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Abstract

This paper examines whether education empowers women. We exploit an exogenous variation in education induced by a longer school year in Indonesia in 1978, which fits a fuzzy regression discontinuity design. We find education reduces the number of live births, increases contraceptive use, and promotes reproductive health practices. However, except for a few outcome measures, we do not find evidence that education improves women's decision making authority within households, asset ownership, or community participation. These results suggest that, to some extent, education does empower women in middle-income countries like Indonesia.

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Keywords: education, women's empowerment, regression discontinuity design, Southeast Asia, Indonesia

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1. INTRODUCTION

Women in developing countries suffer from gender inequalities. Countries like Yemen, Chad, and Pakistan have been ranked at the bottom of the World Economic Forum's Global Gender Gap Index. In Indonesia, for example, 96% of men are literate, but only 90% of women are; 86% of men participate in the labor market, but only 53% of women do; men earn US$ 6,903 on average, but women earn only US$ 2,985; only one in five legislators, senior officials, and managers are women; one in ten married women are 15-19 years old; maternal mortality rate may be as high as one in four hundred live births (World Economic Forum, 2013).

Gender norms that subjugate women in the developing world are one of the culprits why the gender inequalities persist (Agarwal, 1994; Sullivan, 1994). Patriarchy and traditional cultures in Asia, for example, hand more resources and power to men, which leads to women's lack of access to education, healthcare facilities, and labor markets. Perhaps the most abhorrent manifestation of these gender inequalities are what Sen (1990) terms "missing women", the shortfall of women relative to men that would have lived had they had equal access to survival-related goods.

We can empower women, the theoretical literature points out, by strengthening their threat options—resources that women can control and opportunities outside their households they can exploit (Lundberg and Pollak, 1993; McElroy and Horney, 1981; Manser and Brown, 1980). The empirical literature also supports this claim: These papers find access to resources such

as microfinance, earned income, and land rights does empower women (Pitt and Khandker, 1998; Hashemi et al., 1996; Anderson and Eswaran, 2009; Panda and Agarwal, 2005).

In this paper, we focus on the effects of education on women's empowerment. Education may increase women's bargaining power within their households because it endows them knowledge, skills, and resources to make life choices that improve their welfare (Lundberg and Pollak, 1993; Duflo, 2012). Empirical work also show education empowers women: It reduces fertility, increases contraceptives use, and reduces women's tolerance of gender discrimination (Breierova and Duflo, 2004; Osili and Long, 2008; Leon, 2004; Mocan and Cannonier, 2012).

Estimation of the effects of education on empowerment, however, is difficult because women's preferences, family background, and community characteristics that affect both education and empowerment may be unobserved (Duflo, 2012). If these unobserved characteristics correlate with education and women's empowerment, ordinary least square estimates of the effects of education will be biased. One way to solve this problem is to exploit sources of variations in education that are unrelated to women's characteristics and empowerment.

In this paper, we exploit an exogenous variation in schooling induced by a longer school year in Indonesia in 1978. Individuals who were born in 1971 or earlier experienced the longer school year in 1978 if they did not drop out of schools earlier; individuals who were born later did not. There is,

therefore, a discontinuity in the probability of experiencing the longer school year between the 1971 and 1972 cohorts, which fits a fuzzy regression discontinuity (RD) design. Parinduri (2013) shows, using this fuzzy RD design, the longer school year increases years of schooling; in this paper, we focus on women and examine whether the exogenous increase in women's education affects their empowerment.

We find education reduces the number of live births, increases contraceptive use, and promotes reproductive health practices. However, except for a few outcome measures, we do not find evidence that education improves women's decision making authority, asset ownership, or community participation.

We contribute to the literature in three respects. One, we provide the causal effects of education on women's empowerment using a natural experiment that fits an RD design, which complements papers in the literature that use instrumental variable techniques.[1] Two, we analyze Indonesia, a middle-income country, which complements papers on women's empowerment in poor countries like Bangladesh, Nepal, Nigeria, and Sierra Leone.[2] Three, we examine the effects of education on various measures of

[1] These papers use, among others, school construction programmes, compulsory schooling policies, and school entry policies as instruments; see Breierova and Duflo (2004), Osili and Long (2008), Leon (2004), and Mocan and Cannonier (2012).

[2] Panda and Agarwal (2005) analyze women's empowerment in a middle-income country, India; but Indonesia has a different cultural and social environment. We are not aware of papers that examine the effects of education on women's empowerment

empowerment such as fertility, contraceptive use, reproductive health practices, decision making authority, asset ownership, and community participation.

We proceed as follows. Section 2 describes the longer school year. Section 3 presents the empirical strategy and the data. Section 4 discusses the results and robustness checks. Section 5 concludes.

2. THE LONGER SCHOOL YEAR

The government of Indonesia implemented a longer school year in 1978 to change the start of the academic year. The academic year had run from January to December, but in 1978, to synchronize the academic year with the government budget year, the Indonesian Minister of Education and Culture, Daoed Yusuf, changed the start of the school year from January to July. To achieve this objective, he required schools to lengthen the 1978 academic year until June 1979. Therefore, children who attended schools in the 1978 academic year completed their grades not in December 1978, but in June 1979: They remained in the same grades for an extended period of six months.

Community leaders and some lawmakers opposed the change; they argued the government should not change education policies haphazardly as Daoed Yusuf and his predecessors had done. (He announced the change in

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in Indonesia except Gallaway and Bernasek (2004) who analyze correlations between literacy on women's labor force participation.

June 1978, in the middle of the 1978 academic year.) Parents associations opposed it too because, among others, they worried children had become the guinea pigs of every education ministers' desire to change education policies. Parents also protested the additional costs they had to incur because Daoed Yusuf reduced tuition fees by only 50% during the extended term, and it applied to students in public schools only (Tempo, 1978).

Despite the opposition, Daoed Yusuf went ahead and changed the start of the school year by requiring students who attended schools in 1978 to remain in the same grades until June 1979. He did not provide new teaching materials; he did not change the curriculum either. Rather, he asked teachers to revise materials that they had covered in 1978 (Tempo, 1978; MPKRI, 1978), which, in effect, makes the six-month extension in 1979 resembles a one-time longer school year.

3. EMPIRICAL STRATEGY AND DATA

(a) Empirical Strategy

We exploit an exogenous variation in years of schooling induced by a longer school year in Indonesia in 1978, which fits a regression discontinuity (RD) design, to identify the effects of education on women's empowerment.[3]

[3] Thistlethwaite and Campbell (1960) introduce this empirical strategy. See also Lee and Lemieux (2010), Imbens and Lemieux (2008), and Hahn et al. (2001). See

Because whether a woman experienced the longer school year is not a deterministic function of her year of birth, we have a fuzzy RD design. Women who were born in 1972 or later did not experience the longer school year because they had not entered primary schools in 1978 when the government implemented the longer school year; women who were born in 1971 or earlier experienced the longer school year, but only if they did not drop out of school before 1978. Therefore, conditional on the year of birth, there is a discontinuity in the probability of experiencing the longer school year between the 1971 and 1972 cohorts, which we use as an instrumental variable for the treatment status, the *longer school year*, in a fuzzy RD design.

We implement the fuzzy RD design as a system of three equations as follows. Let D_i denote the treatment status, the *longer school year*, which indicates whether woman *i* experienced the longer school year. Using an indicator, *older cohorts*, T_i, that equals one for the 1971 and older cohorts and zero otherwise as an instrumental variable for D_i, we can write the first-stage equation as

$$D_i = \alpha + \beta T_i + f(yob_i) + \varepsilon_{1i} \quad (1)$$

where $f(yob_i)$ is a polynomial function of yob_i, the year of birth of woman i. The second-stage equation-by-equation two-stage least square (2SLS) estimation of the effects of the longer school year on education is

$$edu_i = \gamma + \delta \widehat{D}_i + f(yob_i) + \varepsilon_{2i} \quad (2)$$

McCrary and Royer (2011) for a paper on the effects of female education on fertility using RD designs.

where edu_i is a measure of educational outcomes of woman *i,* and $\widehat{D}_i$ is the predicted value woman *i*'s treatment status from Equation (1). The third-stage of the equation-by-equation 2SLS estimation of the effects of education on women's empowerment is then

$$Y_i = \gamma + \theta \widehat{edu}_i + f(yob_i) + \varepsilon_{3i} \quad (3)$$

where Y_i is a measure of empowerment of woman *i,* and $\widehat{edu}_i$ is the predicted value of her educational outcome from Equation (2).

If education improves women's empowerment, we expect the coefficient of $\widehat{edu}_i$ in Equation (3) to be negative for number of live births and positive for contraceptive use, reproductive health practices, decision making authority, ownership of assets, and community participation.

