individuals. 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 more children in the second-generation. Results using this alternative sample are remarkably similar to our main sample estimates (Table B.12).

7. Potential mechanisms

In this section, we explore some of the possible mechanisms for our key findings. This is challenging, both because there are a multitude of hypothesized channels and because there are important data limitations. For example, ideally, we would like to explore the extent to which parents who were exposed to INPRES chose to invest more in their children's human capital through investments such as breastfeeding or parenting practices. Unfortunately, we lack data on these kinds of parental investments. Nevertheless, there are several important channels that we investigate using the rich IFLS data. For each potential channel, we first present and discuss the effect of the program on that channel. Second, when relevant, we perform some simple calculations to assess the relative contribution of each mechanism in explaining the INPRES effects on the first and the second generation.

In order to assess the potential contribution of each mechanism, we rely on relatively simple "back of the envelope" calculations. We do this by combining: i) estimated associations between each mechanism and our outcome of interest in the *comparison* cohorts;[42] ii) our estimated effects of INPRES on each mechanism. We then compare the implied effects from this exercise to our estimates of INPRES effects on the outcome of interest (See Table B.19). We highlight two important limitations of this analysis. First, for part i), the associations we use are purely observational in nature as we lack a strong research design for causal inference.[43] Second, our calculations only provide reduced-form estimates that may capture many other factors associated with the channel in question.

Marriage

Previous work has documented positive assortative mating on education in the marriage market (Anukriti and Dasgupta, 2017; Behrman and Rosenzweig, 2002; Hahn et al., 2018a). We explore the impacts of INPRES on the spousal characteristics of the first-generation men and

[42] 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.
[43] If there is omitted variable bias, it is possible we may be overstating the effect of each mechanism.

women (Table 6) and find some evidence of improved marital outcomes for women exposed to the program, but not for exposed men (Table 6, Panel B). Specifically, women fully exposed to the program are more likely to marry more highly educated men. However, the estimates are noisier in the expanded sample.[44]

Our back-of-the envelope calculations suggest that the impact of INPRES on husband's education may explain up to 2.5% of the effect on women's self-reported health (Table B.19). For marriage, we limit our investigation of the contribution of this channel to first-generation outcomes only, because we find that second-generation outcomes are causally influenced, mainly by the mother's and not the father's education, so it would not make sense in the context of our particular analysis to consider marital sorting.[45]

Fertility

A large literature has established that improving women's education increases the opportunity cost of having children. As a result, women may delay childbearing and have fewer children due to the trade-off between child quantity and quality (Becker and Lewis, 1973; Osili and Long, 2008).[46] In addition, better-educated women may make better fertility choices through greater knowledge and more effective use of contraceptives (Kim, 2010; Rosenzweig and Schultz, 1989).[47] We investigate women's fertility responses to INPRES exposure in Table 7. We begin by examining whether women delayed their first pregnancy and find a small negative effect that is statistically insignificant (Panel A, col. 1). Next, we examine birth spacing between the first and second child and find no significant effect (Panel A, col. 3). Finally, since our sample of first-

[44] We also examine the gender gap between spousal education but did not find significant effects. In addition, we explored bargaining power as a potential mechanism. The IFLS collects information about household decision-making related to family expenditures, children's wellbeing, use of contraception, etc. We did not find evidence that INPRES improved women's bargaining power.

[45] For the second generation, our back-of-the envelope calculations suggest that the mother's assortative mating channel may explain up to 6% of the maternal exposure to INPRES impacts on children's human capital.

[46] See for example Aaronson, Lange and Mazumder (2014) and Hahn, Nuzhat and Yang (2018).

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

generation women have likely completed their fertility by 2012-2014, column 5 presents the effects on total number of children ever born. Although the estimated impact is negative, suggesting a decline in the number of children, it is noisy, which may be due to the small sample of the IFLS. Therefore, we replicate our analysis in a larger, nationally representative sample using the 2014 Susenas which has the total number of births. Panel B and Panel C present the results from this analysis for all the provinces in Indonesia, and for the provinces surveyed in the IFLS and IFLS-E, respectively. We find that exposure to one INPRES school per 1,000 children reduced the number of live births by around 0.05. The point estimates from the Susenas data are qualitatively similar to those from the IFLS data but are more precise and statistically significant due to the larger sample size. Breierova and Duflo (2004) find qualitatively similar effects to ours, but they use data from 1995 when treated cohorts may not have completed their fertility. The magnitude of the INPRES effects on total fertility are similar to studies of primary school reforms in Nigeria (Osili and Long, 2008) and Uganda (Keats, 2018).[48]

One concern is whether the INPRES effects on intergenerational outcomes may be explained by fertility responses and the quantity-quality trade-off. Our back-of-the envelope calculations indicate that the reduction in total fertility might explain between 2% and 8% of the second-generation impacts on health outcomes and test scores (Table B.19). This suggests that although that women exposed to INPRES have fewer children, this does not appear to be driving our second-generation findings.[49]

[48] For example, exploiting the introduction of universal primary schooling in Nigeria, Osili and Long (2008) find that increasing female education by one year decreases early fertility by 0.26 births. Similarly, using the elimination of primary school fees in Uganda, Keats (2018) documented that an extra year of schooling decreases early fertility by 0.36 births. Considering the INPRES effect on years of schooling documented by Breierova and Duflo (2004), our reduced-form estimate of number of births is similar in magnitude to the findings from these studies. find that exposure to INPRES increases years of schooling by 0.2 years for women in the restricted sample. We replicate similar but noisy effects in the appendix of our companion paper (Mazumder, Rosales-Rueda and Triyana , 2019). Putting together these INPRES impacts on years of schooling and Osili and Long (2008)'s estimate of the effect of one year of female schooling on total births would imply a fertility effect of INPRES of -0.05 births. Similarly, Keats (2018) estimated effects would imply a fertility effect of -0.07 births. Our estimated effect of INPRES (-0.05) is similar to those calculations.

[49] Even if we consider a larger association between total fertility and children outcomes (estimate + 2SDs), fertility responses might explain between 3% and 10% of the intergenerational effects.

Household resources

Another channel we consider is whether the greater access to education due to INPRES led individuals to accumulate more resources and make more productive investments in health (Grossman, 1972).[50] To measure household resources, we use data on per capita consumption, which is a widely used proxy for household income and well-being.[51]

Specifically, we use the IFLS-5 (2014) and IFLS-E (2012) to measure log per capita expenditure (in 2012 Rupiah), which is based on weekly or monthly per capita food and non-food expenditure.[52] We also use data from the same survey years to construct a housing quality index as a proxy for the household environment. The index combines poor housing characteristics (poor-quality toilets, floors, roofs, and walls) and an indicator for high occupancy per room.[53] Higher values of the index reflect poorer housing quality. We also construct an asset ownership index which includes: savings, vehicle, land, TV, appliances, refrigerator, and house.

We find that exposure to INPRES increases household resources (Table 8). Specifically, we show that each additional INPRES school per 1,000 children leads to approximately 5% higher consumption (col. 1), a lower index of poor housing quality of about 0.03 to 0.04 SDs (col. 2), and an imprecisely estimated increase in the asset index by about 0.02 SDs (col. 3).[54] We generally find stronger effects for women who were fully exposed to the program (Table 9, cols. 1-6).

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

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

[52] We exclude annual non-food expenditure, which includes items such as land and vehicle purchases. Table B.13 presents the components separately.

[53] 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 item by subtracting the mean and dividing by the standard deviation of the comparison group, and create an index that is the average of the standardized outcomes.

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

We find that household resources only explain between 2% and 3% of the effect of INPRES on the long-term health index. For the second-generation outcomes, this channel can explain between 6% and 22% of the intergenerational impacts on test scores and health outcomes (Table B.19).

Migration

We also consider migration as a mechanism. INPRES may have led individuals to move to either better or worse areas. For example, in the context of schools built for rural blacks in the American South in the early 20th century, Aaronson et al. (2020) find that a failure to separately account for the effects on migration to areas where blacks suffered higher mortality can obscure the health-promoting effects of education. To address this, we examine whether individuals exposed to INPRES are more or less likely to migrate out of their place of birth than those not exposed to the program. We explore several indicators of migration below.

First generation:

We analyze migration in the first generation using two indicators (Table 10, Panel A). First, we create an indicator that takes the value one if the district of birth is different from the current district of residence in any of the waves of the IFLS. Second, we create a similarly coded variable that only compares the district of birth to the district of residence in 2012 (IFLS-E) or 2014 (IFLS-5), which are the waves we use to measure our health outcomes of interest for the first generation.[55] We find that exposure to INPRES has no significant or sizable effect on either of these indicators of migration. As an additional check, we estimate our long-term effects on the sample of non-movers and find similar estimates (Table B.16).

[55] The first measure is broader in that it accounts for some individuals who might have left their district of birth but then returned, and individuals who were no longer in the sample in 2014. The second measure is directly comparable to our estimation sample. Due to the small sample size of the IFLS, we replicate our analysis using the 2014 Susenas and find qualitatively similar results (Table A.5).

Second generation:

We further explore migration by considering the possibility of parents (first generation) migrating either before or after the birth of their child (second generation). 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 (Table 10, Panel B). 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 B.16).

Neighborhood quality

Another potential channel through which schooling can impact long-term and intergenerational outcomes is through human capital externalities. One prominent example is that education could lead to higher levels of political participation (Wantchekon, Klašnja and Novta, 2015), which could in turn lead to policies that improve socioeconomic outcomes. Indeed, Martinez-Bravo (2017) found that INPRES increased the level of education of potential candidates to local leadership positions, which then improved local governance and the provision of public goods. This finding suggests that we ought to examine the role of neighborhood quality as a potential mechanism behind our results. There is also strong evidence from the U.S. and Australia that neighborhoods impact human capital development (Chetty and Hendren, 2018; Deutscher, 2019).

To investigate the potential role of neighborhoods, we use the IFLS community survey (IFLS-5 in 2014 and IFLS-E in 2012) to create indices of education and health service provision at the community level (village or township in rural and urban areas respectively). 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*).[56] The poverty index captures both poverty and access to anti-poverty programs.

In principle, educational services at the local level could explain the second generation's improved educational outcomes, while access to health services could improve health outcomes in either generation. One caveat is that we are only able to observe neighborhood quality for non-movers in IFLS-5 (in 2014 with respect to IFLS-1 in 1993).[57] Since migration does not appear to drive our estimated effects, selection concerns are minimized. 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.[58] Similarly, the health services index includes: an indicator for a majority of residents having piped water, a similar indicator for a private toilet, the number of community health centers, and the number of available midwives.

We find no statistically significant effect on neighborhood access to education facilities (Table 8, col. 4).[59] In contrast, we find that individuals who were exposed to INPRES have better access to health services in their communities, and this effect is statistically significant for those fully exposed to INPRES in the restricted sample (Panel B, col. 5). In addition, this effect seems to be concentrated among women (Table 9, cols. 7-12). Similarly, we find that women in the restricted sample tend to reside in communities with lower poverty (by 0.069 standard deviations, col. 12). These results are in line with the INPRES program improving public good provision.

When we examine the contribution of community health resources on the first generation's long-term health, our back-of-the envelope calculations suggest that the INPRES impact on this channel may explain between 1% and 3% of the effect on self-reported health (Table B.19). For

[56] *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.
[57] The main IFLS collects information on the original 312 communities sampled in IFLS-1 in each wave of the survey. Therefore, neighborhood quality measures are only available for individuals who resided in one of the original 312 communities in 2014.
[58] 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.
[59] 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.

the second generation, exposure to a neighborhood with better health facilities can explain between 3% and 10% of the program effect on the second generation's health index and height for age.

Other mechanisms

These results suggest that a significant portion of the first generation's long-term health impacts and intergenerational human capital effects cannot be attributed to the mechanisms that we are able to measure. We cautiously interpret these findings as suggesting that the direct effects of INPRES on the educational attainment of individuals in the first generation are likely to be the main explanation for the gains observed in the second generation. Nevertheless, an important caveat for our analysis is that there may be many other interesting and important potential mechanisms at play that we simply cannot measure. These include factors such as knowledge acquisition, family human capital investments, and maternal mental health.[60]

8. Discussion and Conclusion

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

We also find striking evidence of intergenerational spillovers. Children of mothers exposed to INPRES are less likely to be stunted, have better self-reported health, and have significantly

[60] There is growing causal evidence on the impacts of maternal mental health, especially during pregnancy, on children's human capital and long-term outcomes (Black et al., 2016; Persson and Rossin-Slater, 2018). Additionally, in LMICs, maternal mental health has been identified as an important predictor of child development (Walker et al., 2011). We were unable to calculate associations between maternal mental health and children's human capital outcomes for the comparison group because the children of the comparison group are mainly observed in the earlier waves of the IFLS, where adult mental health was not measured. A similar issue also arises for children's birth weight and vaccination. This information is only captured for children born 5 years prior to the survey. Thus, those variables are more likely to be missed for children born to comparison mothers.

higher test scores. We find no statistically significant effects through paternal exposure and the point estimates are either similar to, or smaller, than those from maternal exposure. Our findings are not driven by migration responses and are robust to alternative specifications. With respect to the role of fertility responses, while we find that women exposed to the program experience a decline in their total fertility, our "back-of-the envelope" calculations imply that this channel only explains up to 8 percent of our second-generation effects. Additionally, we present some evidence that greater household resources and better neighborhood quality are potential mechanisms for our intergenerational findings. Our findings point to one important way in which policy makers can influence human capital outcomes, even into the subsequent generation.

How important are these spillovers and do they justify large-scale expenditures on school construction programs such as INPRES? We directly address this question through a cost benefit analysis. Specifically, we calculate the real internal rate of return of the program, both with and without taking into account the spillover effects. We make several simplifying assumptions. For example, we make the conservative assumption that the INPRES schools were operational for twenty years and phased out by 1997.[61] For benefits, we include the earnings and health gains for each generation. We use the results from existing studies to translate the magnitude of the health and educational improvements we observe in the second generation on lifetime earnings. The complete details are available in Appendix C.

We estimate that the internal rate of return of the INPRES program is about 7.9 percent when only including the returns to primary school completion for the first generation. This is similar both to what Duflo (2001) finds for INPRES as well as what Aaronson and Mazumder (2011) find for the Rosenwald school construction program that took place in the rural American South early in the 20th century. When we include the long-term health improvements in the first generation, the estimated internal rate of return rises to about 8.8 percent.[62] When we further account for the intergenerational benefits (health and test scores), we find a vastly higher rate of

[61] Thus, cohorts born between 1963 and 1989 received benefits from the program.
[62] Akresh, Halim and Kleemans (2019) estimate the internal rate of return based on the first generation's taxes and improved living standards. They find a rate of return between 10.5% and 20.7%.

return of as much as 24.8 percent. This suggests that traditional cost-benefit analyses of this type of intervention that only take into account the returns to education may significantly underestimate the societal benefits. These findings have highly salient policy implications for countries that are still struggling with access to basic education and are contemplating large-scale schooling interventions. Our results strongly suggest that these nations should take into account the potential spillover gains to health and to subsequent generations.