(b) Data

We use the Indonesia Family Life Survey (IFLS), a longitudinal survey of a representative sample of the Indonesian population initiated by the RAND Corporation.[4] To have the largest sample of women who completed high school, we use the latest wave of the survey, IFLS-4, done in 2007. To ensure the older cohort (those born in 1971 or earlier) had some likelihood of experiencing the longer school year in 1978 and the younger cohort (those born in 1972 or later) had completed high schools when they were interviewed

[4] See Strauss et al. (2009b) for a description of the survey.

in 2007, we include women born in the period of 1960-1987, which gives us a sample size of 22,197 women.[5]

We define the *older cohort*, T_i, equals one if woman i was born in 1971 or earlier and zero otherwise. The sample consists of about 6,500 women whose T_i equals one and 15,500 women whose T_i equals zero.

We construct the *longer school year*, D_i, using the information on the year of birth of woman i, her educational attainment, and the number of times she repeated grades. In the basic specifications, D_i equals one if woman i was in primary, junior high, or senior high school in the 1978 academic year and zero otherwise. If a woman was born in 1971 or earlier and she did not drop out of school before 1978, she experienced the longer school year; but if she was born in 1972 or later, she did not experience the longer school year.[6] Therefore, women in the 1971 or older cohort have D_i equals one if they were still in school in 1978; women in the 1972 or younger cohort have D_i equals

[5] Only ever married women were asked questions on women's fertility and contraceptive use; therefore, the sample size ranges from about 3,300-10,700 women in some specifications, which depends on the measure of outcome of we use. Only currently married women were asked questions on women's decision making authority; therefore, the sample size for decision making authority ranges from about 4,300-9,300 depending on the measure of outcome.

[6] Most children in Indonesia enter primary schools in the year they are seven years old; in our basic specifications, we assume that women born in 1972 or later entered primary school in 1979 or later and, therefore, did not experience the longer school year.

zero. About 53% of women in the 1960-1971 cohorts experienced the longer school year while none of the women in the 1972-1987 cohorts did.

We use the year of birth to define the longer school year because, in developing countries like Indonesia, some people do not know their date of birth, let alone the year in which they entered primary school. In the IFLS, some people give different birthdates in different books within the same wave so that RAND has to make "best guesses" of these birthdates using an algorithm to make them as consistent as possible (Strauss et al., 2009a). However, we also use the year of entry into primary school to define the longer school year in some specifications as part of robustness checks.

We use two measures of educational outcomes: (1) the years of schooling, and (2) completion of senior high school, an indicator equals one if a woman completed senior high school and zero otherwise.

We use four groups of measures of women's empowerment: (1) women's fertility and reproductive health behavior, (2) decision making authority, (3) asset ownership, and (4) community participation. Women's fertility and reproductive health behavior include the number of live births, ideal number of children, and a set of indicators on whether a woman uses contraception, breastfed youngest child, took iron pills during pregnancy, or received tetanus injections before pregnancy.[7] Women's decision making

[7] The number of live births is the number of children a woman has given birth to in her lifetime, some of whom may have passed away; the ideal number of children is the number of children a woman would have if she could choose. Currently using

authority includes a set of indicators equal one if a woman has some say on a particular household decision (i.e., either she is the sole decision maker or joint decision maker with her spouse) and zero otherwise. Outcome measures for asset ownership include a set of indicators equal one if a woman has some ownership (i.e., either she is the sole owner or joint owner along with her spouse) of a particular asset and zero otherwise. Women's community participation equals one if a woman participated in a community or government activity in the past twelve months and zero otherwise.

Table 1 presents the summary statistics. The averages in Panel A do not show the expected effects of the longer school year on education. Compared to the 1971 or older cohort, women born in 1972 or later (those who did not experience the longer school year) have on average 1.5 additional years of education. They are also more likely to complete senior high school than the older cohort.

<Insert Table 1 here>

The averages do not show the expected effects of the longer school year on fertility and reproductive health behavior either. Women in the older cohort have more live births and desire more children (panel B); fewer women in the older cohort use contraception (panel C); more women in the older

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contraceptives is an indicator equals one if a woman at the time of the survey was using a form of contraception to prevent or postpone a pregnancy and zero otherwise.

cohort breastfeed, but fewer consume iron pills and receive tetanus injections prior to marriage (panel D).

We do not see strong evidence of the expected effects of education on decision making authority, asset ownership, or community participation. Panel E shows the older and the younger cohorts have no differences in women's decision making authority; the averages for all types of decisions are similar except for employment decisions, in which case the older cohort is more likely to have some say on the employment choices of the respondent or spouse. Panel F shows the older and the younger cohorts' asset ownership do not differ much except for poultry, livestock, vehicle, and household appliances. Panel G, however, shows women in the older cohort are more likely to participate in most types of community programs.

4. RESULTS

(a) First-stage, reduced-form and 2SLS regressions

We now discuss the first-stage regressions of the longer school year on older cohorts, the reduced-form estimates of the effects of the longer school year on education, and the corresponding 2SLS estimates of the effects of the longer school year on education.

Figure 1 illustrates the first-stage regressions of the longer school year on the year of birth. The graphs plot the proportion of women who experienced the longer school year in the 1978-1979 academic year by the

year of birth. We define the longer school year using the year of birth in panel A and using the year of entry to primary schools in panel B. Both graphs fit a cubic polynomial of the year of birth that may jump between the 1971 and 1972 cohorts.

<Insert Figure 1 here>

To the left of the vertical dash-line in panel A, the proportion of women who experienced the longer school year increases: About one in five women in the 1960 cohort to about four in five in the 1971 cohort. To the right of the vertical dash line, none of the 1972 or younger cohorts experienced the longer school year by definition. Panel B shows a similar picture: The proportion of women who experienced the longer school year, which we define using the year of entry into primary schools, drops from about 0.7-0.8 for the older cohort near the discontinuity to about 0.2 for the younger cohort. We use this discontinuity in the probability of treatment between the 1971 and 1972 cohorts as an instrumental variable for the longer school year.

Figure 2 illustrates the reduced-form estimates of the longer school year, defined using the year of birth, on educational outcomes. Panel A plots the average number of years of education by the year of birth and fits a cubic polynomial of the year of birth that may jump between the 1971 and 1972 cohorts. The figure shows educational attainment increases from about seven years in 1960 to ten years in the late 1980s, but the average educational attainment falls by about one year between the 1971 and 1972 cohorts. Panel

B shows a similar picture for the proportion of women who completed high school (i.e., twelve years of education). The trend line increases overtime but it drops between the 1971 and 1972 cohorts. The fall indicates that the longer school year increases the likelihood of a woman completing senior high school by about ten percentage points.

<Insert Figure 2 here>

Table 2 presents the estimates from the first-stage (Panel A), reduced-form (Panel B), and second-stage regressions (Panel C). Each column uses a different specification: Column 1 includes year of birth cubic polynomial as controls; column 2 adds age cubic polynomial; and column 3 adds a set of religion indicators. (Because the data fit an RD design, we do not expect additional control variables would affect the results.) In row 1, we define the longer school year using the year of birth; in row 2, using the year of entry into primary school. In Panels B and C, we define the longer school year using the year of birth, which corresponds with the first-stage regressions in row 1.

<Insert Table 2 here>

In column 1 of row 1, the older cohorts are a hundred percentage points more likely to experience the longer school year, which confirms the discontinuity we see in Figure 1. (We present bootstrap standard errors with one hundred replications in parentheses.) We find similar estimates when we include age or religion indicators as additional controls in columns 2 and 3. In row 2, using the year of entry into primary schools to define the longer school

year, the estimates are 77 percentage points. Again, these estimates confirm the discontinuity in Figure 1. All estimates in Panel A are statistically significant at the 1% level.

Panel B reports the reduced-form estimates of the effects of the longer school year, which we define using the year of birth, on educational attainment and completion of senior high school. The estimates for educational attainment and completing senior high school are 0.7 years and 13 percentage points respectively, which correspond with the jumps we see in Figure 2.

Panel C presents the corresponding 2SLS estimates of the effects of the longer school year on educational outcomes. The longer school year increases educational attainment by about 0.7 years, a large increase given the average years of schooling at the time is nine. The longer school year also increases the likelihood of completing senior high school by thirteen percentage points, a 30% increase given that 42% of women completed high schools. Because we use an RD design as the empirical strategy, as we expect, the estimates are similar across the different specifications in columns 1-3 regardless of whether we include additional control variables.

(b) Fertility and reproductive health

Figure 3 illustrates some of the reduced-form estimates of the effects of the longer school year on fertility and reproductive health practices. The trend lines in the graphs seem to jump between the 1971 and 1972 cohorts, though the jumps are less obvious in some. The number of live births, for example,

declines over time, but its trend line rises between the 1971 and 1972 cohorts. The proportion of women who use contraception increases in the 1960s but its trend line falls between 1971 and 1972. The same applies to the proportion of women who breastfeed their children and that of women who receive tetanus injections, though the fall in the former is unclear.

<Insert Figure 3 here>

The reduced-form and the 2SLS estimates in columns 1-2 of Table 3 confirm these effects: The longer school year decreases the number of live births by 0.3 and increases the likelihood that women use contraception, breastfeed their children, and receive tetanus injections by six (10%), two (3%), and nine (14%) percentage points respectively. There is no evidence that the longer school year decreases ideal number of children that the women want or increases the probability that they take iron pills: The estimate of the former is positive, but its standard error is as large as the estimate; the estimate of the latter is positive but insignificant statistically.

<Insert Table 3 here>

Column 3 shows the equation-by-equation 2SLS estimates of the effects of one more year of schooling: An additional year of education reduces the number of live births by 0.4 and increases the likelihood of using contraception, breastfeeding, and receiving tetanus injections by six (10%), three (3%), and eight (12%) percentage points, respectively. Though

educational attainment appears to increase intake of iron pills by two percentage points, the estimate is insignificant statistically.

Column 4, which presents the corresponding estimates of the effect of completing senior high school, shows the results are consistent with those in columns 2 and 3. Completing senior high school reduces number of live births by 1.9 children on average and increases the use of contraception, breast feeding, and receiving tetanus injections by 37 (60%), 16 (16%), and 37 (57%) percentage points respectively. The estimate for iron pills is positive but insignificant statistically.

(c) Household decision making authority

Table 4 presents the estimates of the effects of education on women's household decision making authority. Each panel represents a different category of decisions: Panel A is about decisions on household expenditure, Panel B children's welfare, Panel C household savings, and Panels D and E whether a respondent or spouse should work or use of contraceptives, respectively.

<Insert Table 4 here>

The reduced-form and 2SLS estimates in columns 1 and 2 show the longer school year increases the likelihood that women have some say on routine purchases, children's education and health, monthly savings, employment, and contraceptive use. However, only the estimate for monthly

savings is significant statistically (four percentage points or 5%). (Figure 4 illustrates some of the reduced-form estimates.) The estimates for food eaten at home, children's clothing, and money for *arisan*—a form of rotating savings and credit association—are negative, but only that of money for *arisan* is significant statistically; the longer school year reduces the likelihood that women have a say on *arisan* by four percentage points (4%).[8]

<Insert Figure 4 here>

The equation-by-equation 2SLS estimates in columns 3 and 4 show no evidence that education improves women's decision making authority on expenditure, children's outcomes, employment, and contraceptive use; it affects decision making on household savings, however. An extra year of education increases the likelihood of having a say on monthly savings by five percentage points (6%); completion of senior high school increases the likelihood by 22 percentage points (26%). Furthermore, educational attainment reduces decision making authority on *arisan* money by seven percentage points (7%); completing twelve years of education reduces it by 26 percentage points (28%). The other estimates are insignificant statistically; the standard errors are as large as the estimates.

[8] *Arisan* is one of the oldest and most widespread form of rural financial institutions in Indonesia (Hospes, 1996).

(d) Asset ownership

Table 5, which presents the effects of education on asset ownership, shows the longer school year does not seem to affect ownership of land, poultry, livestock, vehicles, savings, and receivables. (The estimates are insignificant statistically; the estimate for vehicles is significant only at the 10% level.) There is, however, some evidence that education affects ownership of household appliances and jewelry: The reduced-form and 2SLS estimates in columns 1 and 2 indicate that the longer school year increases the likelihood of owning household appliances by about three percentage points (3%) and decreases the likelihood of owning jewelry by about two percentage points (2%). The estimates of the effect of education in columns 3 and 4 show an extra year of education and completing senior high school increases the likelihood of owning household appliances by five (5%) and 20 (22%) percentage points, respectively, and reduces the likelihood of owning jewelry by two (2%) and nine (9%) percentage points, respectively. All other estimates are insignificant statistically.

<Insert Table 5 here>

(e) Community participation

Table 6, which presents the effects of education on community participation, shows no evidence that education improves community participation for monthly *arisan* meetings, community meetings, participating in village

cooperatives, programs to improve the village, voluntary labor, village loans and savings programs, health fund, and women's association activities; all estimates are insignificant statistically at conventional level of significance. The longer school year, however increases the likelihood of a woman participating in *Posyandu* or the community weighing posts—community centers that the government of Indonesia sets up to provide pre- and postnatal healthcare for women and infants—by about six percentage points (16 percent).

<Insert Table 6 here>

(f) Robustness checks

We do a number of robustness checks: (1) we include alternative polynomial functions of the assignment variable and additional control variables, (2) we use alternative assignment variables and definitions of the longer school year, and (3) we do some falsification tests.

Table 7 presents the effects of education on key outcome measures using additional controls and alternative polynomial functions of the assignment variable. Columns 1 and 5 include year of birth quadratic polynomial; columns 2 and 6 year of birth quartic polynomial; columns 3 and 7 age cubic polynomial; and columns 4 and 8 both age cubic polynomial and religion indicators. Overall the results are robust; both the signs and magnitude of the estimates are similar to those in the basic results.

<Insert Table 7 here>

Table 8 presents the effects of education using alternative assignment variables and different definitions of the longer school year. Columns 1 and 3 use the year of birth as the assignment variable and define the longer school year using the year of entry into primary schools; columns 2 and 4 use the year of entry as the assignment variable and define the longer school year using the year of entry. Overall, the results are robust except for a few cases in which we use the year of entry into primary schools as the assignment variable. Some of the estimates in columns 2 and 4 are insignificant statistically, which may be caused by measurement errors in the year of entry to primary schools we describe in the data section. Nevertheless, the signs and the magnitude of the estimates are similar to those in the basic results.

<Insert Table 8 here>

Table 9 presents some falsification tests to see whether there are other discontinuities between the 1971 and 1972 cohorts. We consider the age of women, whether they were born in rural areas, whether they lived in rural areas when they were twelve years old, whether their biological parents were married when they were twelve years old, and whether their biological parents are currently living in the same household. In column 1, we define the longer school year using the year of birth; in column 2 using the year of entry to primary schools.

<Insert Table 9 here>

All estimates are insignificant statistically at conventional level of significance; we do not find evidence that there are discontinuities in these variables between the 1971 and 1972 cohorts that may compromise identification using the RD design.

5. CONCLUDING REMARKS

Education reduces women's fertility, increases contraceptive use, and promotes reproductive health practices. An additional year of schooling reduces women's number of live births by 0.4 on average; it increases women's likelihood of using contraception, breastfeeding children, and receiving tetanus injections by 10, 3, and 12%, respectively. Completing senior high school reduces the number of live births by two children and increases the likelihood of using contraception, breastfeeding children, and receiving tetanus injections by 60, 17, and 57%, respectively.

There is no evidence that education improves women's decision making authority (except on savings), women's assets ownership (except that of household appliances and jewelry), or community participation (except visiting the community weighing post), at least along the measures that we examine in this paper. In any case, most women in Indonesia have some say on expenditure and children's decisions and almost all own houses or jewelry (see Panel F of Table 1), which perhaps drives the insignificant results. Most women do not participate in community activities, in particular women in the

younger cohorts who are more educated on average (see Panel G of Table 1). Therefore, it may be difficult to identify the effects of education on women's decision making authority, asset ownership, or community participation in Indonesia using the measures that we have in the IFLS even if education matters. Among the significant results, one more year of schooling increases the likelihood that women have a say on monthly savings by 6% and reduces the likelihood that they have decision making authority on *arisan* money by 7%. Education also increases ownership of household appliances by 5% and reduces ownership of jewelry by 2%. Education gives women some say on savings, including on moving away from *arisan* as means of saving. There is no evidence that education increases women's ownership of savings, however.

These findings are in line with the bargaining theory of Lundberg and Pollak (1993), Manser and Brown (1980), and McElroy and Horney (1981). Education is a threat option that increases women's bargaining power within households; it endows women with knowledge, power, and resources to make life choices that improve their welfare. More educated women have fewer children, use contraception, have better reproductive health practices, and have some say on household decision making—education empowers women to choose the best for themselves and to bargain with their husbands on how to allocate resources within their households.

Our results are in line with the empirical literature on the effects of education on women's empowerment; they also sit within the broader empirical literature on how women's threat options empower women. Mocan

and Cannonier (2012), for example, find education improves Sierra Leonean women's attitudes towards women's health and domestic violence, reduces their number of desired children, and increases their likelihood of using contraceptives and getting tested for AIDS; Breierova and Duflo (2004) and Osili and Long (2008) also find education reduces women's fertility in Indonesia and Nigeria, respectively. On women's threat option literature, Panda and Agarwal (2005) find ownership of land reduces risk of marital violence in India; Hashemi et al. (1996) find access to microfinance increases women's mobility, decision making authority, ownership of productive assets, and awareness and participation in public campaigns and protests in Bangladesh.