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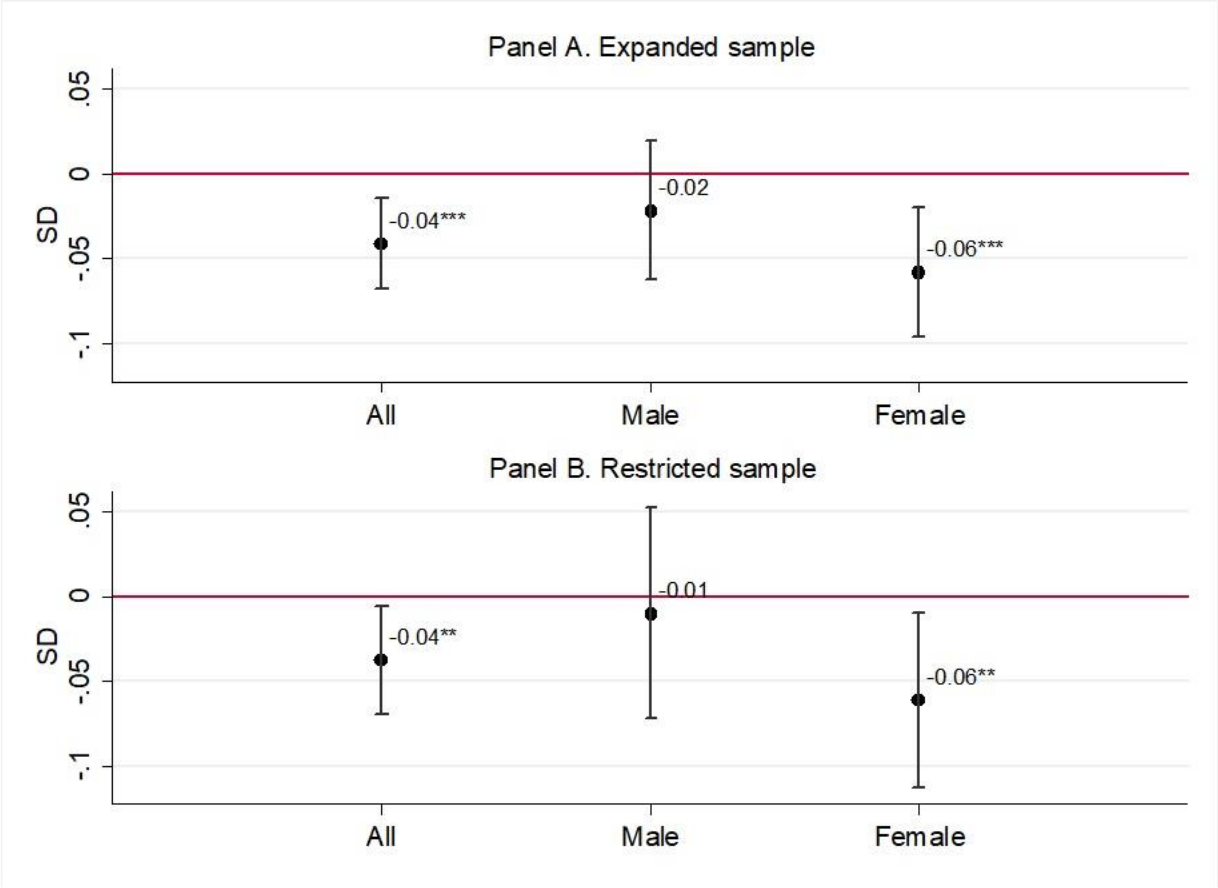
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Tables and Figures

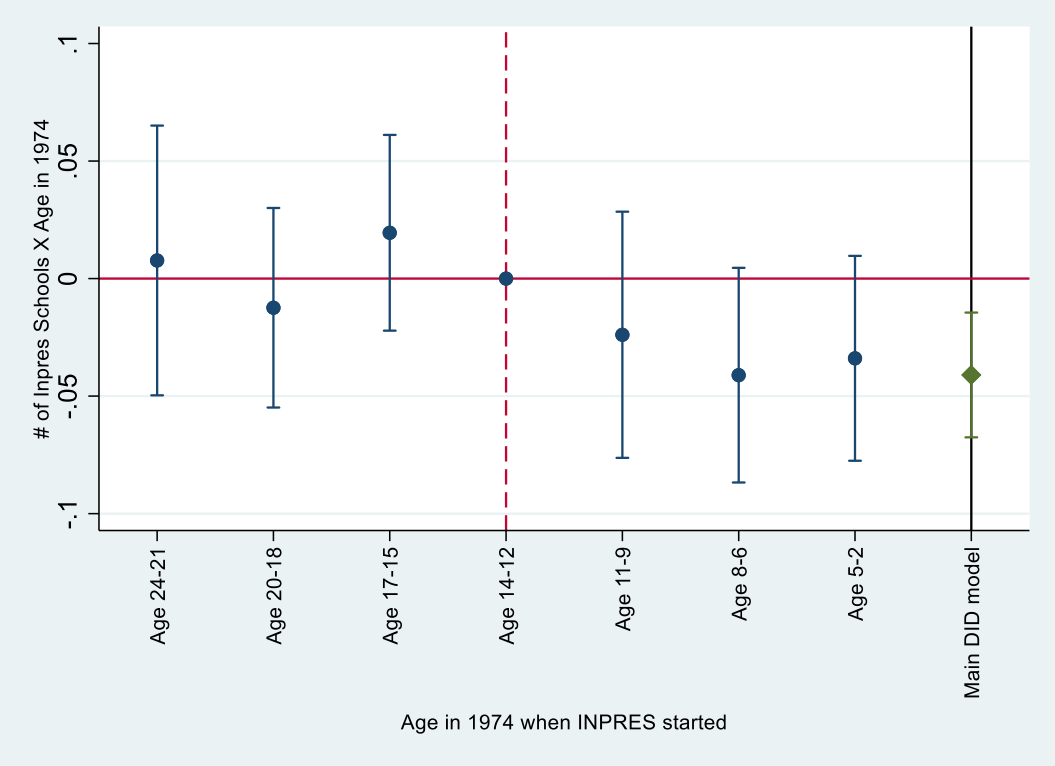
First generation estimations

Figure 1: Effect on the first generation: poor health index



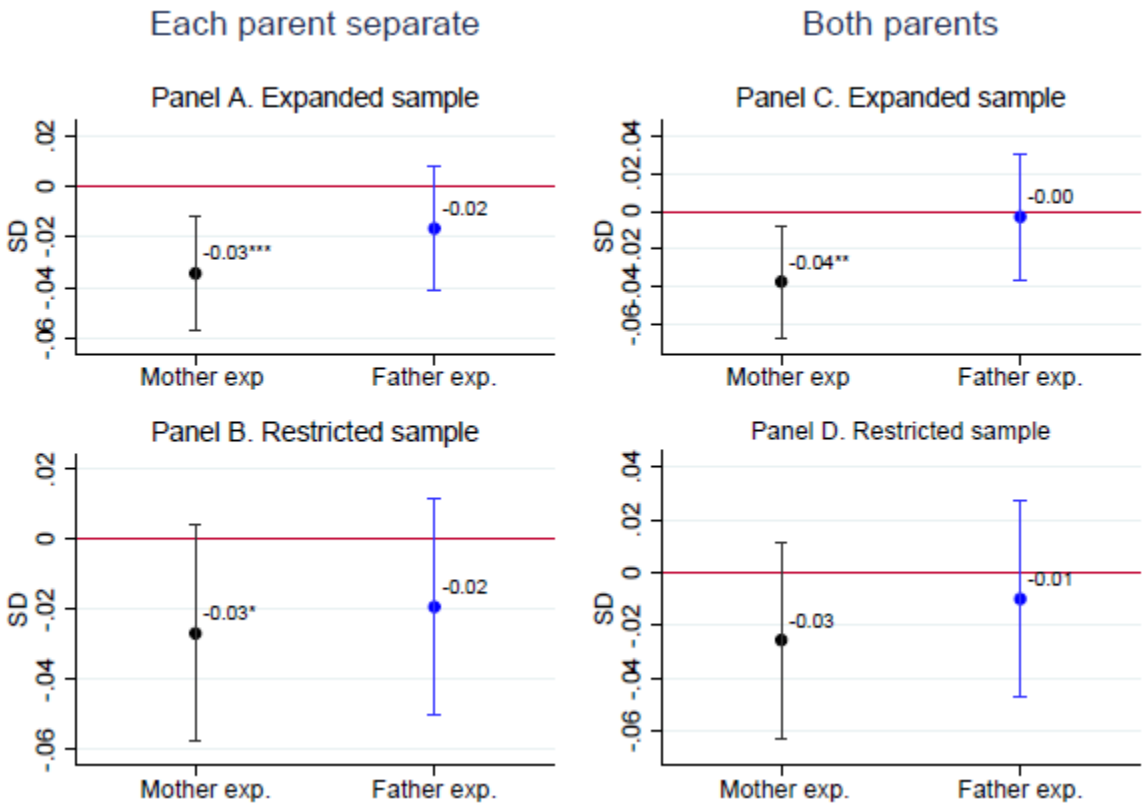
Notes: Overall poor health index corresponds to a summary index from the multiple self-reported health measures analyzed: self-reported poor general health, number of days missed, any chronic conditions, number of conditions, and mental health screening score where higher scores correspond to more symptoms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

Figure 2: Effect on the first generation: poor health index event study



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

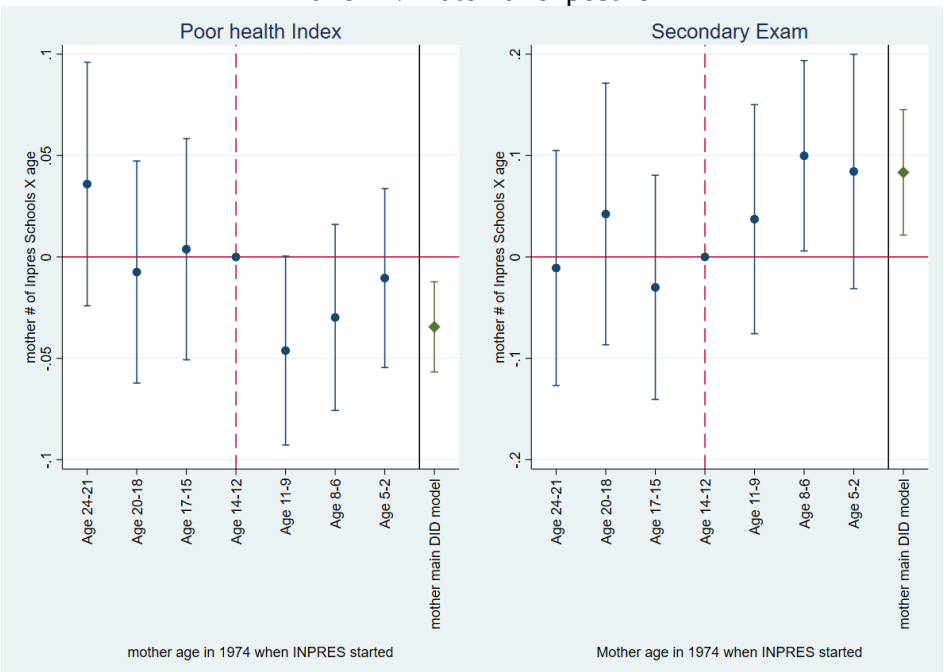
Figure 3: Intergenerational effects: poor health index



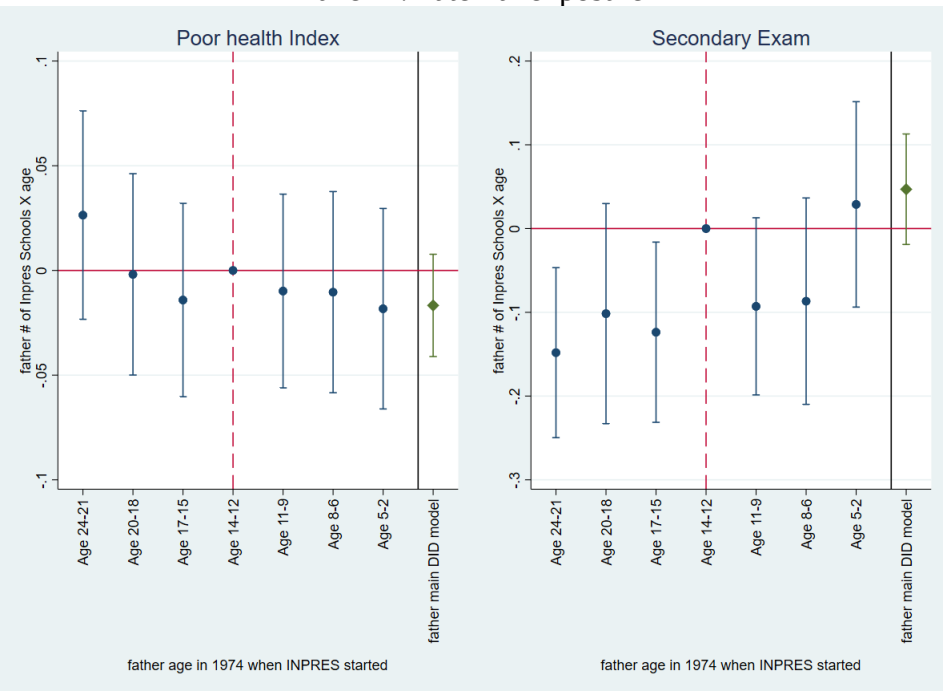
Notes: Overall poor health index corresponds to a summary index from the following health measures for the second generation: being stunted, anemic and self-reported poor health. Sample corresponds to children born to first generation INPRES individuals. Expanded sample includes children born to adults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

Figure 4: Intergenerational outcome: effect on poor health index – Event study

Panel A. Maternal exposure



Panel B. Paternal exposure



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

Table 1: Program effect on the first generation's health outcomes

Panel A: Expanded Sample						
	(1)	(2)	(3)	(4)	(5)	(6)
	Poor health index	Self-reported healthy	Days missed	Any chronic condition	No. of conditions	Mental health
Born bet. 1963-1972	-0.041***	0.039***	-0.171**	-0.025**	-0.047**	-0.180
× INPRES	(0.013)	(0.010)	(0.068)	(0.011)	(0.019)	(0.141)
No. of obs.	9891	10792	10420	10729	10729	10244
Dep. var. mean		0.677	2.010	0.482	0.833	5.402
R-squared	0.10	0.08	0.06	0.09	0.10	0.09
Joint test p-value			0.0003			

Panel B. Restricted sample						
	(1)	(2)	(3)	(4)	(5)	(6)
	Poor health index	Self-reported healthy	Days missed	Any chronic condition	No. of conditions	Mental health
Born bet. 1968-1972	-0.038**	0.033***	-0.079	-0.014	-0.037	-0.271*
× INPRES	(0.016)	(0.012)	(0.102)	(0.017)	(0.024)	(0.153)
No. of obs.	5537	5999	5826	5940	5940	5741
Dep. var. mean		0.705	1.887	0.456	0.772	5.378
R-squared	0.12	0.09	0.07	0.10	0.11	0.12
Joint test p-value			0.045			

Notes: Overall poor health index corresponds to a summary index from the multiple self-reported health measures analyzed: self-reported poor general health, number of days missed, any chronic conditions, number of conditions, and mental health screening score where higher scores correspond to more symptoms of depression. The index has mean 0, SD 1 based on those born between 1950-1962 in low INPRES areas. Healthy takes the value one if a respondent reports being "Very healthy" or "Healthy". "Days missed" corresponds to the number of days a respondent missed his or her activities due to health reasons in the past 4 weeks prior to the survey. Any chronic condition and the number of chronic conditions come from self-reported chronic conditions. Mental health score is based on the following items: being bothered by things, having trouble concentrating, feeling depressed, feeling like everything was an effort, feeling hopeful about the future, feeling fearful, having restless sleep, feeling happy, lonely, and unable to get going. Higher scores correspond to poorer mental health. Expanded sample includes those born between 1950-1972. Restricted sample includes those born between 1957-1962 or 1968-1972. Covariates include: district, year of birth, month of birth, ethnicity (Javanese dummy), birth-year interacted with: the number of school-aged children in the district in 1971 (before the start of the program), the enrollment rate of the district in 1971 and the exposure of the district to another INPRES program: a water and sanitation program. Robust standard errors clustered at district of birth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 2: Program effect on the first generation's health outcomes by gender