Our findings imply publicly funded education (the use of taxpayers' money and government resources to finance public schools) in middle-income countries like Indonesia has higher rates of returns than previous estimates in the literature because education not only produces skilled workers and informed voters, but also empowers women. Public education may increase contraceptive use (which will limit unwanted pregnancies), reduce fertility rates (with better family planning), and promote women's health practices. As women become more educated, their children may also do better because the women, among others, have their children breastfed and immunized, which reduces child malnutrition and mortality rates.[9] Moreover, women will have

[9] Indonesia, for example, 28% of children below the age of five are underweight; 45% of them are malnourished (WHO, 2012).

more say on how to allocate resources within their households, which may funnel more resources to children's health and education.[10]

In this paper, we do not explore the mechanisms through which education empower women; we do not examine whether education affects other aspects of women's welfare such as domestic violence or freedom of movement. These questions could be perhaps explored in future research.

[10] Thomas (1994), for example, finds finances controlled by women improve children's health.

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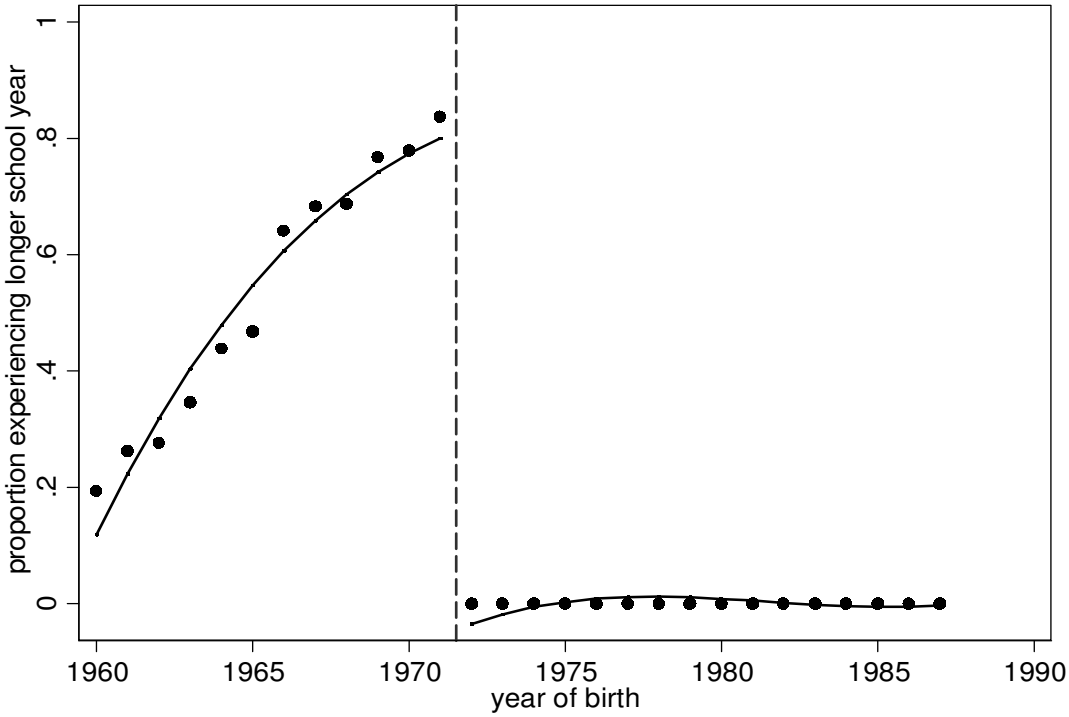
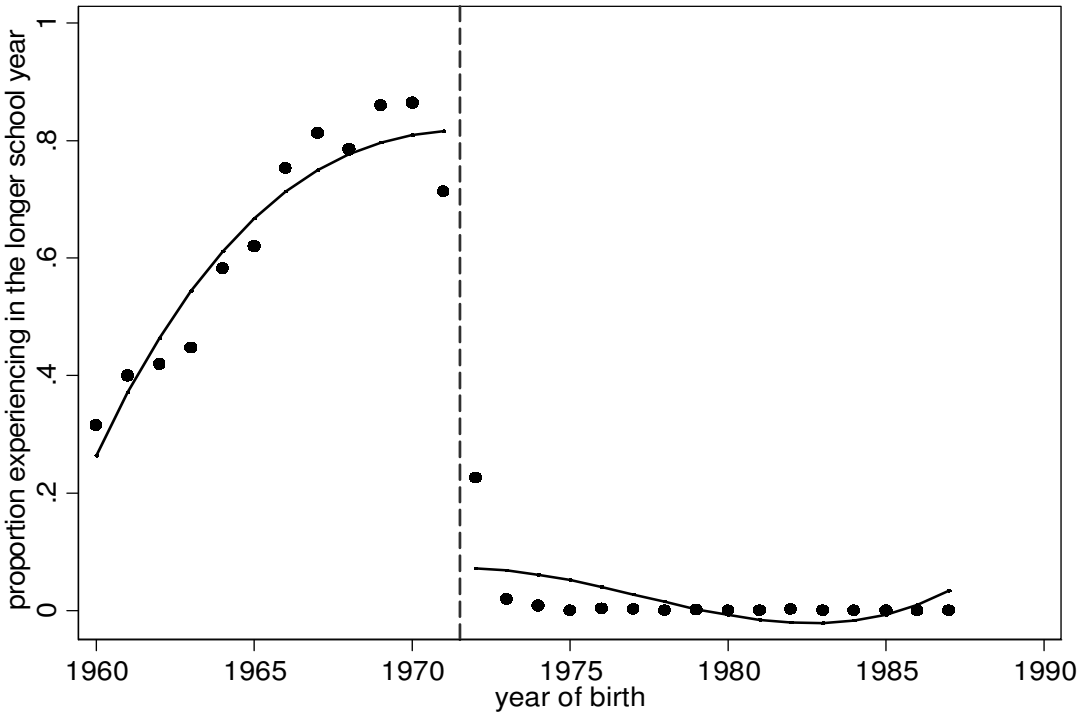
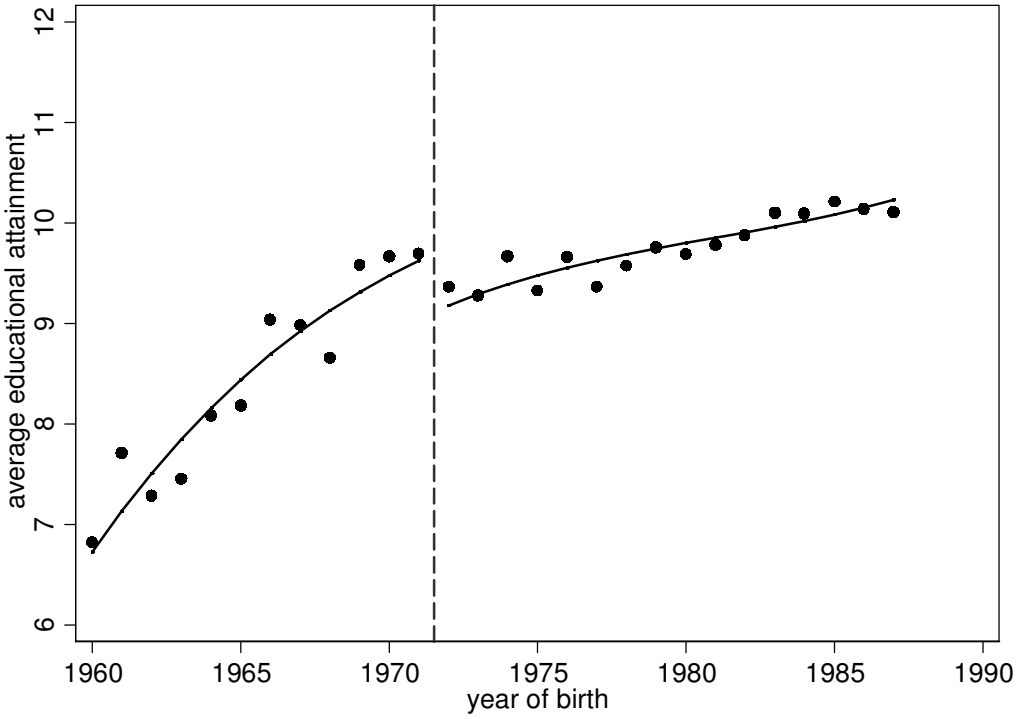


A. Using the year of birth to define the longer school year

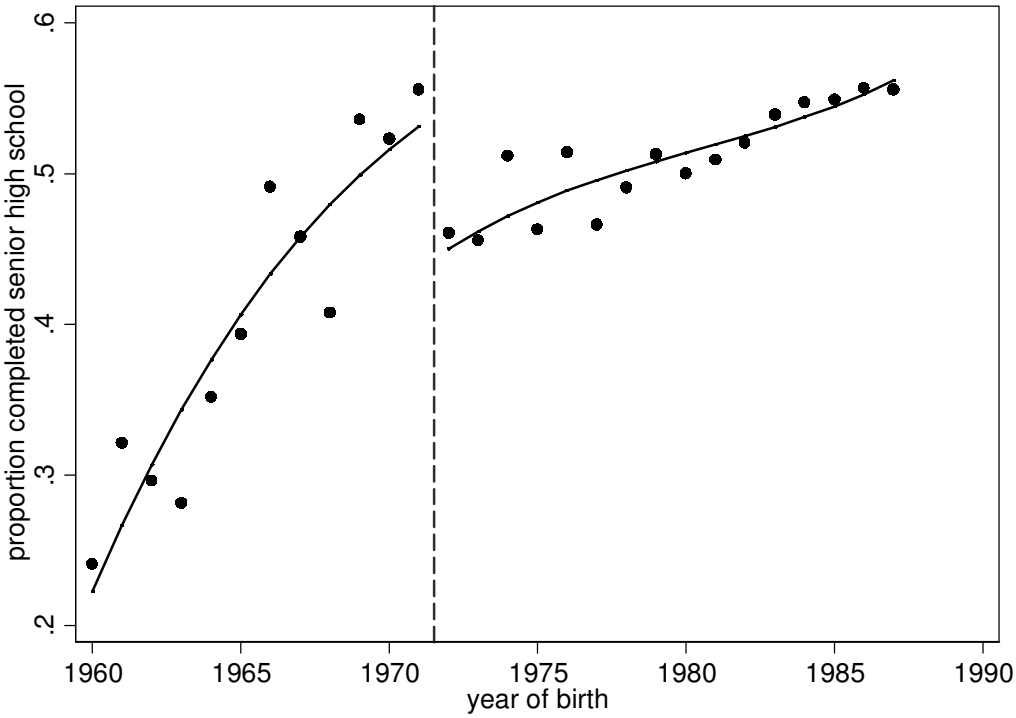


B. Using year of the entry into primary schools to define the longer school year

Figure 1: The first-stage regressions

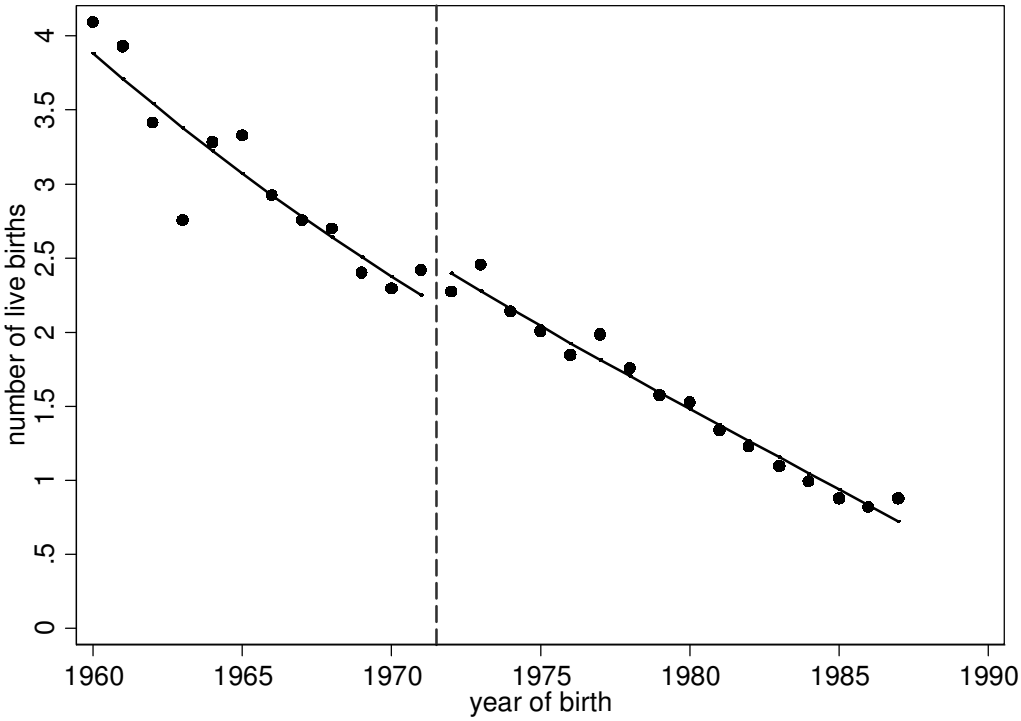


A. Educational attainment

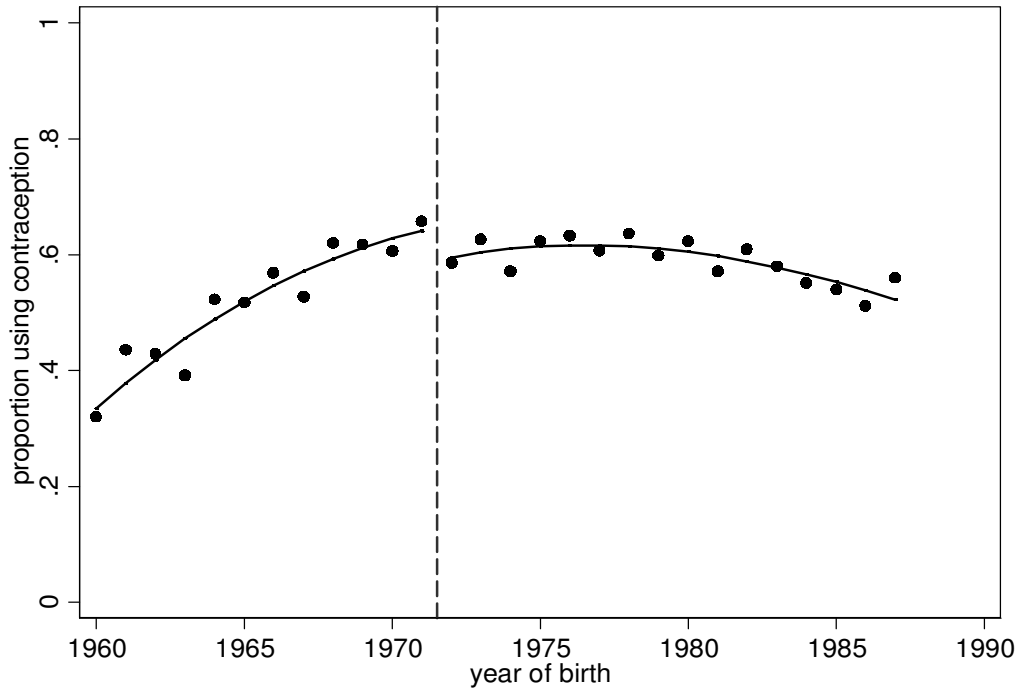


B. Completed twelve years of education

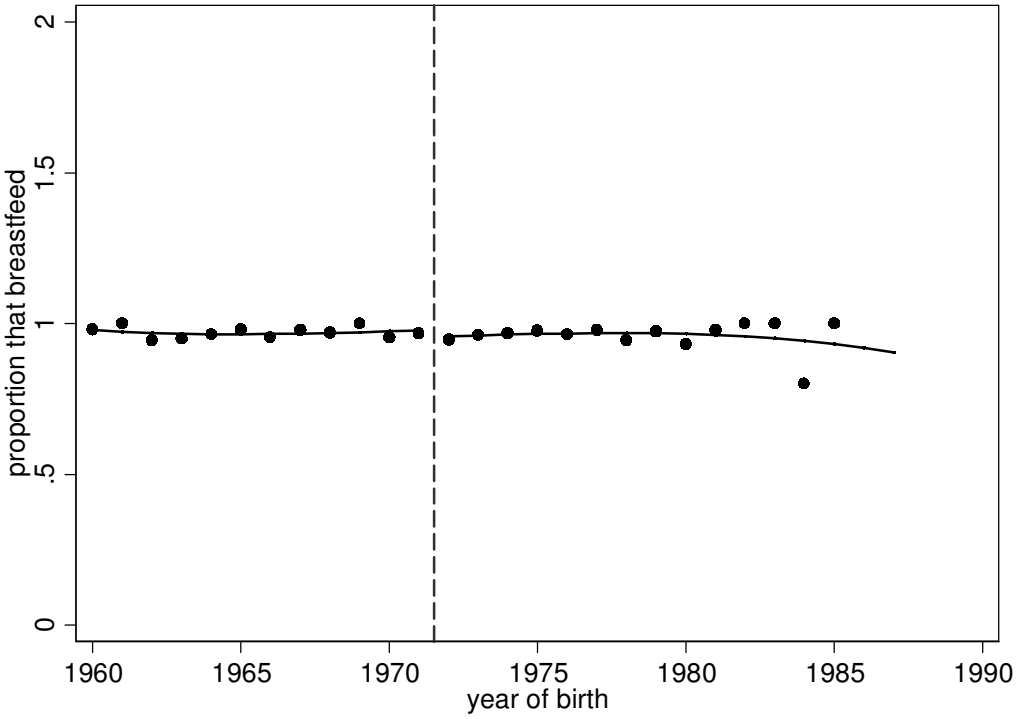
Figure 2: The effects on education



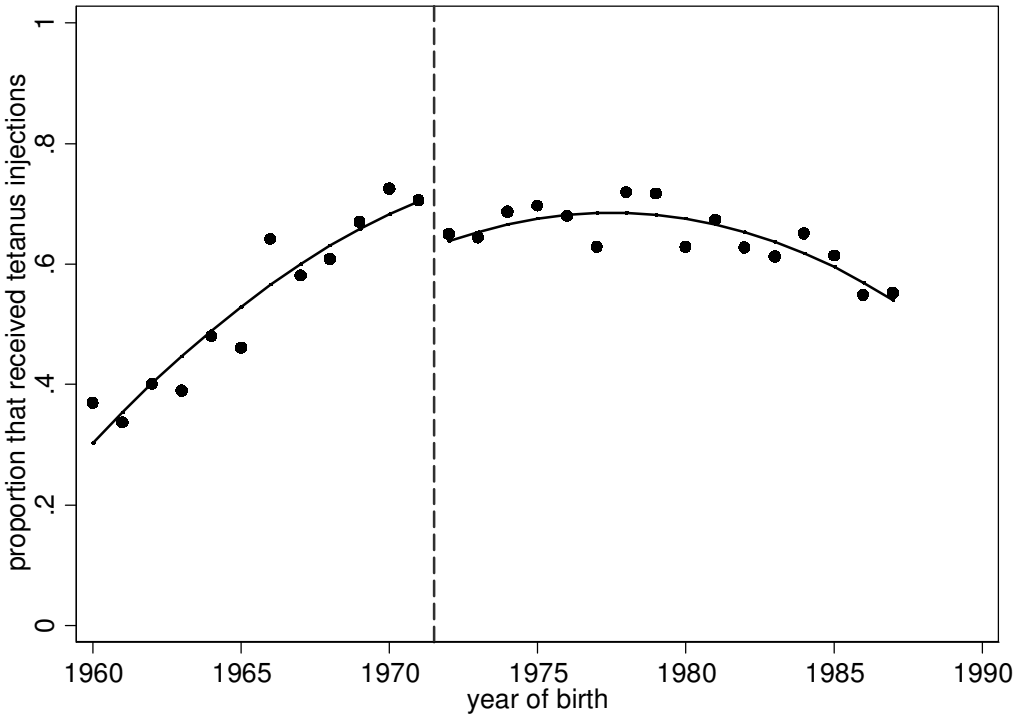
A. The number of live births



B. The proportion of women using contraception

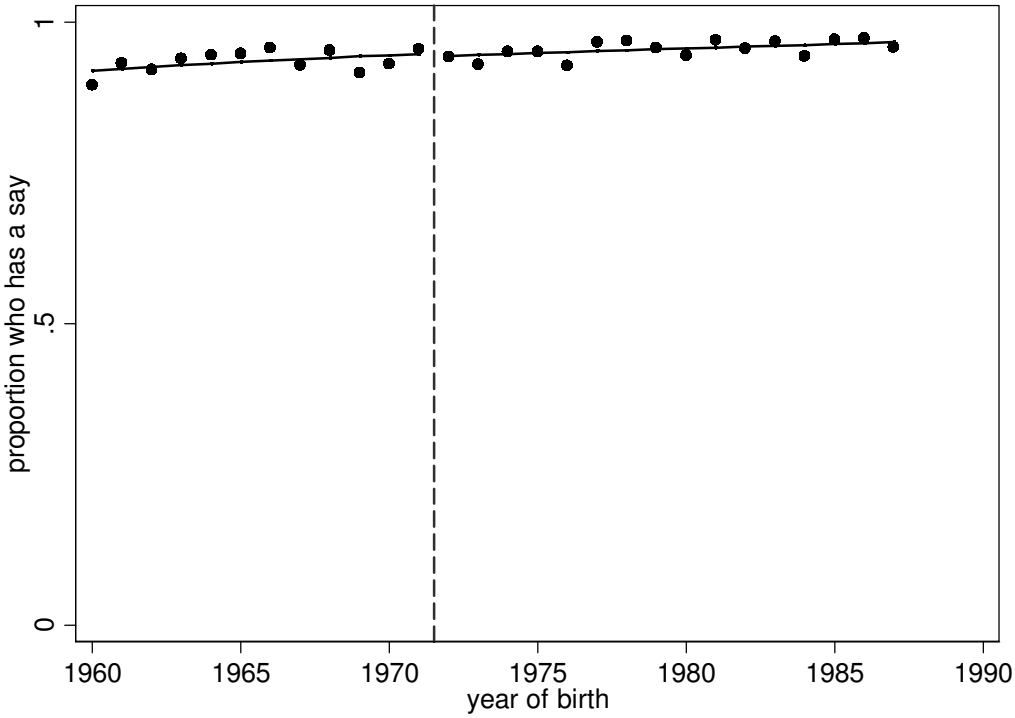


C. The proportion of women that breastfeed

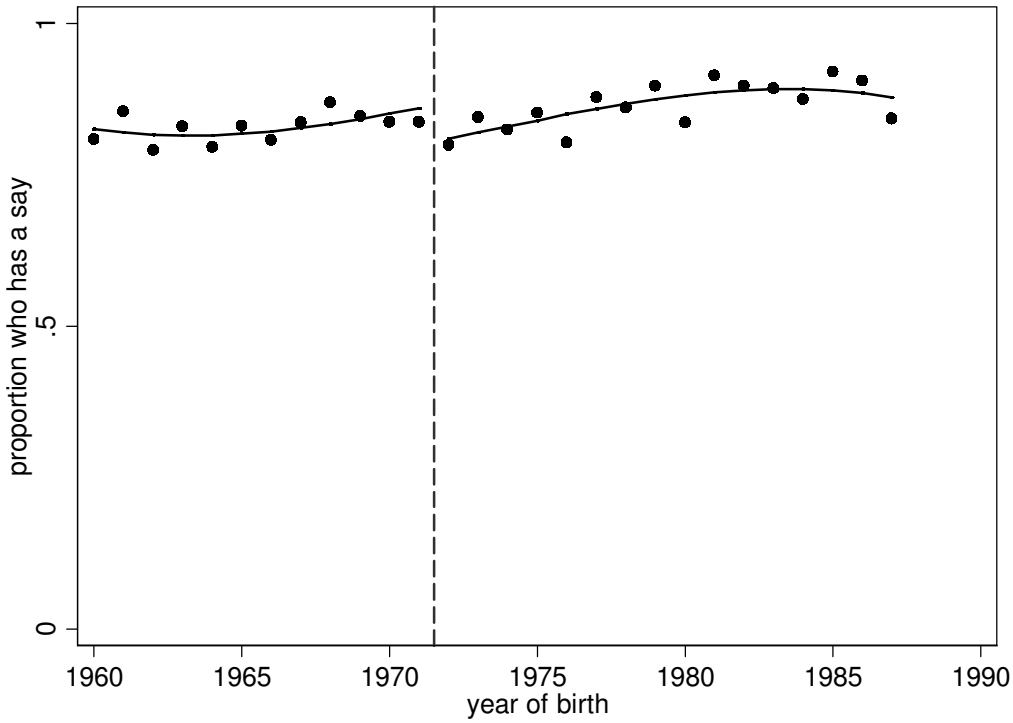


D. The proportion of women that received tetanus injections

Figure 3: The effects on fertility and reproductive health behavior



A. The proportion of women who has a say on children's health decisions



B. The proportion of women who has a say on monthly savings

Figure 4: The effects on decision making authority

Table 1: Summary statistics

Variable	1960-1971 cohort (1)	1972-1987 cohort (2)	1960-1987 cohort (3)
A: Educational outcomes			
Educational attainment	8.017	9.514	9.068
	(3.728)	(2.860)	(3.217)
Completed senior high school	0.341	0.459	0.422
	(0.474)	(0.498)	(0.494)
B: Fertility outcomes			
Number of live births	2.865	1.546	1.849
	(2.455)	(1.041)	(1.590)
Ideal number of children	3.039	2.560	2.713
	(1.695)	(1.103)	(1.341)
C: Contraceptive use			
Currently using contraception	0.579	0.611	0.601
	(0.493)	(0.487)	(0.489)
D: Health practices			
Breastfed child	0.970	0.964	0.967
	(0.170)	(0.185)	(0.178)
Took iron pills	0.073	0.126	0.108
	(0.259)	(0.331)	(0.310)
Received tetanus injection	0.558	0.653	0.623
	(0.496)	(0.476)	(0.484)
E: Household decision making authority			
Expenditure			
On food eaten at home	0.915	0.921	0.919
	(0.277)	(0.268)	(0.271)
On routine purchases	0.938	0.935	0.936
	(0.240)	(0.246)	(0.243)
On large expensive purchases	0.902	0.902	0.902
	(0.269)	(0.296)	(0.296)
Children			
On clothes	0.955	0.962	0.960
	(0.205)	(0.188)	(0.194)
On education	0.955	0.965	0.962
	(0.206)	(0.182)	(0.190)
On health	0.971	0.972	0.971
	(0.167)	(0.164)	(0.165)

Table 1: Summary statistics (continued)

	1960-1971 cohort (1)	1972-1987 cohort (2)	1960-1987 cohort (3)
Savings			
On monthly savings	0.857	0.856	0.856
	(0.349)	(0.350)	(0.350)
On money for *arisan*	0.919	0.932	0.928
	(0.272)	(0.250)	(0.258)
Others			
On employment of respondent or spouse	0.840	0.770	0.793