Panel A. Expanded sample												
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Poor health index		Self-reported healthy		Days missed		Any chronic condition		No. of conditions		Mental health	
	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female
Born bet. 1963-1972	-0.021	-0.058***	0.017	0.062***	-0.116	-0.205*	-0.015	-0.039***	-0.030	-0.075**	-0.109	-0.235
× INPRES	(0.021)	(0.019)	(0.013)	(0.014)	(0.086)	(0.112)	(0.020)	(0.014)	(0.026)	(0.034)	(0.188)	(0.238)
No. of obs.	4817	5074	5296	5496	5140	5280	5266	5463	5266	5463	4962	5282
Dep. var. mean			0.709	0.647	1.717	2.284	0.402	0.556	0.680	0.976	5.129	5.653
R-squared	0.12	0.11	0.13	0.10	0.09	0.08	0.10	0.10	0.12	0.11	0.12	0.12
Gender diff. p-val	0.176		0.008		0.517		0.365		0.267		0.670	
Panel B. Restricted sample												
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Poor health index		Self-reported healthy		Days missed		Any chronic condition		No. of conditions		Mental health	
	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female
Born bet. 1968-1972	-0.010	-0.061**	0.006	0.060***	-0.025	-0.102	0.003	-0.032*	-0.009	-0.072*	0.015	-0.520**
× INPRES	(0.032)	(0.026)	(0.019)	(0.017)	(0.125)	(0.179)	(0.031)	(0.018)	(0.040)	(0.040)	(0.281)	(0.214)
No. of obs.	2668	2869	2930	3069	2861	2965	2900	3040	2900	3040	2751	2990
Dep. var. mean			0.742	0.673	1.596	2.151	0.361	0.541	0.578	0.945	5.058	5.655
R-squared	0.15	0.14	0.14	0.12	0.12	0.12	0.13	0.13	0.14	0.14	0.16	0.15
Gender diff. p-val	0.089		0.020		0.733		0.300		0.287		0.137	

Notes: Overall poor health index corresponds to a summary index from the multiple self-reported health measures analyzed: self-reported poor general health, number of days missed, any chronic conditions, number of conditions, and mental health screening score where higher scores correspond to more symptoms of depression. The index has mean 0, SD 1 based on those born between 1950-1962 in low INPRES areas. The p-value of the joint test for items in the health index for men in Panel A is 0.545, and in Panel B is 0.992. The p-value of the joint test for items in the health index for women in Panel A is <0.001, and in Panel B is 0.002. Healthy takes the value one if a respondent reports being “Very healthy” or “Healthy”. 'Days missed' corresponds to the number of days a respondent missed his or her activities due to health reasons in the past 4 weeks prior to the survey. Any chronic condition and the number of chronic conditions come from self-reported chronic conditions. Mental health score is based on the following items: being bothered by things, having trouble concentrating, feeling depressed, feeling like everything was an effort, feeling hopeful about the future, feeling fearful, having restless sleep, feeling happy, lonely, and unable to get going. Higher scores correspond to poorer mental health. Expanded sample includes those born between 1950-1972. Restricted sample includes those born between 1957-1962 or 1968-1972. Covariates include: district, year of birth, month of birth, ethnicity (Javanese dummy), birth-year interacted with: the number of school-aged children in the district in 1971 (before the start of the program), the enrollment rate of the district in 1971 and the exposure of the district to another INPRES program: a water and sanitation program. Robust standard errors clustered at district of birth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Second generation estimations

Table 3: Program effects on intergenerational outcomes: poor health index

Panel A. Expanded sample			
	(1) Mother only	(2) Father only	(3) Both parents
Mother bet. 1963-72	-0.034***		-0.037**
× INPRES	(0.011)		(0.015)
Father bet. 1963-72		-0.017	-0.003
× INPRES		(0.012)	(0.017)
No. of obs.	17919	17384	19042
R-squared	0.17	0.15	0.17
Panel B. Restricted sample			
	(1) Mother only	(2) Father only	(3) Both parents
Mother bet. 1968-72 × INPRES	-0.027*		-0.026
	(0.016)		(0.019)
Father bet. 1968-72 × INPRES		-0.019	-0.010
		(0.016)	(0.019)
No. of obs.	9911	9007	13707
R-squared	0.17	0.15	0.17

Notes: Overall poor health index corresponds to a summary index from the following health measures for the second generation: being stunted, anemic and self-reported poor health. Sample corresponds to children born to first generation INPRES individuals. Expanded sample includes children born to adults born between 1950-1972. Restricted sample includes children born to adults born between 1957-1962 or 1968-1972. For column 1 and 2, covariates include the following FE: parent year of birth × 1971 enrollment, parent year of birth × 1971 number of children, parent year of birth × water sanitation program, child's gender, birth order, year and month of birth dummies, urban, and ethnicity (Javanese dummy). Robust standard errors in parentheses clustered at the parent's district of birth. For column 3 (both parents), the sample corresponds to children either born mothers or fathers in the first generation sample. These models include mother's and father's exposure and the full set of covariates for the mother, while for the father we include: province of birth, two-year bins for year of birth fixed effects and interactions between the father's year of birth (in two-year bins) and the father's district-level covariates. In this estimation, standard errors are clustered two-way at the mother and father district of birth.
Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 4: Program effects on intergenerational outcomes: Health measures

Panel A. Expanded sample												
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Height-for-age			Stunting			Self-reported healthy			Anemia		
	Mother only	Father only	Both parents	Mother only	Father only	Both parents	Mother only	Father only	Both parents	Mother only	Father only	Both parents
Mother bet. 1963-72	0.056*		0.065*	-0.025*		-0.023	0.012**		0.013*	-0.010		-0.013
× INPRES	(0.030)		(0.033)	(0.013)		(0.016)	(0.005)		(0.007)	(0.011)		(0.013)
Father bet. 1963-72		-0.016	-0.044		-0.009	0.002		0.004	-0.001		-0.009	-0.007
× INPRES		(0.029)	(0.040)		(0.012)	(0.016)		(0.005)	(0.007)		(0.010)	(0.011)
No. of obs.	21382	19890	22224	21382	19890	22224	18648	18171	19852	18104	17535	19217
Dep. var. mean	-1.648	-1.627	-1.662	0.365	0.360	0.370	0.833	0.870	0.831	0.256	0.247	0.250
R-squared	0.16	0.17	0.18	0.12	0.13	0.13	0.31	0.25	0.29	0.07	0.07	0.07
Mother joint test p-val	0.01											
Father joint test p-val		0.16										

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. For the each parent models, covariates include the following FE: parent year of birth × 1971 enrollment, parent year of birth × 1971 number of children, parent year of birth × water sanitation program, child's gender, birth order, year and month of birth dummies, urban, and ethnicity (Javanese dummy). Robust standard errors in parentheses clustered at the parent's district of birth. For the both parents models, the sample corresponds to children either born mothers or fathers in the first generation sample. These models include mother's and father's exposure and the full set of covariates for the mother, while for the father we include: province of birth, two-year bins for year of birth fixed effects and interactions between the father's year of birth (in two-year bins) and the father's district-level covariates. In this estimation, standard errors are clustered two-way at the mother and father district of birth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 4: Program effects on intergenerational outcomes: Health measures

Panel B. Restricted sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Height-for-age			Stunting			Self-reported healthy			Anemia		
	Mother only	Father only	Both parents	Mother only	Father only	Both parents	Mother only	Father only	Both parents	Mother only	Father only	Both parents
Mother bet. 1968-72	0.044		0.046	-0.022		-0.008	0.005		0.007	-0.011		-0.019
× INPRES	(0.046)		(0.046)	(0.020)		(0.021)	(0.006)		(0.008)	(0.014)		(0.016)
Father bet. 1968-72		-0.055	-0.035		-0.007	-0.013		-0.005	-0.004		-0.018	-0.009
× INPRES		(0.049)	(0.049)		(0.017)	(0.019)		(0.006)	(0.008)		(0.012)	(0.012)
No. of obs.	11464	10007	15562	11464	10007	15562	10319	9423	14303	9994	9084	13821
Dep. var. mean	-1.644	-1.641	-1.651	0.368	0.365	0.370	0.853	0.898	0.852	0.250	0.244	0.242
R-squared	0.19	0.20	0.20	0.14	0.15	0.14	0.29	0.19	0.26	0.09	0.09	0.09
Mother joint test p-val	0.45											
Father joint test p-val		0.14										

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. For the each parent models, covariates include the following FE: parent year of birth × 1971 enrollment, parent year of birth × 1971 number of children, parent year of birth × water sanitation program, child's gender, birth order, year and month of birth dummies, urban, and ethnicity (Javanese dummy). Robust standard errors in parentheses clustered at the parent's district of birth. For the both parents models, the sample corresponds to children either born mothers or fathers in the first generation sample. These models include mother's and father's exposure and the full set of covariates for the mother, while for the father we include: province of birth, two-year bins for year of birth fixed effects and interactions between the father's year of birth (in two-year bins) and the father's district-level covariates. In this estimation, standard errors are clustered two-way at the mother and father district of birth.
Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 5: Program effects on intergenerational outcomes: National secondary test scores

Panel A. Expanded sample			
	(1) Mother only	(2) Father only	(3) Both parents
Mother bet. 1963-72 × INPRES	0.083***		0.110***
	(0.031)		(0.039)
Father bet. 1963-72 × INPRES		0.047	0.006
		(0.033)	(0.043)
No. of obs.	6819	5744	6604
R-squared	0.11	0.11	0.14
Panel B. Restricted sample			
	(1) Mother only	(2) Father only	(3) Both parents
Mother bet. 1968-72 × INPRES	0.105*		0.112**
	(0.059)		(0.057)
Father bet. 1968-72 × INPRES		0.103	0.040
		(0.065)	(0.056)
No. of obs.	3512	2639	4361
R-squared	0.14	0.17	0.17

Notes: Ninth grade test scores standardized for each exam year. Sample corresponds to children born to first generation INPRES individuals. Expanded sample includes children born to adults born between 1950-1972. Restricted sample includes children born to adults born between 1957-1962 or 1968-1972. For column 1 and 2, covariates include the following FE: parent year of birth × 1971 enrollment, parent year of birth × 1971 number of children, parent year of birth × water sanitation program, child's gender, birth order, year and month of birth dummies, urban, and ethnicity (Javanese dummy). Robust standard errors in parentheses clustered at the parent's district of birth. For column 3 (both parents), the sample corresponds to children either born mothers or fathers in the first generation sample. These models include mother's and father's exposure and the full set of covariates for the mother, while for the father we include: birth province, year of birth, interactions between year of birth (in two-year bins) and the 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. In this estimation, standard errors are clustered two-way at the mother and father district of birth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Potential mechanisms

Table 6: Potential mechanisms: First generation's marriage outcomes

Panel A. Expanded sample						
	(1)	(2)	(3)	(4)	(5)	(6)
	Spouse with primary completion		Spouse with secondary completion		Age difference Respondent - spouse	
	Men	Women	Men	Women	Men	Women
Born bet. 1963-72	0.007	0.006	-0.025	0.018	0.079	0.197
× INPRES	(0.013)	(0.013)	(0.016)	(0.016)	(0.153)	(0.271)
No. of obs.	5989	5884	5989	5884	5999	5443
Dep. var. mean	0.80	0.78	0.47	0.48	-5.14	4.93
R-squared	0.22	0.19	0.26	0.24	0.09	0.11
Panel B. Restricted sample						
	(1)	(2)	(3)	(4)	(5)	(6)
	Spouse with primary completion		Spouse with secondary completion		Age difference Respondent - spouse	
	Men	Women	Men	Women	Men	Women
Born bet. 1968-72	0.008	0.032**	-0.012	0.052**	-0.191	0.326
× INPRES	(0.019)	(0.016)	(0.021)	(0.024)	(0.218)	(0.362)
No. of obs.	3281	3226	3281	3226	3290	3004
Dep. var. mean	0.83	0.81	0.51	0.52	-4.89	4.80
R-squared	0.25	0.21	0.27	0.25	0.11	0.14

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

Table 7: Potential mechanisms: Fertility

Panel A. IFLS and IFLS-E data						
	(1)	(2)	(3)	(4)	(5)	(6)
	Age 1st preg.		Spacing 1st and 2nd child		No. of births	
	Expanded sample	Restricted sample	Expanded sample	Restricted sample	Expanded sample	Restricted sample
Young cohort	-0.182	-0.111	0.355	0.381	-0.072	-0.089
× INPRES	(0.192)	(0.264)	(1.517)	(1.963)	(0.083)	(0.080)
No. of obs.	5673	3110	4693	2553	5987	3293
Dep. var. mean	22.76	22.06	48.52	47.63	3.77	3.34
R-squared	0.17	0.21	0.15	0.18	0.33	0.32

Panel B. Susenas: All provinces		
	No. of births	
	Expanded sample	Restricted sample
Young cohort	-0.048**	-0.052*
× INPRES	(0.023)	(0.027)
No. of obs.	128,853	69,900
Dep. var. mean	4.20	3.95
R-squared	0.16	0.16

Panel C. Susenas: Restricted to IFLS and IFLS-E provinces		
	No. of births	
	Expanded sample	Restricted sample
Young cohort	-0.037	-0.030
× INPRES	(0.028)	(0.033)
No. of obs.	99,727	53,739
Dep. var. mean	4.14	3.91
R-squared	0.17	0.17

Notes: Expanded sample includes those born between 1950-1975. Restricted sample includes those born between 1957-1962 or 1968-1975. Young cohort for the expanded sample corresponds to those born between 1963 and 1972. Young cohort for the restricted sample corresponds to those born between 1968 and 1972. Covariates include: district, year of birth, month of birth, ethnicity (Javanese dummy), birth-year interacted with: the number of school-aged children in the district in 1971 (before the start of the program), the enrollment rate of the district in 1971 and the exposure of the district to another INPRES program: a water and sanitation program. Robust standard errors clustered at district of birth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 8: Potential mechanisms: Household resources and neighborhood quality

Panel A. Expanded sample						
	(1)	(2)	(3)	(4)	(5)	(6)
	Household resources			Neighborhood quality		
	Per capita expenditure	Poor housing	Asset index	Education index	Health index	Poverty index
Born bet. 1963-1972	0.046**	-0.031*	0.024	0.014	0.024	-0.003
× INPRES	(0.018)	(0.018)	(0.028)	(0.021)	(0.022)	(0.022)
No. of obs.	11941	11507	11951	9329	9078	8015
Dep. var. mean	12.860	-0.038				
R-squared	0.15	0.38	0.20	0.56	0.48	0.50
Panel B. Restricted sample						
	(1)	(2)	(3)	(4)	(5)	(6)
	Household resources			Neighborhood quality		
	Per capita expenditure	Poor housing	Asset index	Education index	Health index	Poverty index
Born bet. 1968-1972	0.054**	-0.040*	0.037	-0.001	0.055**	-0.012
× INPRES	(0.026)	(0.020)	(0.029)	(0.031)	(0.027)	(0.027)
No. of obs.	6752	6502	6756	5118	4974	4377
Dep. var. mean	12.899	-0.042				
R-squared	0.15	0.38	0.19	0.58	0.48	0.51

Notes: Total log per capita expenditure in 2012-14 based on weekly or monthly per capita food and non-food expenditure in 2012 *Rupiah*). We exclude annual non-food expenditure (which includes items like land/vehicle purchases). Index of poor housing quality in 2012-14 includes: poor toilet, poor floor, poor roof, poor wall, high occupancy per room. Poor toilet is captured by not having access to a toilet (including shared or public toilet). Poor floor includes board/lumber, bamboo, or dirt floor. Poor roof includes leaves or wood. Poor wall includes lumber/board and bamboo/mat. Occupancy per room is defined as more than two persons per room in the house (based on household size). Asset index includes the following asset ownership: savings, vehicle, land, TV, appliances, refrigerator, and house. The community index is only available for households that continue to reside in the original IFLS enumeration areas. Education index includes the number of primary, junior high, and high schools used by the community. Health index includes the following: an indicator for having a majority of the residents using piped water, an indicator for having a majority of the residents using private toilet, the number of community health centers, and the number of midwives available to the community. Poverty index includes the fraction of households in the community in the subsidized rice program (*Raskin*), subsidized national health insurance (*Jamkesmas*), subsidized regional (province or district) health insurance (*Jamkesda*). Expanded sample includes those born between 1950-1972. Restricted sample includes those born between 1957-1962 or 1968-1972. Covariates include: district, year of birth, month of birth, ethnicity (Javanese dummy), birth-year interacted with: the number of school-aged children in the district in 1971 (before the start of the program), the enrollment rate of the district in 1971 and the exposure of the district to another INPRES program: a water and sanitation program. Robust standard errors clustered at district of birth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 9: Potential mechanisms: Household resources and neighborhood quality by gender