	(0.366)	(0.420)	(0.404)
On contraceptive use by respondent or spouse	0.969	0.971	0.970
	(0.170)	(0.168)	(0.169)
F: Asset ownership			
House (including land)	0.981	0.966	0.974
	(0.134)	(0.180)	(0.159)
Poultry	0.838	0.792	0.813
	(0.368)	(0.405)	(0.389)
Livestock	0.771	0.831	0.806
	(0.420)	(0.374)	(0.395)
Vehicle	0.787	0.713	0.738
	(0.409)	(0.452)	(0.439)
Household appliances	0.966	0.922	0.938
	(0.180)	(0.267)	(0.241)
Savings	0.857	0.856	0.857
	(0.349)	(0.350)	(0.350)
Receivables	0.878	0.856	0.864
	(0.327)	(0.350)	(0.341)
Jewelry	0.959	0.979	0.973
	(0.196)	(0.142)	(0.160)

Table 1: Summary statistics (continued)

	1960-1971 cohort (1)	1972-1987 cohort (2)	1960-1987 cohort (3)
G: Community Participation			
Arisan	0.452	0.362	0.389
	(0.497)	(0.480)	(0.487)
Community meeting	0.272	0.146	0.186
	(0.445)	(0.353)	(0.389)
Village cooperative	0.163	0.089	0.113
	(0.369)	(0.285)	(0.317)
Programme to improve the village	0.211	0.155	0.172
	(0.408)	(0.362)	(0.378)
Voluntary labor	0.271	0.218	0.235
	(0.445)	(0.413)	(0.424)
Village savings and loans	0.163	0.095	0.117
	(0.369)	(0.293)	(0.321)
Health fund	0.658	0.493	0.548
	(0.474)	(0.500)	(0.497)
Women's association activities	0.285	0.146	0.190
	(0.451)	(0.353)	(0.392)
Community weighing post	0.209	0.373	0.324
	(0.406)	(0.483)	(0.468)

Notes: The number in each cell is the mean; the standard deviations are in parentheses. The number of women who did not experience the longer school year in column 1 are 2000-8000 (Panel B), 7000-8000 (Panel C), 3400-7200 (Panel D), 300-3900 (Panel E), 2300-9300 (Panel F); and 2000-8000 (Panel G). The number of women who experienced the school year in column 2 are 1700-4700 (Panel B), 3900-4400 (Panel C), 1800-3700 (Panel D), 300-2300 (Panel E), 1200-4600 (Panel F), and 1200-4100 (Panel G).

Table 2: First-stage, reduced-form, and second-stage regressions

		(1)	(2)	(3)
A: First-stage regressions				
Using the year of birth to define the longer school year				
Older cohorts	(1)	1.049***	1.049***	1.050***
		(0.008)	(0.008)	(0.009)
Adjusted R^2		0.737	0.738	0.738
Number of observations		17427	17427	13247
Using the year of entry to define the longer school year				
Older cohorts	(2)	0.777***	0.777***	0.767***
		(0.009)	(0.009)	(0.011)
Adjusted R^2		0.579	0.579	0.560
Number of observations		17326	17326	13150
B: Reduced-form				
Educational attainment				
Older cohorts	(3)	0.703***	0.702***	0.682**
		(0.121)	(0.121)	(0.138)
Completed senior high school				
Older cohorts	(4)	0.136***	0.136***	0.135***
		(0.018)	(0.018)	(0.020)
C: 2SLS				
Educational attainment				
Longer school year	(5)	0.670***	0.670***	0.650**
		(0.115)	(0.113)	(0.128)
Completed senior high school				
Longer school year	(6)	0.134***	0.134***	0.132***
		(0.018)	(0.018)	(0.019)
Controls				
Year of birth cubic polynomial		✓	✓	✓
Age cubic polynomial			✓	✓
Religion indicators				✓

Notes: In Panel A, the number in each cell is the estimate of older cohorts from a regression of longer school year on older cohorts and a set of control variables. In row 1, the longer school year equals one if a woman was born in 1971 or earlier and was still in school in 1978, zero otherwise; in row 2, the longer school year equals one if a woman entered primary school in 1978 or earlier and was in school in 1978. In Panel B, the number in each cell is the reduced-form estimate of the longer school year defined using the year of birth. Panel C reports the corresponding 2SLS estimates. The numbers in parentheses are bootstrap standard errors with 100 replications. The asteriks ***, **, and * indicate statistical significance at 1, 5, and 10%, respectively.

Table 3: The effects on fertility and reproductive health behavior

	Reduced-form	The effects of		
		Longer school year	Educational attainment	Completing high school
	(1)	(2)	(3)	(4)
A: Number of children				
Number of live births	-0.264***	-0.318***	-0.406**	-1.977***
	(0.067)	(0.0713)	(0.105)	(0.429)
Ideal number of children	0.056	0.066	-0.036	-0.094
	(0.057)	(0.060)	(0.076)	(0.371)
B: Contraceptive Use				
Currently using contraception	0.055***	0.063***	0.058**	0.372**
	(0.018)	(0.021)	(0.036)	(0.165)
C: Health practices				
Breastfeed child	0.033**	0.018*	0.034**	0.160**
	(0.010)	(0.012)	(0.015)	(0.070)
Took iron pills	0.014	0.016	0.019	0.108
	(0.012)	(0.013)	(0.016)	(0.083)
Received tetanus injection	0.085**	0.098***	0.078**	0.372**
	(0.017)	(0.021)	(0.035)	(0.146)

Notes: The number in each cell in column 1 is the estimate of older cohorts in a regression of fertility or reproductive health behavior on older cohorts and year of birth cubic polynomial. Each cell in column 2 is the corresponding 2SLS estimate. Columns 3 and 4 present the equation-by-equation 2SLS estimates of the effects of educational attainment or completion of senior high school on fertility and reproductive health behavior, respectively. Bootstrap standard errors with 100 replications are in parentheses. The asteriks ***, **, and * indicate statistical significance at 1, 5, and 10%, respectively.