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Per capita expenditure		Poor housing		Asset index		Education index		Health index		Poverty index	
	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female
Panel A. Expanded sample												
Born bet. 1963-1972	0.044*	0.051	-0.036	-0.031	0.024	0.028	0.059	-0.003	0.036	0.014	0.015	-0.022
× INPRES	(0.023)	(0.034)	(0.025)	(0.022)	(0.034)	(0.035)	(0.038)	(0.029)	(0.030)	(0.032)	(0.035)	(0.033)
No. of obs.	6007	5934	5785	5722	6007	5944	4526	4776	4399	4651	3920	4067
Dep. var. mean	12.912	12.806	-0.045	-0.030								
R-squared	0.15	0.19	0.38	0.42	0.50	0.49	0.57	0.58	0.50	0.49	0.51	0.53
Panel B. Restricted sample												
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Per capita expenditure		Poor housing		Asset index		Education index		Health index		Poverty index	
	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female
Born bet. 1968-1972	0.058*	0.060	-0.028	-0.070***	0.024	0.074**	0.022	0.008	0.036	0.079*	0.017	-0.069*
× INPRES	(0.033)	(0.042)	(0.036)	(0.023)	(0.043)	(0.035)	(0.042)	(0.046)	(0.035)	(0.044)	(0.036)	(0.040)
No. of obs.	3408	3344	3276	3226	3407	3349	2464	2617	2389	2549	2122	2218
Dep. var. mean	12.938	12.858	-0.040	-0.044								
R-squared	0.15	0.21	0.40	0.40	0.21	0.24	0.59	0.60	0.51	0.50	0.53	0.53

Notes: Total log per capita expenditure in 2012-14 based on weekly or monthly per capita food and non-food expenditure in 2012 *Rupiah*). We exclude annual non-food expenditure (which includes items like land/vehicle purchases). Index of poor housing quality in 2012-14 includes: poor toilet, poor floor, poor roof, poor wall, high occupancy per room. Poor toilet is captured by not having access to a toilet (including shared or public toilet). Poor floor includes board/lumber, bamboo, or dirt floor. Poor roof includes leaves or wood. Poor wall includes lumber/board and bamboo/mat. Occupancy per room is defined as more than two persons per room in the house (based on household size). Asset index includes the following asset ownership: savings, vehicle, land, TV, appliances, refrigerator, and house. The community index is only available for households that continue to reside in the original IFLS enumeration areas. 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 most of the residents using private toilet, the number of community health centers, and the number of midwives available to the community. Poverty index includes the fraction of households in the community in the subsidized rice program (*Raskin*), subsidized national health insurance (*Jamkesmas*), subsidized regional (province or district) health insurance (*Jamkesda*). Expanded sample includes those born between 1950-1972. Restricted sample includes those born between 1957-1962 or 1968-1972. Covariates include: district, year of birth, month of birth, ethnicity (Javanese dummy), birth-year interacted with: the number of school-aged children in the district in 1971 (before the start of the program), the enrollment rate of the district in 1971 and the exposure of the district to another INPRES program: a water and sanitation program. Robust standard errors clustered at district of birth. Significance: *** p < 0.01, ** p < 0.05, * p < 0.10.

Table 10: Potential mechanisms: Migration

Panel A. First generation				
	(1)	(2)	(3)	(4)
	Expanded sample		Restricted Sample	
	Ever moved	Moved by 2012-14	Ever moved	Moved by 2012-14
Young cohort	-0.002	0.005	-0.000	0.001
× INPRES	(0.010)	(0.010)	(0.012)	(0.012)
No. of obs.	14049	13199	7818	7356
Dep. var. mean	0.342	0.296	0.345	0.297
R-squared	0.25	0.25	0.25	0.25

Panel B. Second generation				
	(1)	(2)	(3)	(4)
	Expanded sample		Restricted Sample	
	Maternal mig. pre	Maternal mig. post	Maternal mig. pre	Maternal mig. post
Mother: Young cohort	-0.006	0.014	0.022	0.033
× INPRES	(0.016)	(0.016)	(0.022)	(0.027)
No. of obs.	15464	15397	8117	8083
Dep. var. mean	0.285	0.285	0.280	0.271
R-squared	0.28	0.11	0.30	0.14

Notes: Ever moved is an indicator that takes the value one if the adult respondent's district of birth is different from the respondent's current district of residence. Moved by 2012-14 is indicator that compares the respondent's district of birth and his or her district of residence in 2012 (IFLS-E) or 2014 (IFLS). Maternal mig. pre is an indicator that takes the value one if the mother's district of birth is different from the child's district of birth. Maternal mig. post is an indicator that takes the value one if the child's district of birth is different from the child's current district of birth. Expanded sample includes those born between 1950-1975. Restricted sample includes those born between 1957-1962 or 1968-1975. Young cohort for the expanded sample corresponds to those born between 1963 and 1972. Young cohort for the restricted sample corresponds to those born between 1968 and 1972. See Table 1 for covariates. Robust standard errors clustered at district of birth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Appendix

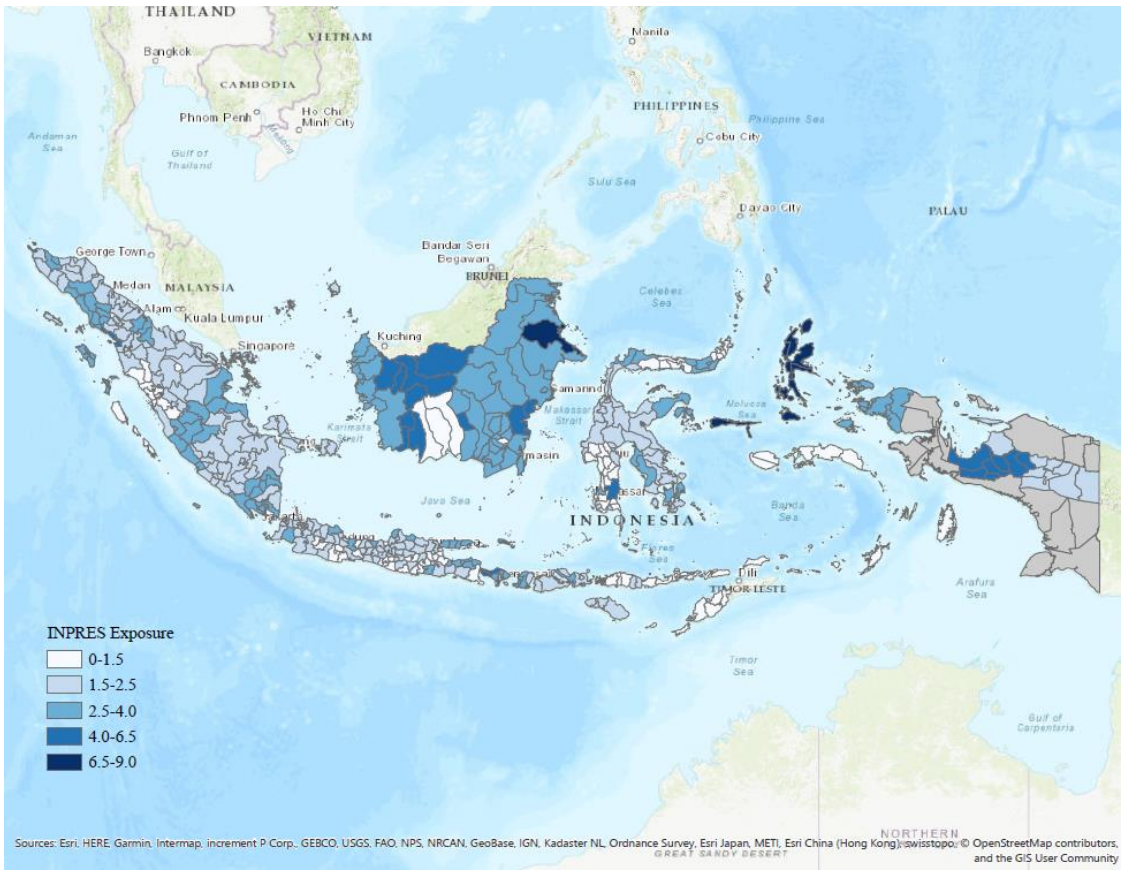
A Data Appendix

A.1 Coverage of the IFLS and INPRES program

Program intensity

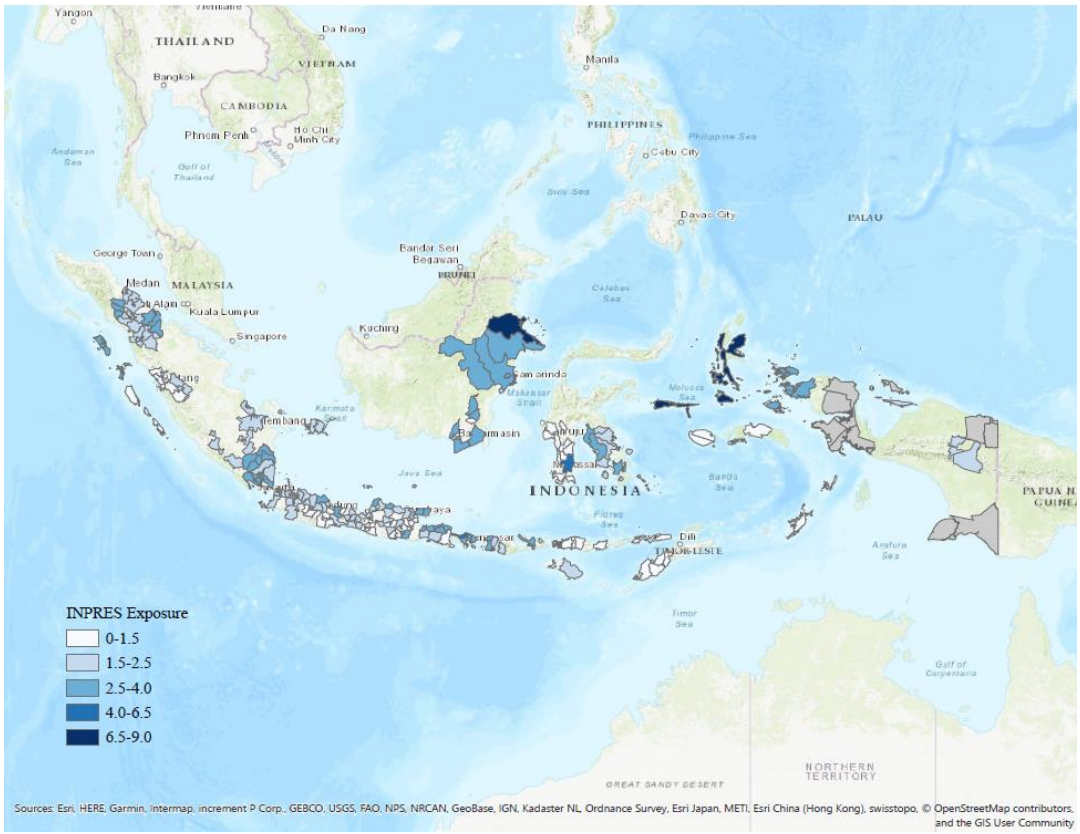
We compare the intensity of the INPRES school construction project in the IFLS and IFLS-E against the national record. The IFLS provinces include 13 out of Indonesia's 26 provinces in 1993. They include: North Sumatra, West Sumatra, South Sumatra, Lampung, Jakarta, West Java, Central Java, Yogyakarta,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

Figure A.1: INPRES exposure - All Indonesia



Source: Authors' calculations based on Duflo (2001)

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



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

A.2 Data construction

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

Date and district of birth

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

Linking the first and second generation

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

Long-term outcomes for the first generation

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

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

Intergenerational outcomes

Using children's height and age, we calculate height-for-age z-score using the WHO reference data.[1] 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.

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

INPRES exposure variable

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

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

Table A.1: Summary statistics

	(1) Mean	(2) SD	(3) 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-reported healthy	0.719	0.450	10,801
Days missed activities	1.841	3.267	10,429
Any chronic condition	0.422	0.494	10,738
No. of chronic conditions	0.683	1.007	10,738
Mental health screening score	5.513	4.694	10,254
Panel B. Second generation			
First child	0.335	0.472	16,098
Male child	0.512	0.500	16,098
Child's year of birth	1,992	7.562	16,098
Javanese	0.425	0.494	16,098
Mother born 1963-1972	0.556	0.497	13,322
Father born 1963-1972	0.478	0.500	12,569
Height for age z-score	-1.649	1.096	25,482
Stunted	0.366	0.482	25,482
Anemia	0.249	0.433	21,952
Self-reported health	0.879	0.326	22,748

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

A.3 Data Comparison

Comparison of non-coresident and coresident second generation individuals in the IFLS

The longitudinal nature of the main IFLS allows us to track non-coresident children who make up the second generation. Non-coresident children in the IFLS account for about 45% of the total sample of the second generation individuals in our sample. Including only coresident children in the second generation individuals may introduce some sample selection. To address this, we explore the characteristics of coresident and non-coresident children (Table A.2). We begin by comparing the raw sample mean, followed by the adjusted differences. The adjusted difference takes into account mother's district of birth (in columns 9-10), and then we include mother's year of birth (in columns 11-12). These comparisons suggest that coresident children are more likely to be younger, come from households with a higher asset index, and their parents have higher education. Additionally, co-resident children are more likely to have mothers who were older at the time of their first birth.

Comparison of coresident second generation individuals in the IFLS and a nationally representative survey

We also examine the representativeness of the coresident children's characteristics in the IFLS by comparing the IFLS against the 2014 Socioeconomic survey, 2014 Susenas (Table A.3). Comparing the characteristics of the national sample to the sample restricted to the IFLS and IFLS-E provinces (columns 4-6 of Table A.3) suggests that the child characteristics are similar to children's characteristics in the national sample. Additionally, when we restrict the 2014 Susenas to the IFLS provinces (columns 7-9 of Table A.3), the children's characteristics are similar to the co-resident children in the IFLS in columns 4-6 of Table A.2.

Table A.2: Comparison of non-coresident and coresident children in the IFLS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Out of Household			In Household			Raw		Mother's birth place FE		Mother's birth place, year of birth FE	
	Mean	SD	Obs	Mean	SD	Obs	Diff	SE	Mean Diff	SE	Mean Diff	SE
Child male	0.50	0.50	7,214	0.52	0.50	5,475	0.021**	0.01	0.02**	0.01	0.02*	0.01
Child age in 2014	27.00	5.48	6,524	19.40	6.76	5,475	-7.599***	0.13	-7.58***	0.14	-5.65***	0.12
Log HH expenditure	14.59	0.77	6,558	14.82	0.72	5,461	0.223***	0.02	0.19***	0.02	0.20***	0.02
Asset index	-0.12	1.05	6,574	0.13	0.85	5,468	0.253***	0.02	0.21***	0.02	0.23***	0.02
Household size	4.76	2.74	6,716	5.95	2.35	5,475	1.190***	0.07	1.19***	0.07	1.28***	0.07
Child: primary complete	0.65	0.48	7,080	0.58	0.49	5,461	-0.073***	0.01	-0.07***	0.01	-0.04***	0.01
Child: secondary complete	0.32	0.47	7,005	0.31	0.46	5,459	-0.015	0.01	-0.01	0.01	0.01	0.01
Mother: primary complete	0.67	0.47	7,213	0.75	0.43	5,475	0.079***	0.01	0.07***	0.01	0.02**	0.01
Father: primary complete	0.75	0.44	6,646	0.80	0.40	5,191	0.052***	0.01	0.05***	0.01	0.02***	0.01
Mother: years of schooling	6.62	4.12	7,213	7.61	4.30	5,475	0.982***	0.10	0.87***	0.09	0.31***	0.08
Father: years of schooling	7.69	4.25	6,646	8.52	4.31	5,191	0.833***	0.10	0.73***	0.09	0.37***	0.09
Urban	0.60	0.49	6,716	0.61	0.49	5,475	0.017	0.01	-0.02*	0.01	-0.02	0.01
Mother born 1963-72	0.44	0.50	7,214	0.69	0.46	5,475	0.248***	0.01	0.24***	0.01	0.00	0.00
Mother's year of birth	1,961.30	5.75	7,214	1,964.73	5.58	5,475	3.427***	0.15	3.31***	0.14	-0.00	0.00
Father’s year of birth	1,955.95	7.87	6,654	1,960.22	7.64	5,195	4.262***	0.20	4.09***	0.20	0.70***	0.12
Ever moved	0.08	0.28	6,707	0.03	0.18	5,473	-0.050***	0.01	-0.05***	0.01	-0.05***	0.01
Mother's age at first birth	22.65	4.59	2,604	25.54	5.48	1,288	2.894***	0.20	2.81***	0.21	3.61***	0.22

Notes: Observations in cols. 1-6 weighted by IFLS cross sectional weight. Standard errors clustered at mother's district of birth.
Significance: *** p < 0.01, ** p < 0.05, * p < 0.10.

Table A.3: Coresident children characteristics in Susenas 2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All provinces			IFLS and IFLS-E provinces			IFLS provinces		
	Mean	SD	Obs	Mean	SD	Obs	Mean	SD	Obs
Child male	0.56	0.50	177,436	0.55	0.50	135,698	0.55	0.50	105,948
Child primary complete	0.86	0.34	174,130	0.87	0.34	133,075	0.87	0.33	104,485
Child secondary complete	0.64	0.48	174,130	0.65	0.48	133,075	0.65	0.48	104,485
Child year of birth	1,993.48	7.34	177,436	1,993.44	7.37	135,698	1,993.34	7.37	105,948
Household size	5.23	1.85	177,436	5.21	1.84	135,698	5.15	1.80	105,948
Mother born 1963-72	0.65	0.48	177,436	0.65	0.48	135,698	0.65	0.48	105,948
Mother's age at marriage	19.76	4.01	177,433	19.72	4.03	135,695	19.62	4.01	105,945
Mother's year of birth	1,964.22	5.83	177,436	1,964.20	5.83	135,698	1,964.18	5.84	105,948
Mother: primary complete	0.75	0.43	160,555	0.75	0.43	122,495	0.75	0.43	97,105
Mother: secondary complete	0.38	0.49	160,555	0.38	0.49	122,495	0.38	0.49	97,105
Mother: years of schooling	7.49	3.79	160,288	7.48	3.79	122,296	7.47	3.80	96,950
Urban	0.54	0.50	177,436	0.57	0.50	135,698	0.59	0.49	105,948

Notes: Observations weighted by Susenas cross sectional weight.

Comparison of primary school effects with a nationally representative survey

The IFLS contains rich information, but its relatively small sample may be a concern. To address this, we examine the representativeness of the IFLS and IFLS-E by comparing our data against the 2014 Socioeconomic survey, 2014 Susenas, since the fifth wave of the IFLS was administered in 2014. The Susenas is a nationally representative survey that is administered annually. The survey covers every district in Indonesia and includes some questions that are also available in the IFLS and IFLS-E. Table A.4 presents primary completion rates using the 2014 Susenas. Panel A presents estimated effects for all provinces and Panel B presents effects for the IFLS and IFLS-E provinces. The effects are similar in panels A and B, and these estimated effects are similar to our main estimates on first generation primary school completion shown in Table B.2.

Table A.4: Primary completion: 2014 Susenas

Panel A. Susenas 2014: all provinces						
	(1)	(2)	(3)	(4)	(5)	(6)
	Expanded sample			Restricted sample		
	All	Male	Female	All	Male	Female
Young cohort	0.013***	0.016***	0.011**	0.017***	0.017***	0.017***
× INPRES	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
No. of obs.	236,270	122,213	114,057	130,855	67,674	63,181
Dep. var. mean (unweighted)	0.77	0.81	0.73	0.77	0.81	0.73
Dep. var. mean (weighted)	0.76	0.80	0.72	0.76	0.80	0.72
R-squared	0.099	0.084	0.107	0.099	0.088	0.111
Panel B. Susenas 2014: restricted to the IFLS and IFLS-E provinces						
	(1)	(2)	(3)	(4)	(5)	(6)
	Expanded sample			Restricted sample		
	All	Male	Female	All	Male	Female
Young cohort	0.017***	0.019***	0.015**	0.022***	0.022***	0.025***
× INPRES	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.007)
No. of obs.	180,283	92,552	87,731	99,308	50,992	48,316
Dep. var. mean (unweighted)	0.77	0.81	0.73	0.77	0.81	0.73
Dep. var. mean (weighted)	0.76	0.76	0.72	0.76	0.76	0.72
R-squared	0.103	0.086	0.111	0.104	0.088	0.117

Notes: Expanded sample includes those born between 1950-1972. Restricted sample includes those born between 1957-1962 or 1968-1972. Young cohort for the expanded sample corresponds to those born between 1963 and 1972. Young cohort for the restricted sample corresponds to those born between 1968 and 1972. Covariates include: district, year of birth, month of birth, ethnicity (Javanese dummy), birth-year interacted with: the number of school-aged children in the district in 1971 (before the start of the program), the enrollment rate of the district in 1971 and the exposure of the district to another INPRES program: a water and sanitation program. Robust standard errors clustered at district of birth.
Significance: *** p < 0.01, ** p < 0.05, * p < 0.10.

We also compare migration between the IFLS and IFLS-E and 2014 Susenas in Table A.5. Migration is defined as currently residing in a district that is different from one's district of birth. The estimated effects are similar in Panels A and B, and the estimates are similar to our main estimates.

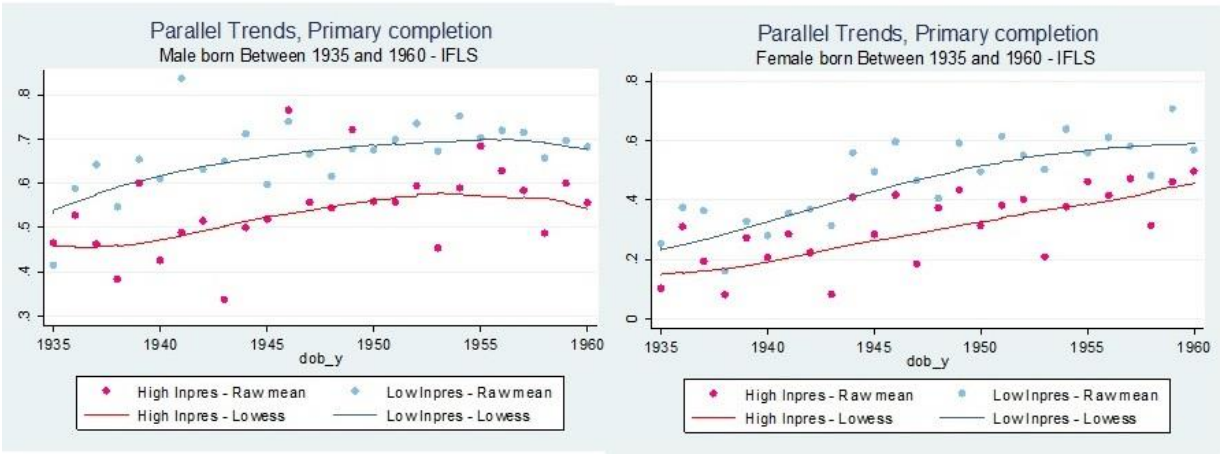
Table A.5: Potential mechanisms: Migration

	(1)	(2)
	Current residence different from birth place	
	Expanded sample	Restricted sample
Panel A. IFLS and IFLS-E data		
Young cohort	0.005	0.001
× INPRES	(0.010)	(0.012)
No. of obs.	13,199	7,356
Dep. var. mean	0.296	0.297
R-squared	0.25	0.25
Panel B. Susenas: All provinces		
Young cohort	0.000	0.001
× INPRES	(0.002)	(0.003)
No. of obs.	258,308	141,048
Dep. var. mean	0.250	0.254
R-squared	0.115	0.117
Panel C. Susenas: Restricted to IFLS and IFLS-E provinces		
Young cohort	0.004	0.001
× INPRES	(0.003)	(0.003)
No. of obs.	198,337	141,048
Dep. var. mean	0.229	0.234
R-squared	0.125	0.117

Notes: Expanded sample includes those born between 1950-1975. Restricted sample includes those born between 1957-1962 or 1968-1975. Young cohort for the expanded sample corresponds to those born between 1963 and 1972. Young cohort for the restricted sample corresponds to those born between 1968 and 1972. See Table 1 for covariates. Ever migrate takes the value one if respondent is not currently residing in his/her district of birth. Robust standard errors clustered at district of birth.
Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

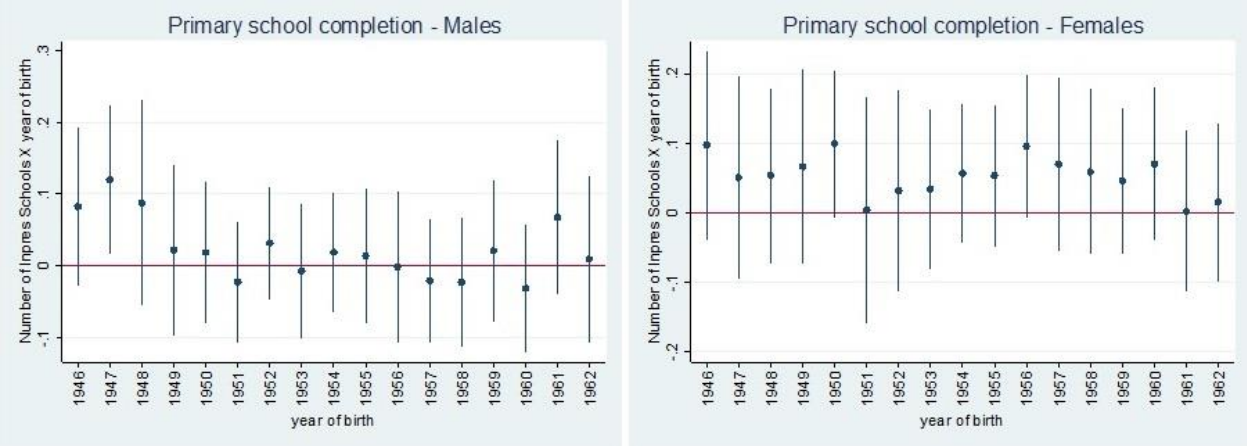
B Additional figures and tables

Figure B.1: Pre-trends raw data: Primary school completion – IFLS



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

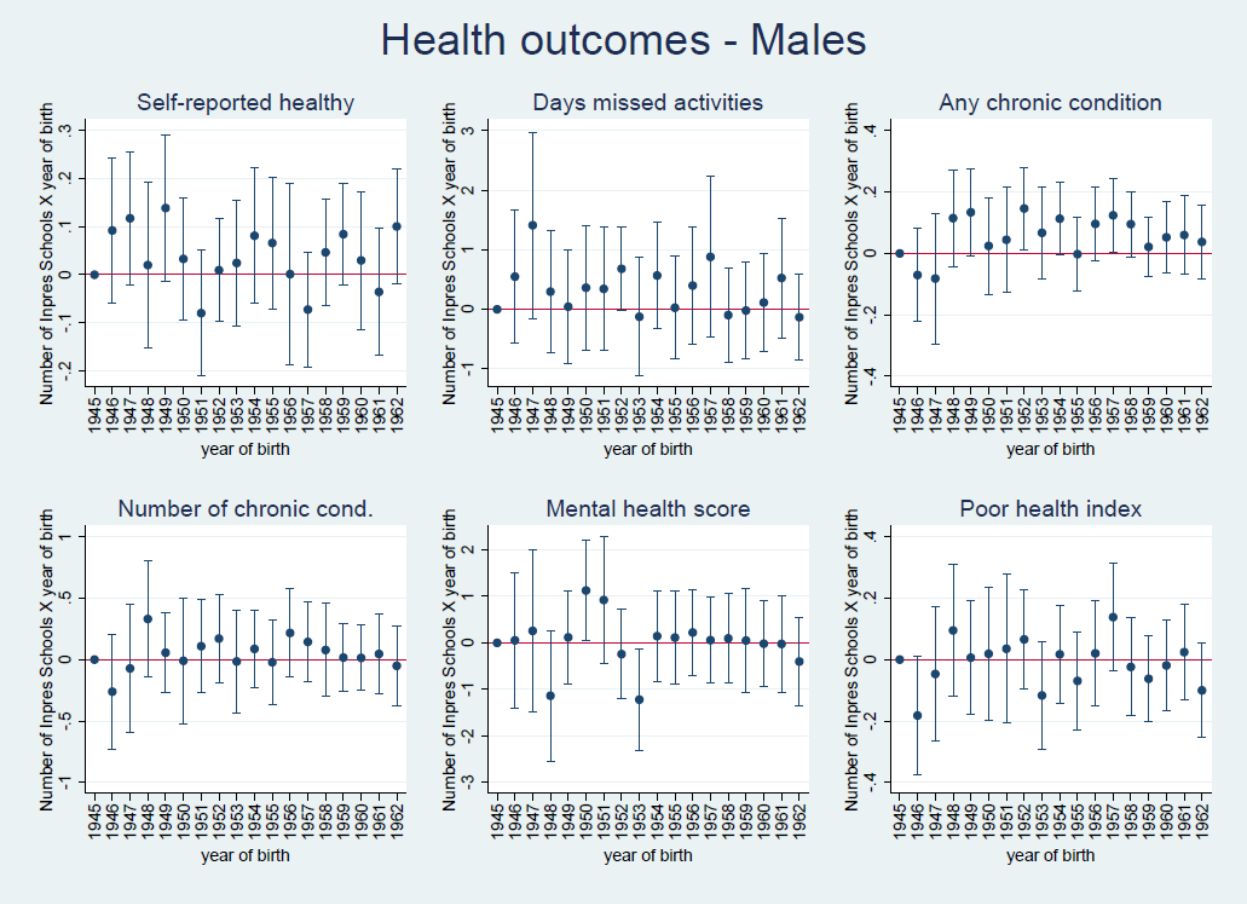
Figure B.2: Pre-trends raw regression: Primary school completion – IFLS



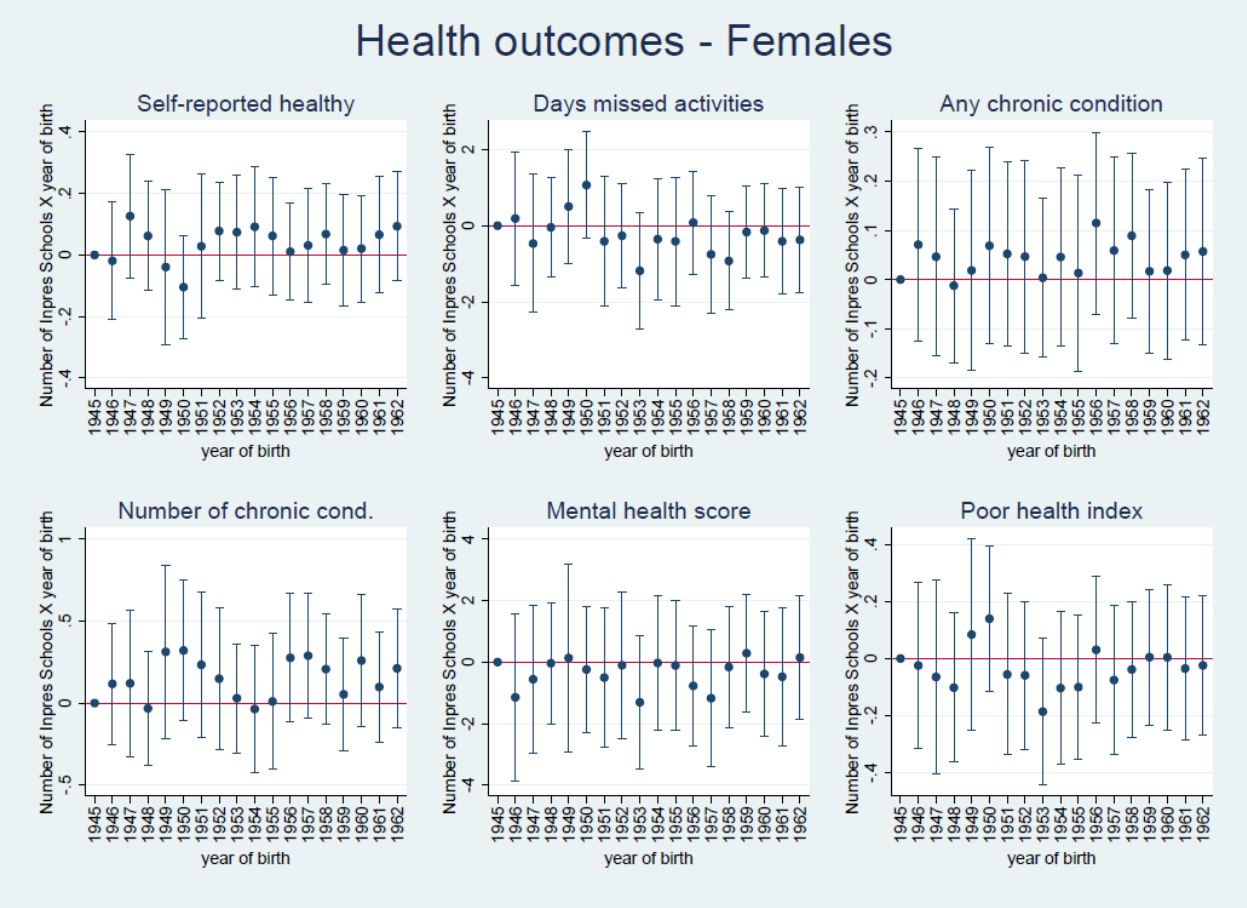
Notes: Coefficients from difference-in-differences model that interacts the number of INPRES schools and year of birth for cohorts born between 1945 and 1962. Year of birth 1945 is the omitted category. Bars indicate the 95% confidence intervals.