Table 4: The effects on decision making authority

	Reduced-form	The effects of		
		Longer school year	Educational attainment	Completing high school
	(1)	(2)	(3)	(4)
A: Expenditure				
Food eaten at home	-0.005	-0.005	-0.017	-0.088
	(0.011)	(0.013)	(0.016)	(0.080)
Routine purchases	0.012	0.014	0.008	0.047
	(0.010)	(0.011)	(0.015)	(0.073)
Large expensive purchases	0.001	0.001	0.001	-0.022
	(0.012)	(0.014)	(0.017)	(0.087)
B: Children				
Clothes	-0.007	-0.008	-0.012	-0.066
	(0.008)	(0.009)	(0.013)	(0.063)
Education	0.006	0.007	0.018	0.068
	(0.007)	(0.009)	(0.012)	(0.060)
Health	0.012*	0.013*	0.019*	0.067
	(0.006)	(0.007)	(0.011)	(0.052)
C: Savings				
Monthly savings	0.034**	0.040***	0.050***	0.220***
	(0.010)	(0.012)	(0.019)	(0.071)
Money for *arisan*	-0.033**	-0.041**	-0.073**	-0.256**
	(0.020)	(0.017)	(0.034)	(0.112)
D: Employment of respondent or spouse	0.001	0.001	-0.034	-0.138
	(0.016)	(0.018)	(0.025)	(0.125)
E: Contraceptive use by respondent or spouse	0.012	0.014	0.015	0.059
	(0.007)	(0.008)	(0.011)	(0.050)

Notes: The number in each cell in column 1 is the estimate of older cohorts in a regression of decision making authority on older cohorts and year of birth cubic polynomial. Each cell in column 2 is the corresponding 2SLS estimate. Columns 3 and 4 present the equation-by-equation 2SLS estimates of the effect of educational attainment and completion of senior high school on decision making authority, respectively. Bootstrap standard errors with 100 replications are in parentheses. The asteriks ***, **, and * indicate statistical significance at 1, 5, and 10%, respectively.

Table 5: The effects on ownership of assets

	Reduced-form	The effects of		
		Longer school year	Educational attainment	Completing high school
	(1)	(2)	(3)	(4)
House and land	0.016	0.018	0.014	0.057
	(0.009)	(0.011)	(0.013)	(0.067)
Poultry	0.027	0.031	0.148	-0.257
	(0.039)	(0.044)	(0.294)	(20.24)
Livestock	-0.048	-0.060	0.488	-0.511
	(0.068)	(0.084)	(1.880)	(0.697)
Vehicles	0.046*	0.054*	0.052	0.200
	(0.034)	(0.029)	(0.046)	(0.175)
Household appliances	0.028**	0.032**	0.045**	0.198**
	(0.012)	(0.013)	(0.021)	(0.089)
Savings	-0.024	-0.033	0.022	0.053
	(0.041)	(0.041)	(0.063)	(0.183)
Receivables	-0.014	-0.014	0.004	0.016
	(0.027)	(0.055)	(0.030)	(0.205)
Jewelry	-0.026**	-0.014*	-0.021**	-0.093**
	(0.010)	(0.011)	(0.010)	(0.046)

Notes: The number in each cell in column 1 is the estimate of older cohorts in a regression of ownership of assets on older cohorts and year of birth cubic polynomial. Each cell in column 2 is the corresponding 2SLS estimate. Columns 3 and 4 present the equation-by-equation 2SLS estimates of the effects of educational attainment and completion of senior high school on ownership of assets, respectively. Bootstrap standard errors with 100 replications are in parentheses. The asteriks ***, **, and * indicate statistical significance at 1, 5, and 10%, respectively.

Table 6: The effect on community participation

	Reduced-form	The effects of		
		Longer school year	Educational attainment	Completing high school
	(1)	(2)	(3)	(4)
Monthly *arisan*	0.033*	0.038*	0.042	0.213
	(0.023)	(0.019)	(0.026)	(0.134)
Community meeting	0.018	0.021	0.001	0.013
	(0.016)	(0.019)	(0.018)	(0.106)
Village cooperative	-0.035	-0.041	-0.040	-0.268
	(0.028)	(0.025)	(0.022)	(0.150)
Programme to improve the village	-0.004	-0.005	-0.026	-0.132
	(0.019)	(0.023)	(0.021)	(0.106)
Voluntary labor	0.003	0.004	-0.008	-0.041
	(0.029)	(0.025)	(0.024)	(0.125)
Village savings and loans	0.032	0.037	0.163	0.663*
	(0.037)	(0.030)	(0.111)	(0.381)
Health fund	0.061	0.083	0.078	0.270
	(0.053)	(0.074)	(0.169)	(0.387)
Women's association activities	-0.002	-0.003	-0.001	-0.027
	(0.023)	(0.019)	(0.027)	(0.145)
Community weighing post	0.050***	0.058***	0.111***	0.539***
	(0.023)	(0.020)	(0.036)	(0.170)

Notes: The number in each cell in column 1 is the estimate of older cohorts in a regression of community participation on older cohorts and year of birth cubic polynomial. Each cell in column 2 is the corresponding 2SLS estimate. Columns 3 and 4 present the equation-by-equation 2SLS estimates of the effects of educational attainment and completion of senior high school on political or community participation, respectively. Bootstrap standard errors with 100 replications are in parentheses. The asteriks ***, **, and * indicate statistical significance at 1, 5, and 10%, respectively.

Table 7: Using additional control variables and alternative polynomial functions of the assignment variable

	Effects of educational attainment				Effects of completing senior high school			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of live births	-0.400*** (0.0750)	-0.268*** (0.101)	-0.405** (0.103)	-0.369*** (0.109)	-2.313*** (0.414)	-1.089** (0.469)	-1.969** (0.493)	-1.759*** (0.515)
Received Tetanus Injection	0.075*** (0.022)	0.119*** (0.032)	0.075** (0.027)	0.079** (0.041)	0.051** (0.129)	0.624** (0.158)	0.382** (0.141)	0.392** (0.154)
Currently using Contraception	0.069*** (0.024)	0.058** (0.029)	0.069** (0.028)	0.067** (0.032)	0.512** (0.142)	0.371** (0.153)	0.434** (0.151)	0.422** (0.163)
Breastfeed child	0.031** (0.015)	0.035*** (0.013)	0.034** (0.013)	0.039** (0.016)	0.175* (0.093)	0.166** (0.070)	0.163** (0.071)	0.181** (0.080)
Decision making on monthly savings	0.021 (0.014)	0.042** (0.020)	0.051** (0.019)	0.046** (0.020)	0.099* (0.057)	0.162** (0.069)	0.222*** (0.072)	0.213** (0.082)
Household appliances	0.027** (0.013)	0.057** (0.024)	0.046** (0.012)	0.043** (0.020)	0.143* (0.074)	0.262** (0.109)	0.201** (0.089)	0.201** (0.092)
Controls								
Year of birth quadratic polynomial	✓				✓			
Year of birth cubic polynomial			✓	✓			✓	✓
Year of birth quartic polynomial		✓				✓		
Age cubic polynomial			✓	✓			✓	✓
Religion indicators				✓				✓

Notes: The number in each cell is the equation-by-equation 2SLS estimate of the effect of educational attainment or completion of senior high school. Bootstrap standard errors with 100 replications are in parentheses. The asteriks ***, **, and * indicate statistical significance at 1, 5, and 10%, respectively.

Table 8: Using alternative assignment variables and definitions of the longer school year

	Effects of educational attainment		Effects of completing senior high school	
Dependent variable	Assignment variable: year of birth Longer school year: using year of entry (1)	Assignment variable: year of entry Longer school year: using year of entry (2)	Assignment variable: year of birth Longer school year: using year of entry (3)	Assignment variable: year of entry Longer school year: using year of entry (4)
Number of live births	-0.390*** (0.098)	-0.545*** (0.153)	-1.934*** (0.479)	-3.157*** (0.883)
Received Tetanus Injection	0.075** (0.027)	0.085*** (0.028)	0.383** (0.139)	0.671** (0.243)
Currently using Contraception	0.058** (0.029)	0.032 (0.027)	0.375** (0.152)	0.238 (0.245)
Breastfeed child	0.034** (0.014)	0.034 (0.022)	0.150** (0.075)	0.299 (0.216)
Decision making on monthly savings	0.049* (0.019)	0.051** (0.021)	0.219*** (0.071)	0.338** (0.138)
Household appliances	0.045** (0.021)	0.035** (0.018)	0.198** (0.089)	0.212** (0.108)

Notes: The number in each cell is the equation-by-equation 2SLS estimate of the effect of educational attainment (column 1 and 2) or completion of senior high school (columns 3 and 4). Each regression includes year of birth cubic polynomial. Bootstrap standard errors with 100 replications are in parentheses. The asteriks ***, **, and * indicate statistical significance at 1, 5, and 10%, respectively.

Table 9: Falsification tests

Dependent variable	(1)	(2)
Age	-0.081	-0.020
	(0.020)	(0.023)
Born in rural area	0.083	0.096*
	(0.043)	(0.050)
Lived in rural area when twelve years old	0.029	0.036
	(0.044)	(0.049)
When twelve years old biological parents were married	-0.045	-0.052*
	(0.025)	(0.028)
Biological parents live in household	-0.029	-0.035
	(0.018)	(0.021)
Variable used to define longer school year		
Year of birth	✓	
Year of entry		✓

Notes: The number in each cell is the 2SLS estimate of the longer school year, which is defined using year of birth or year of entry. Each regression includes the year of birth cubic polynomial. Bootstrap standard errors with 100 replications are in parentheses. The asterik * indicates statistical significance at 10% level.