Figure B.3: Pre-trends raw regression: First generation's health outcomes

Panel A. Males

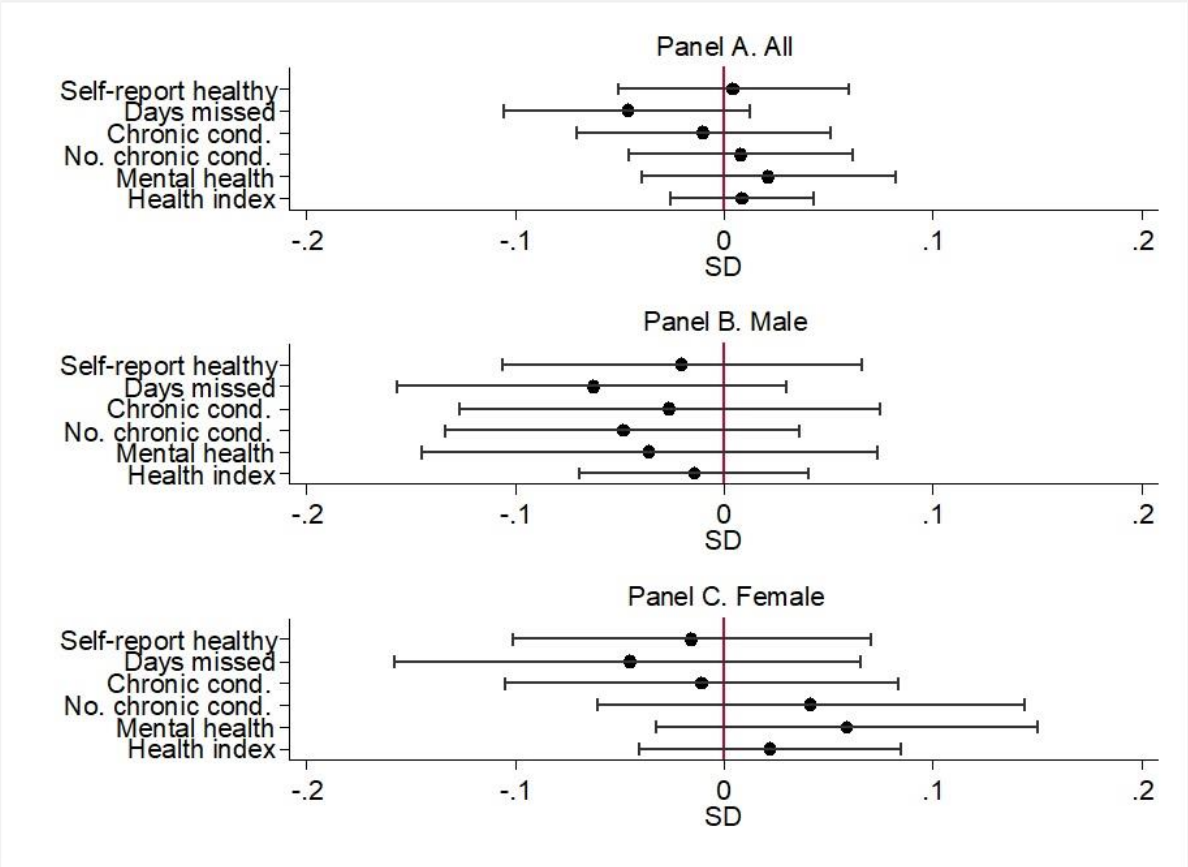


Panel B. Females



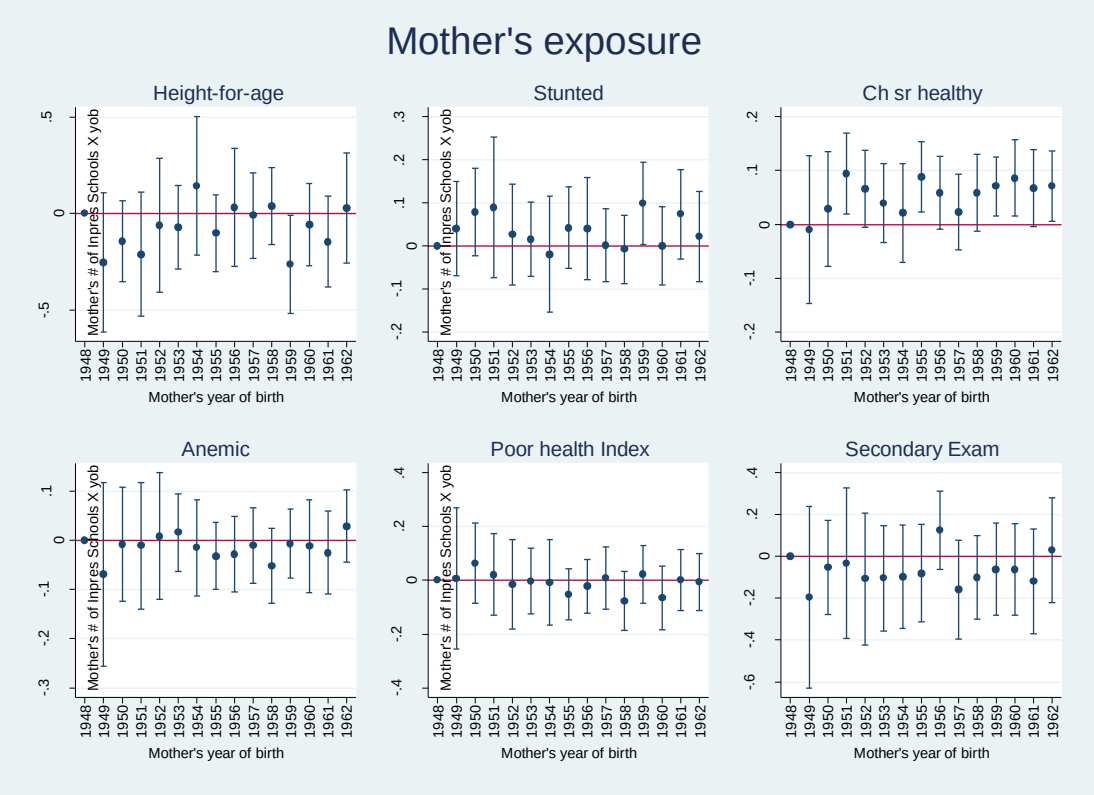
Notes: Coefficients from difference-in-differences model that interacts the number of INPRES schools and year of birth for cohorts born between 1945 and 1962. Bars indicate the 95% confidence intervals.

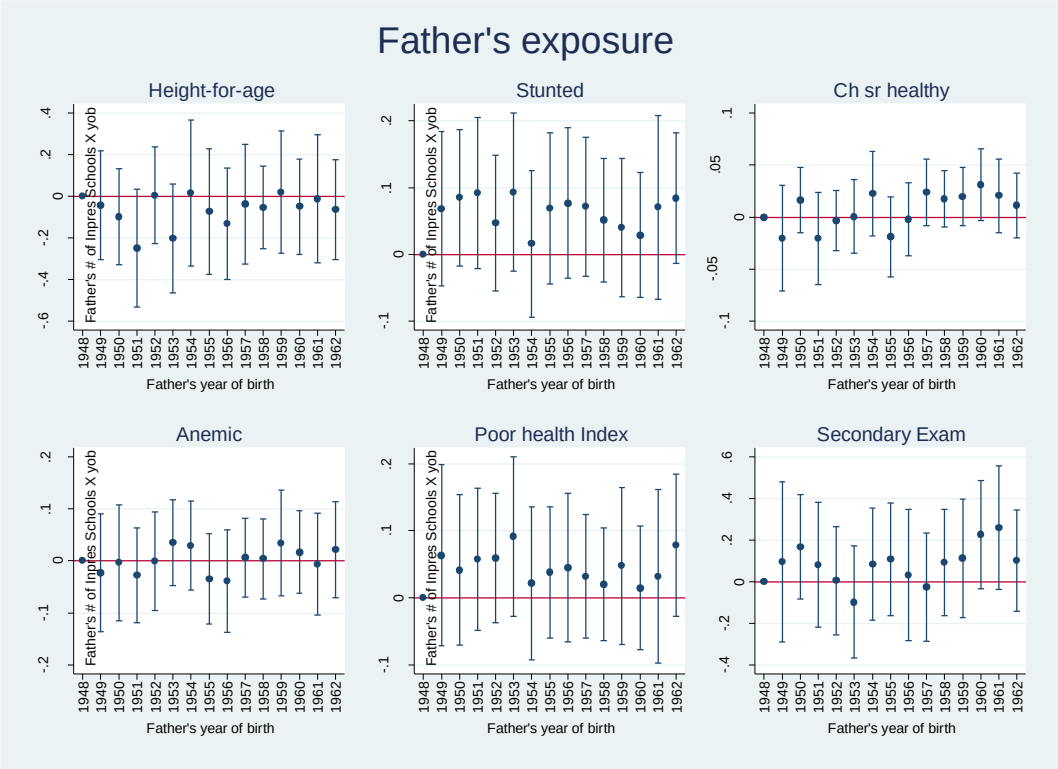
Figure B.4: Placebo regression: First generation



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

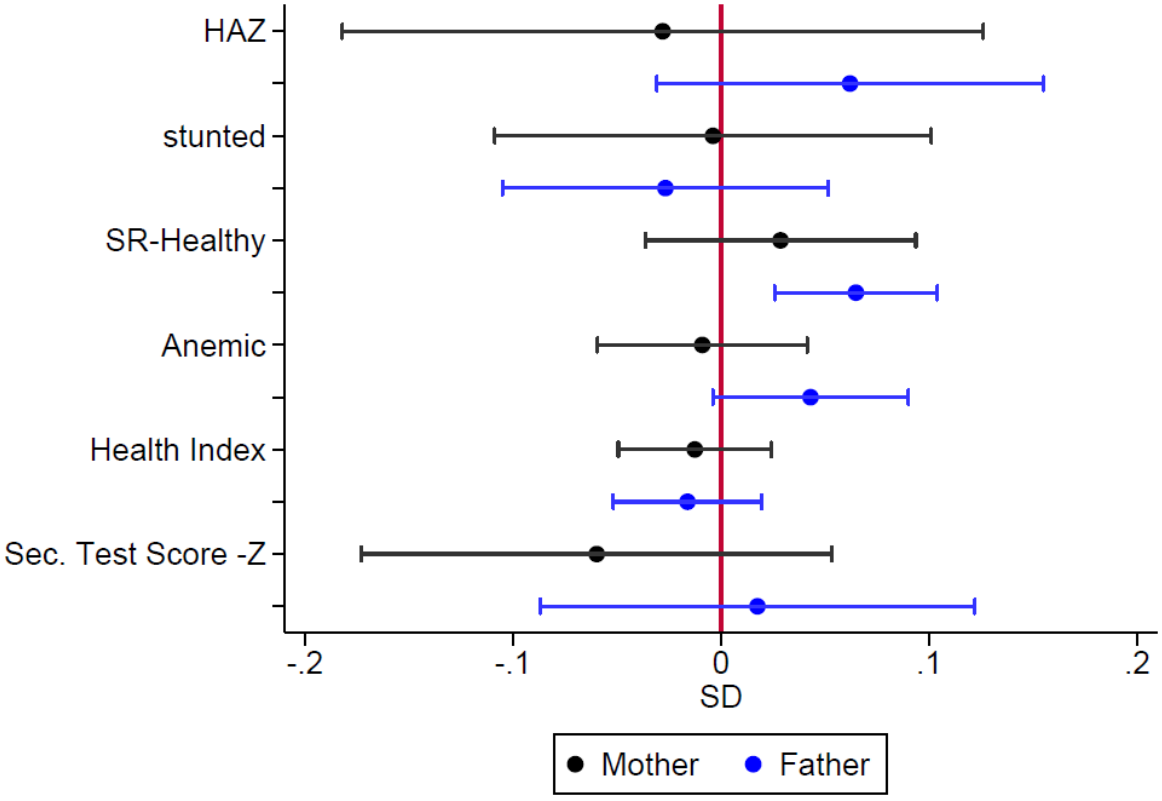
Figure B.5: Pre-trends raw regression: Second generation outcomes





Notes: “Ch sr healthy” corresponds to an indicator of child's self-reported health status as ‘very healthy’ or ‘healthy’. Coefficients from difference-in-differences model that interacts the number of INPRES schools in parent's birth place and parent's year of birth. Sample corresponds to children from parent's born between 1948-1962 (we exclude children from parents born in 1945-1947 because of very small sample sizes). Bars indicate the 95% confidence intervals.

Figure B.6: Placebo regression: Second generation - each parent separately



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

Table B.1: Summary of the Literature

Study	Research question/Outcomes	Data and sample	Methods	Results
First generation medium-term effects				
Duflo (2001)	Medium run effects on education and earnings for men.	Cross-sectional data. Sample of men born between 1950 and 1975 from the Indonesian intercensal survey (SUPAS)	Difference-in-differences exploiting geographic variation on the number of INPRES schools built in district of birth and cohort variation. 2SLS that instruments years of schooling using INPRES exposure.	Each new school constructed per 1000 children increased years of schooling by 0.12 to 0.2 and earnings by 1.5 to 2.7 percent for men.
Duflo and Breinova (2004)	Medium run effects of mother's and father's education on child health and fertility.	Cross-sectional data. Sample of men and women born between 1950 and 1975 from the Indonesian intercensal survey (SUPAS)	2SLS: to instrument for average education in a family, they combine cohort variation and program intensity in exposure to INPRES for each partner. To instrument for the difference in education between husband and wife, they add the interaction between whether the husband was old to benefit from the program (age >12), the age difference between husband and wife and the number of schools constructed in the district of birth.	- Higher mother and father education reduced child mortality. - Female education matters more than male education in delaying marriage and lower fertility (number of children by age 15 or 25). The 2SLS estimates of the impacts of education gaps were not significant.

Karachiwalla and Palloni (2019)	Long-term impact of education on participation in agriculture.	Cross-sectional data. Men born between 1950 and 1972 in SUPAS 1995.	Difference-in-Differences as in Duflo (2001) and 2SLS.	Treated men are more likely to be employed for a wage and work in non-agricultural sectors.
Ashraf et al. (2020)	How the impact of INPRES on women's education depend on bride price customs.	Cross-sectional data. Women born between 1950 and 1972 in SUPAS 1995.	Difference-in-Differences as in Duflo (2001) allowing for heterogeneity by bride price ethnic groups.	Exposure to INPRES increased the probability of primary school completion by 2.5 percentage points for bride price females but no significant effect for non-bride price females.
First generation general equilibrium effects and human capital externalities				
Duflo (2004)	Medium run general equilibrium effects on labor market. Human capital externalities.	Cross-sectional data. Repeated cross-section surveys from the annual Indonesian Labor Force Survey (SAKERNAS), from 1986 to 1999. Sample: males aged 20-60, excluding Jakarta.	2SLS: use interactions between survey year and the number of INPRES school per 1000 children at the district of residence as instruments for the fraction of educated workers in the district. Also, a two-sector model as framework to interpret these findings.	Between 1986 and 1999, an increase of 10 percentage points in the fraction of primary school graduates in the labor force decreased the wages of the older workers (both educated and uneducated) by 4% to 10%.
Martinez-Bravo (2017)	Effect on local governance and provision of public goods.	Cross-sectional data. Information on local leaders from the Village Potential Statistics (PODES) from 1986 to 2003.	Difference-in-differences by comparing villages before and after the first post-1992 election, when individuals exposed to the program could potentially become local leaders.	- Increased provision of public goods: number of doctors, primary health care centers, access to basic services. - Effects are stronger in villages exposed to a higher program intensity.

Zha (2019)	Impacts of a market-level shock to the education distribution on marital outcomes.	Cross-sectional data. Women born between 1953 and 1961 and between 1965 and 1970 in SUPAS 2005 and Census 2010.	Difference-in-Differences as in Duflo (2001) and 2SLS.	- In densely populated areas, INPRES did not affect primary school attainment rate. -INPRES decreased secondary school attainment rate for both men and women due to crowding out of teacher resources. - When average education of other women decreases, a woman marries earlier, holding their potential husbands' education distribution unchanged.
First Generation long-term effects				
Bharati et al. (2016)	Effect on time preference.	Longitudinal data. Individuals born between 1958-1962 and 1968-1972 from the Indonesian Family Life Survey (IFLS).	Difference-in-Differences as in Duflo (2001).	- Higher education among women increased their patience (as measured by the hypothetical time-preference question).
Bharati et al. (2019)	Program effect on education to mitigate early-life weather shocks.	Longitudinal data. Individuals born between 1957-1961 and 1969-1973 from the Indonesian Family Life Survey (IFLS).	Difference-in-differences exploiting variation in rainfall in early life, and geographic variation on the number of INPRES schools built in district of birth and cohort variation.	- Individuals who experienced a negative weather shock in early life and were exposed to the program can catch up in their educational attainment.

First Generation long-term effects and intergenerational effects				
Akresh et al. (2019)	Impacts on long-term and intergenerational effects.	Cross-sectional data. First generation individuals born between 1950 and 1972 and their co-resident offspring from the 2016 Susenas.	Difference-in-Differences as in Duflo (2001).	- First generation: Higher educational attainment for men and women. For men: more likely to be employed, work in the formal sector, work in the non-agricultural sector, and migrate. For women: more likely to migrate and decline in number of children (measured as children co-resident in the household). For households: increase in several measures of expenditures and assets, and higher taxes payments. - Second generation: Mother's exposure increases their children's educational attainment.
Mazumder et al (2019)	Effect on primary school completion and intergenerational effect on primary school performance.	Longitudinal data. First generation individuals born between 1950 and 1972 and their offspring from IFLS and IFLS-East regardless of co-residency.	Difference-in-Differences as in Duflo (2001).	- Higher rates of primary school completion for the first generation of men and women. - Children whose mothers were exposed to the program score higher on the national primary school examination.

Bazzi el al., (2020)	How private school markets (in particular Islamic schools at different levels of education) responded to INPRES mass primary school expansion. Does that process influence identity and beliefs and its transmission to the next generation?	- Mix survey and administrative data. Administrative data of school registries (longitudinal). School curriculum data. Repeated cross-sectional data from National Sample survey (Susenas 2012-2018) to capture individual school enrollment, identity, and religiosity. Cross-sectional data from 2010 population Census and 2008 survey of Islamic beliefs and practices. Data on legislature candidates.	- Individual responses: See Duflo (2001) - School level responses: Difference-in-Differences estimations at the type of school, district, and year of establishment level. Geographic variation: number of INPRES schools built in district. Time variation: post 1972.	- Primary enrollment shifted towards state schools, However, Islamic secondary schools supply responded and absorbed the higher demand for continued education. The Islamic sector also increased religious curriculum. - Individuals exposed report greater religiosity and transmit these religious values to the next generation.

Table B.2: Replication: Primary completion

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

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

Table B.3: First generation health outcomes with multiple hypothesis adjustment

Panel A. Expanded sample		
	(1)	(2)
	Original p-value	Adjusted p-value
Self-reported healthy	0.000	0.000
Number of days missed	0.013	0.026
Any chronic conditions	0.025	0.031
Number of conditions	0.015	0.026
Mental health	0.202	0.202

Panel B. Restricted sample		
	(1)	(2)
	Original p-value	Adjusted p-value
Self-reported healthy	0.009	0.047
Number of days missed	0.437	0.437
Any chronic conditions	0.396	0.437
Number of conditions	0.131	0.219
Mental health	0.078	0.196

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

Table B.4: Additional health outcomes for the first generation

	Panel A: Expanded sample								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	High BP measured			Diagnosed hypertension			BMI		
	All	Male	Female	All	Male	Female	All	Male	Female
Born bet. 1963-1972	0.002	-0.011	0.012	-0.023**	-0.013	-0.042**	0.162*	0.205	0.086
X INPRES	(0.009)	(0.014)	(0.012)	(0.011)	(0.011)	(0.019)	(0.095)	(0.146)	(0.160)
No. of obs.	10480	5072	5408	10727	5265	5462	10046	5011	5035
Dep. var. mean	0.722	0.710	0.732	0.258	0.185	0.326	24.187	22.915	25.460
R-squared	0.07	0.09	0.11	0.09	0.10	0.11	0.16	0.12	0.11
Gender difference p-value		0.209			0.093			0.587	
	Panel B: Restricted sample								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	High BP measured			Diagnosed hypertension			BMI		
	All	Male	Female	All	Male	Female	All	Male	Female
Born bet. 1968-1972	-0.024**	-0.025	-0.015	-0.018	0.002	-0.038**	0.201*	0.236*	0.269
X INPRES	(0.012)	(0.018)	(0.017)	(0.012)	(0.012)	(0.019)	(0.120)	(0.143)	(0.219)
No. of obs.	5837	2807	3030	5939	2900	3039	5639	2781	2858
Dep. var. mean	0.698	0.687	0.708	0.240	0.152	0.318	24.440	23.172	25.631
R-squared	0.08	0.12	0.13	0.11	0.12	0.14	0.16	0.13	0.14
Gender difference p-value		0.688			0.031			0.897	

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. Diagnosed hypertension based on self-reported diagnosis of chronic conditions. BMI defined as a person's weight in kilograms (kg) divided by his or her height in meters squared. 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 B.5: Gender-specific first generation's health behavior

Panel A. Expanded sample					
	(1)	(2)	(3)	(4)	(5)
	Male				Female
	Ever smoked	Currently smoking	No. of daily cigarettes	Alcohol expenditure	Teen pregnancy
Born bet. 1963-1972	0.012	0.029**	-0.320	779.799*	-0.008
X INPRES	(0.013)	(0.013)	(0.380)	(408.435)	(0.014)
No. of obs.	5296	5296	4041	6006	6003
Dep. var. mean	0.851	0.632	12.439	268.183	0.25
R-squared	0.12	0.11	0.16	0.08	0.14
Panel B. Restricted sample					
	(1)	(2)	(3)	(4)	(5)
	Male				Female
	Ever smoked	Currently smoking	No. of daily cigarettes	Alcohol expenditure	Teen pregnancy
Born bet. 1968-1972	-0.006	0.022	-0.161	446.972	-0.015
X INPRES	(0.019)	(0.022)	(0.560)	(500.690)	(0.020)
No. of obs.	2930	2930	2242	3407	3310
Dep. var. mean	0.855	0.656	12.550	247.151	0.25
R-squared	0.15	0.14	0.18	0.06	0.16

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 B.6: Intergenerational outcomes: health index - by gender

Panel A: Expanded Sample									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Mother only			Father only			Both parents		
	All	Sons	Daughters	All	Sons	Daughters	All	Sons	Daughters
Mother bet. 1963-72 X INPRES	-0.034***	-0.027*	-0.047***				-0.037**	-0.022	-0.052**
	(0.011)	(0.015)	(0.015)				(0.015)	(0.021)	(0.021)
Father bet. 1963-72 X INPRES				-0.017	-0.007	-0.029*	-0.003	-0.017	0.010
				(0.012)	(0.016)	(0.016)	(0.017)	(0.024)	(0.022)
No. of obs.	17919	9137	8772	17384	8855	8514	19042	9771	9264
R-squared	0.17	0.20	0.19	0.15	0.18	0.17	0.17	0.20	0.19
Panel B: Restricted sample									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Mother only			Father only			Both parents		
	All	Sons	Daughters	All	Sons	Daughters	All	Sons	Daughters
Mother bet. 1963-72 X INPRES	-0.027*	-0.028	-0.030				-0.026	-0.018	-0.041
	(0.016)	(0.023)	(0.019)				(0.019)	(0.025)	(0.025)
Father bet. 1963-72 X INPRES				-0.019	-0.032	-0.019	-0.010	-0.008	-0.009
				(0.016)	(0.021)	(0.026)	(0.019)	(0.027)	(0.025)
No. of obs.	9911	5068	4829	9007	4627	4357	13707	7073	6617
R-squared	0.17	0.22	0.21	0.15	0.18	0.18	0.17	0.20	0.20

Notes: Overall poor health index corresponds to a summary index from the following health measures for the second generation: being stunted, anemic and self-reported poor health. Sample corresponds to children born to first generation INPRES individuals. Expanded sample includes children born to adults born between 1950-1972. Restricted sample includes children born to adults born between 1957-1962 or 1968-1972. For column 1 and 2, covariates include the following FE: parent year of birth × 1971 enrollment, parent year of birth × 1971 number of children, parent year of birth × water sanitation program, child's gender, birth order, year, and month of birth dummies, urban, and ethnicity (Javanese dummy). Robust standard errors in parentheses clustered at the parent's district of birth. For column 3 (both parents), the sample corresponds to children either born mothers or fathers in the first generation sample. These models include mother's and father's exposure and the full set of covariates for the mother, while for the father we include: province of birth, two-year bins for year of birth fixed effects and interactions between the father's year of birth (in two-year bins) and the father's district-level covariates. In this estimation, standard errors are clustered two-way at the mother and father district of birth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table B.7: Intergenerational outcomes: test scores - by gender

Panel A: Expanded Sample									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Mother only			Father only			Both parents		
	All	Sons	Daughters	All	Sons	Daughters	All	Sons	Daughters
Mother bet. 1963-72 X INPRES	0.083***	0.091	0.063				0.110***	0.142**	0.073
	(0.031)	(0.056)	(0.044)				(0.039)	(0.066)	(0.056)
Father bet. 1963-72 X INPRES				0.047	0.025	0.070	0.006	-0.021	0.005
				(0.033)	(0.051)	(0.071)	(0.043)	(0.066)	(0.068)
No. of obs.	6819	3419	3400	5744	2846	2898	6604	3280	3285
R-squared	0.11	0.16	0.16	0.11	0.17	0.17	0.14	0.21	0.19
Panel B: Restricted sample									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Mother only			Father only			Both parents		
	All	Sons	Daughters	All	Sons	Daughters	All	Sons	Daughters
Mother bet. 1963-72 X INPRES	0.105*	0.042	0.185**				0.112**	0.032	0.178**
	(0.059)	(0.105)	(0.073)				(0.057)	(0.095)	(0.082)
Father bet. 1963-72 X INPRES				0.103	0.020	0.139	0.041	-0.008	-0.021
				(0.065)	(0.100)	(0.106)	(0.056)	(0.080)	(0.080)
No. of obs.	3512	1727	1785	2639	1294	1345	4361	2148	2184
R-squared	0.14	0.23	0.23	0.17	0.27	0.30	0.17	0.27	0.26

Notes: Overall poor health index corresponds to a summary index from the following health measures for the second generation: being stunted, anemic and self-reported poor health. Sample corresponds to children born to first generation INPRES individuals. Expanded sample includes children born to adults born between 1950-1972. Restricted sample includes children born to adults born between 1957-1962 or 1968-1972. For column 1 and 2, covariates include the following FE: parent year of birth × 1971 enrollment, parent year of birth × 1971 number of children, parent year of birth × water sanitation program, child's gender, birth order, year, and month of birth dummies, urban, and ethnicity (Javanese dummy). Robust standard errors in parentheses clustered at the parent's district of birth. For column 3 (both parents), the sample corresponds to children either born mothers or fathers in the first generation sample. These models include mother's and father's exposure and the full set of covariates for the mother, while for the father we include: province of birth, two-year bins for year of birth fixed effects and interactions between the father's year of birth (in two-year bins) and the father's district-level covariates. In this estimation, standard errors are clustered two-way at the mother and father district of birth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table B.8: Intergenerational outcomes with multiple hypothesis adjustment

Panel A. Expanded sample				
	(1)	(2)	(3)	(4)
	Maternal exposure		Paternal exposure	
	Original p-value	Adjusted p-value	Original p-value	Adjusted p-value
Height-for-age	0.067	0.084	0.583	0.583
Stunted	0.065	0.084	0.421	0.527
Anemia	0.014	0.036	0.373	0.527
Self-reported healthy	0.348	0.348	0.334	0.527
Secondary test score	0.008	0.036	0.162	0.527
Panel B. Restricted sample				
	(1)	(2)	(3)	(4)
	Maternal exposure		Paternal exposure	
	Original p-value	Adjusted p-value	Original p-value	Adjusted p-value
Height-for-age	0.336	0.444	0.257	0.428
Stunted	0.285	0.444	0.667	0.667
Anemia	0.355	0.444	0.474	0.593
Self-reported healthy	0.459	0.459	0.116	0.291
Secondary test score	0.076	0.378	0.115	0.291

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

Table B.9: Intergenerational outcomes: average poor health index

Panel A. Expanded sample			
	(1)	(2)	(3)
	Mother only	Father only	Both parents
Mother bet. 1963-72 X INPRES	-0.034***		-0.026
	(0.013)		(0.017)
Father bet. 1963-72 X INPRES		-0.018	-0.008
		(0.013)	(0.018)
No. of obs.	10712	10805	11703
R-squared	0.19	0.17	0.19
Panel B. Restricted sample			
	(1)	(2)	(3)
	Mother only	Father only	Both parents
Mother bet. 1963-72 X INPRES	-0.021		-0.016
	(0.020)		(0.021)
Father bet. 1963-72 X INPRES		-0.020	-0.008
		(0.019)	(0.020)
No. of obs.	6084	5686	8531
R-squared	0.20	0.17	0.20

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

Table B.10: Intergenerational outcomes: Complete secondary school

Panel A. Expanded sample			
	(1)	(2)	(3)
	Mother only	Father only	Both parents
Mother bet. 1963-72 X INPRES	-0.007		-0.012
	(0.010)		(0.014)
Father bet. 1963-72 X INPRES		0.015	0.021
		(0.013)	(0.015)
No. of obs.	13446	10764	12466
Dep. var. mean	0.76	0.78	0.76
R-squared	0.23	0.24	0.25
Panel B. Restricted sample			
	(1)	(2)	(3)
	Mother only	Father only	Both parents
Mother bet. 1963-72 X INPRES	0.005		-0.011
	(0.014)		(0.017)
Father bet. 1963-72 X INPRES		0.018	0.024
		(0.021)	(0.017)
No. of obs.	6724	4725	8040
Dep. var. mean	0.77	0.78	0.77
R-squared	0.26	0.27	0.27

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

Table B.11: Robustness: Alternative exposure based on schools built per year

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	First generation			Second generation			
	Health index			Health index		Test score	
	All	Male	Female	Maternal exposure	Paternal exposure	Maternal exposure	Paternal exposure
Born bet. 1963-1972 × INPRES	-0.032**	-0.027	-0.035*	-0.012	-0.014	0.116***	0.104**
	(0.013)	(0.019)	(0.021)	(0.013)	(0.014)	(0.038)	(0.045)
No. of obs.	9836	4785	5051	17851	17306	6802	5718
R-squared	0.10	0.12	0.11	0.17	0.15	0.11	0.11

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

Table B.12: Robustness: Alternative cohorts

Panel A. Expanded sample							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	First generation			Second generation			
	Health index			Health index		Test score	
	All	Male	Female	Maternal	Paternal	Maternal	Paternal
Born bet. 1963-1975	-0.041***	-0.021	-0.058***	-0.024**	-0.013	0.102***	0.041
× INPRES	(0.013)	(0.021)	(0.019)	(0.012)	(0.011)	(0.029)	(0.034)
No. of obs.	9891	4817	5074	20341	19145	7338	5981
R-squared	0.10	0.12	0.11	0.16	0.14	0.11	0.11
Panel B. Restricted sample							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	First generation			Second generation			
	Health index			Health index		Test score	
	All	Male	Female	Maternal	Paternal	Maternal	Paternal
Born bet. 1968-1975	-0.038**	-0.010	-0.061**	-0.014	-0.012	0.133***	0.086
× INPRES	(0.016)	(0.032)	(0.026)	(0.015)	(0.014)	(0.050)	(0.061)
No. of obs.	5537	2668	2869	12334	10766	4031	2876
R-squared	0.12	0.15	0.14	0.16	0.13	0.13	0.15

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

Table B.13: Potential mechanism: Household resources

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

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

Table B.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
× INPRES	(0.009)	(0.007)	(0.008)	(0.006)	(0.003)	(0.008)
No. of obs.	11835	11849	11934	12132	12121	12022
Dep. var. mean	0.437	0.086	0.092	0.163	0.033	0.234
R-squared	0.21	0.13	0.11	0.39	0.27	0.33
Panel B. Men						
Born between 1963-1972	0.010	0.001	-0.026**	-0.010	0.000	-0.009
× INPRES	(0.013)	(0.009)	(0.012)	(0.008)	(0.003)	(0.010)
No. of obs.	5887	5891	5944	6042	6036	5986
Dep. var. mean	0.435	0.087	0.094	0.164	0.031	0.235
R-squared	0.24	0.15	0.14	0.43	0.27	0.36
Panel C. Women						
Born between 1963-1972	0.013	-0.011	-0.002	-0.002	0.004	-0.019*
× INPRES	(0.012)	(0.009)	(0.008)	(0.009)	(0.005)	(0.011)
No. of obs.	5948	5958	5990	6090	6085	6036
Dep. var. mean	0.439	0.086	0.089	0.162	0.035	0.233
R-squared	0.22	0.15	0.12	0.38	0.31	0.34

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

Table B.15: Potential mechanism: Housing quality (Restricted sample)

	(1)	(2)	(3)	(4)	(5)	(6)
	Drinking water	Bad Toilet	Bad Occupancy	Bad Floor	Bad Roof	Bad Wall
Panel A. All						
Born between 1968-1972	0.016	-0.003	-0.012	-0.006	0.002	-0.020**
× INPRES	(0.013)	(0.008)	(0.010)	(0.008)	(0.004)	(0.009)
No. of obs.	6696	6698	6760	6867	6861	6795
Dep. var. mean	0.449	0.085	0.092	0.161	0.035	0.232
R-squared	0.22	0.14	0.11	0.39	0.27	0.34
Panel B. Men						
Born between 1968-1972	0.006	-0.002	-0.037***	-0.015	0.005	-0.026**
× INPRES	(0.016)	(0.010)	(0.013)	(0.010)	(0.005)	(0.011)
No. of obs.	3319	3319	3363	3408	3405	3371
Dep. var. mean	0.448	0.087	0.096	0.160	0.032	0.231
R-squared	0.25	0.17	0.15	0.43	0.27	0.37
Panel C. Women						
Born between 1968-1972	0.043**	-0.006	0.013	0.000	-0.001	-0.020
× INPRES	(0.020)	(0.012)	(0.014)	(0.012)	(0.007)	(0.014)
No. of obs.	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 B.14 for notes. Restricted sample includes those born between 1957-1962 or 1968-1972. Robust standard errors clustered at district of birth.
Significance: *** p < 0.01, ** p < 0.05, * p < 0.10.

Table B.16: Robustness: Non-movers only

Panel A. Expanded sample							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	First generation			Second generation			
	Health index			Health index		Test score	
	All	Male	Female	Maternal	Paternal	Maternal	Paternal
Born bet. 1963-1972	-0.039*	0.008	-0.074***	-0.037**	-0.030**	0.100***	0.032
× INPRES	(0.023)	(0.033)	(0.028)	(0.014)	(0.015)	(0.038)	(0.036)
No. of obs.	4779	2271	2508	12738	11568	4731	3790
R-squared	0.12	0.15	0.14	0.18	0.16	0.12	0.14
Panel B. Restricted sample							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	First generation			Second generation			
	Health index			Health index		Test score	
	All	Male	Female	Maternal	Paternal	Maternal	Paternal
Born bet. 1968-1972	-0.026	0.019	-0.052	-0.044**	-0.032*	0.142*	0.114
× INPRES	(0.025)	(0.047)	(0.032)	(0.017)	(0.016)	(0.076)	(0.071)
No. of obs.	2662	1275	1387	6987	5941	2476	1778
R-squared	0.13	0.16	0.18	0.18	0.16	0.17	0.20

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

Table B.17: Potential mechanism: Neighborhood quality (Expanded sample)

	(1) Elementary school	(2) Middle school	(3) High school	(4) Piped water	(5) Private providers	(6) Clinic	(7) Midwives	(8) Rice subsidy	(9) National health insurance	(10) District health insurance
Panel A. All										
Born between 1963-1972	0.001	0.028	0.044	0.010	0.004	0.010	0.028	-0.000	0.001	0.004
× INPRES	(0.090)	(0.059)	(0.060)	(0.011)	(0.006)	(0.025)	(0.025)	(0.005)	(0.006)	(0.006)
No. of obs.	9329	9329	9329	9140	9329	9329	9267	9063	8538	8394
Dep. var. mean	5.466	4.451	4.735	0.342	0.863	2.110	1.205	0.326	0.328	0.261
R-squared	0.55	0.49	0.48	0.43	0.51	0.42	0.44	0.47	0.42	0.42
Panel B. Men										
Born between 1963-1972	0.114	0.134	0.123	0.022	-0.003	0.037	0.047	0.005	0.008	0.011
× INPRES	(0.129)	(0.105)	(0.104)	(0.019)	(0.008)	(0.031)	(0.032)	(0.009)	(0.011)	(0.011)
No. of obs.	4526	4526	4526	4430	4526	4526	4495	4399	4156	4099
Dep. var. mean	5.405	4.397	4.679	0.331	0.853	2.099	1.213	0.331	0.331	0.260
R-squared	0.57	0.50	0.50	0.43	0.53	0.44	0.47	0.48	0.43	0.42
Panel C. Women										
Born between 1963-1972	-0.035	-0.017	0.018	-0.004	0.009	0.001	0.004	-0.008	-0.003	-0.003
× INPRES	(0.125)	(0.068)	(0.083)	(0.013)	(0.009)	(0.036)	(0.029)	(0.007)	(0.006)	(0.008)
No. of obs.	4776	4776	4776	4683	4776	4776	4744	4637	4353	4269
Dep. var. mean	5.519	4.507	4.795	0.352	0.874	2.120	1.200	0.322	0.326	0.263
R-squared	0.56	0.52	0.50	0.47	0.52	0.44	0.46	0.49	0.44	0.46

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

Table B.18: Potential mechanism: Neighborhood quality (Restricted sample)

	(1) Elementary school	(2) Middle school	(3) High school	(4) Piped water	(5) Private providers	(6) Clinic	(7) Midwives	(8) Rice subsidy	(9) National health insurance	(10) District health insurance
Panel A. All										
Born between 1963-1972	-0.065	-0.012	0.034	0.017	0.008	0.061*	0.048	0.003	-0.007	-0.007
× INPRES	(0.110)	(0.072)	(0.093)	(0.012)	(0.010)	(0.035)	(0.031)	(0.008)	(0.006)	(0.009)
No. of obs.	5118	5118	5118	5015	5118	5118	5077	4959	4672	4599
Dep. var. mean	5.514	4.421	4.670	0.354	0.861	2.092	1.216	0.323	0.329	0.266
R-squared	0.57	0.52	0.49	0.46	0.50	0.44	0.47	0.46	0.42	0.42
Panel B. Men										
Born between 1963-1972	0.117	-0.024	0.110	0.018	-0.002	0.060	0.031	0.014	0.002	0.009
× INPRES	(0.162)	(0.104)	(0.106)	(0.018)	(0.009)	(0.040)	(0.043)	(0.011)	(0.011)	(0.014)
No. of obs.	2464	2464	2464	2411	2464	2464	2442	2387	2251	2221
Dep. var. mean	5.419	4.339	4.635	0.338	0.852	2.064	1.226	0.323	0.329	0.272
R-squared	0.60	0.51	0.52	0.47	0.52	0.44	0.49	0.49	0.45	0.45
Panel C. Women										
Born between 1963-1972	-0.117	0.033	0.047	0.022	0.017	0.067	0.067*	-0.013	-0.017**	-0.019
× INPRES	(0.160)	(0.115)	(0.140)	(0.015)	(0.013)	(0.056)	(0.040)	(0.011)	(0.008)	(0.016)
No. of obs.	2617	2617	2617	2568	2617	2617	2598	2534	2383	2341
Dep. var. mean	5.596	4.493	4.702	0.365	0.869	2.117	1.211	0.324	0.328	0.259
R-squared	0.59	0.57	0.50	0.49	0.52	0.48	0.50	0.48	0.43	0.46

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

Table B.19: Potential mechanisms and their contributions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Association between	Expanded sample			Restricted sample		
	channel and outcome	INPRES effect on channel	INPRES effect on outcome	Contribution (%)	INPRES effect on channel	INPRES effect on outcome	Contribution (%)
Panel A: First generation							
Outcome: Poor health index							
Log per cap expenditures	0.016	0.046	0.04	1.840%	0.054	0.033	2.618%
Neighborhood quality:							
Health resources	0.018	0.024	0.04	1.080%	0.055	0.033	3.000%
Assortative mating for women	0.025	0.006	0.04	0.382%	0.032	0.033	2.468%

Notes: Expanded sample includes those born between 1950-1972. See Table 1 for covariates. Robust standard errors clustered at district of birth.
Significance: *** p < 0.01, ** p < 0.05, * p < 0.10.

Table B.19: Potential mechanisms and their contributions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Association between	Expanded sample			Restricted sample		
	channel and outcome	INPRES effect on channel	INPRES effect on outcome	Contribution (%)	INPRES effect on channel	INPRES effect on outcome	Contribution (%)
Panel B: Second generation							
Outcome: Poor health index							
Number of children	0.019	-0.048	-0.034	2.723%	-0.052	-0.027	3.714%
Log per cap expenditures	-0.045	0.046	-0.034	6.131%	0.054	-0.027	9.063%
Neighborhood quality:							
Health resources	-0.036	0.024	-0.034	2.568%	0.055	-0.027	7.410%
Parents' education:							
Mother	-0.071	0.03	-0.034	6.248%	0.052	-0.027	13.637%
Father	-0.049	0.025	-0.034	3.607%	0.032	-0.027	5.813%
Outcome: Height for age z-score							
Number of children	-0.072	-0.048	0.056	6.172%	-0.052	0.044	8.510%
Log per cap expenditures	0.179	0.046	0.056	14.727%	0.054	0.044	22.003%
Neighborhood quality:							
Health resources	0.079	0.024	0.056	3.370%	0.055	0.044	9.828%
Parents' education:							
Mother	0.163	0.03	0.056	8.744%	0.052	0.044	19.289%
Father	0.081	0.025	0.056	3.608%	0.032	0.044	5.877%
Outcome: Test scores							
Number of children	-0.040	-0.048	0.083	2.285%	-0.052	0.105	1.956%
Log per cap expenditures	0.117	0.046	0.083	6.484%	0.054	0.105	6.017%
Parents' education:							
Mother	0.177	0.03	0.083	6.409%	0.052	0.105	8.782%
Father	0.118	0.025	0.083	3.549%	0.032	0.105	3.591%

Notes: Expanded sample includes those born between 1950-1972. See Table 1 for covariates. Robust standard errors clustered at district of birth.
Significance: *** p < 0.01, ** p < 0.05, * p < 0.10.

C Cost-Benefit Analysis

Cost

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

Cohort size

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

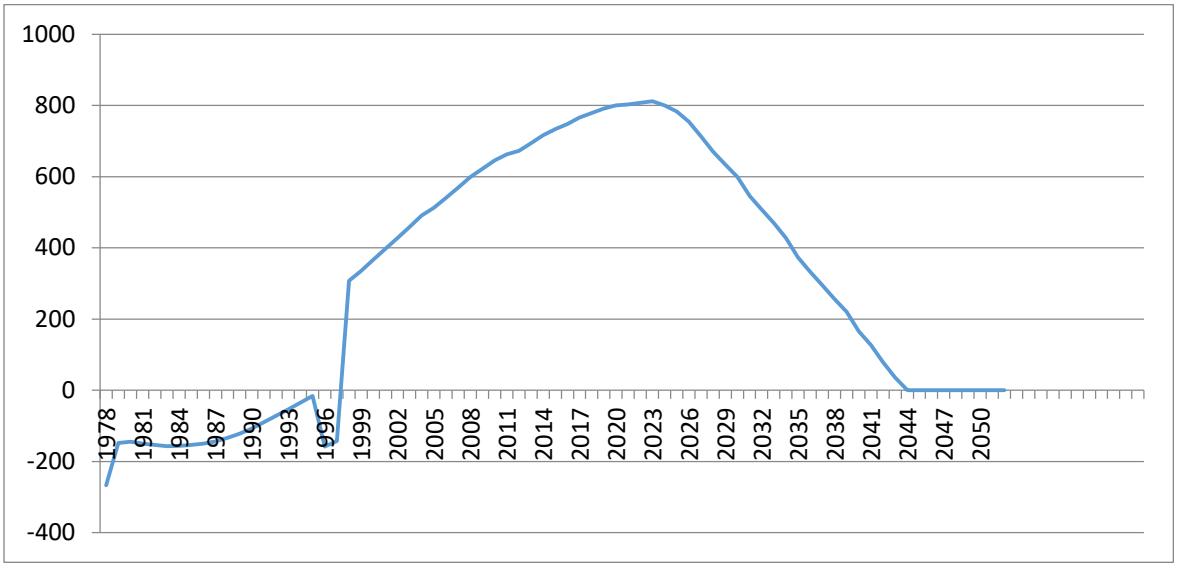
The program sought to attain a teacher student ratio of 40 students per teacher. Each school would typically hold a morning and afternoon session, with 3 classes in each session, so each school served 240 students.[2] To obtain the INPRES coverage for each year, we use the number of INPRES schools in each year divided by the number of primary school-aged children (6-12 year olds) enrolled in primary school. The INPRES exposure for each cohort is given by the average INPRES coverage for the cohort's primary schooling (6 years).

First generation benefits

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

[2] Conversations with Bappenas and former Bappenas officials.

Figure B.7: Cost Benefit Analysis



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

We include the gains from health based on the relationship between poor health and mortality at older ages. We follow the literature and assume that self-reported poor health is associated with a 2.73 odds ratio among those 50 and above in Indonesia (Frankenberg and Jones, 2004). We then combine this with mean earnings between the ages of 50 and 55 (in 1990 USD) and estimated survival probability for those ages from Statistics Indonesia.[3]

Second generation benefits

To obtain the cohort size in the second generation, we assume each first generation individual has 1.2 children at age 22.[4] We assume second generation individuals have 20% higher lifetime earnings compared to the first generation individuals and the second generation would be in the labor market between the ages of 16 and 55. The effect of INPRES on the second generation's height is 0.056 standard deviations. With about 6 centimeters standard deviation in height, this would correspond to about 0.366 centimeters height increase. With an 8% gain in earnings resulting from the height premium (Sohn, 2015), the program effect would then translate to a 0.26% gain in lifetime earnings for the second generation. For the second generation gain in education, we use literacy as a proxy for gains in test score. Following the literature, we assume a one standard deviation increase in literacy would increase earnings by 8.5% (Perez-Alvarez, 2017).

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

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

Table C.1: Internal Rate of Return Estimates

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

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

The program effect would then translate to a 0.68% gain in lifetime earnings for the second generation.

Scenarios

We present calculations based on several scenarios (Table C.1). First, we assume the gains for the first generation came from earnings only. Next, we assume the gains for the first generation came from earnings and mortality gains. We then add the gains from the second generation's test score alone, the second generation's health alone, and finally, we assume the gains from health and test scores are independent and combine the gains.