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Education reforms, sibling spillovers, and fertility

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Résumé

Dans de nombreux pays, des réformes de l'éducation primaire ont été implanté dans le but d'améliorer l'accès et les taux d'inscriptions aux écoles. Nous risquons de sous-estimer l'impact de ces réformes si nous ne considérons pas les retombées de celles-ci des enfants ciblés à leurs frères et sœurs. J'utilise une approche de régression par discontinuité et des données provenant de six pays pour comparer les femmes dont les frères et sœurs cadets ont été affectés par une réforme de l'éducation à celles dont les frères et sœurs cadets ne l'ont pas été. Je constate que, dans plusieurs pays, il y a des retombées significatives sur l'éducation secondaire et sur la fertilité des sœurs ainées. Ces résultats mettent en évidence les vastes impacts des réformes de l'éducation, et permettent de mieux comprendre les liens entre l'éducation et la fertilité, ainsi que l'importance de la réallocation des ressources par les parents.

Mots-clés : économétrie, économie de la santé, éducation, réformes nationales

Abstract

Across multiple countries, primary school reforms have been implemented with the goal of improving school attendance and accessibility. Failing to account for spillover effects from the children directly targeted by these reforms to their siblings may underestimate the reforms' full impact. Using a regression discontinuity design and data from six countries, I compare women whose younger siblings were exposed to an education reform with those whose younger siblings were not. I find that, across several countries, there is a significant younger-to-older sibling spillover effect on an older sister's probability of enrolling in secondary school and on her fertility. These findings demonstrate the broad impact of education reforms, and contribute to the understanding of the links between education and fertility, and the role played by parental reallocation of resources.

Keywords: econometrics, education, health economics, national reforms

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DHS	Demographic Health Surveys
RD	Regression discontinuity
UPE	Universal Primary Education

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Introduction

Over the last half-century, numerous primary education reforms have been implemented in developing countries. These reforms have been the subject of studies evaluating the effects of education on a number of different outcomes, e.g., marriage, teenage fertility, employment, and infant mortality [**43, 37, 20, 22, 17**]. The studies tend to focus on the children directly targeted by the reform and their children while the intra-generational spillover effects, i.e., the influence of one sibling's exposure to a reform on another sibling, have not been systematically studied. Failing to account for spillover effects among siblings may underestimate the full impact of education reforms.

This paper uses data from several country-wide primary school reforms to investigate how a younger sibling's exposure to a reform affects their older sister. I address this question systematically by analyzing reforms in Burundi, Indonesia, Malawi, Nepal, Peru, and Zambia. Following Godefroy's (2023) identification, I employ a sharp regression discontinuity (RD) design and compare exposed and non-exposed cohorts [**24**]. I limit my analysis to reforms that led to a marked increase in the education level from one cohort to the next. The results provide compelling evidence that a younger sibling's primary education has a significant impact on their older sister's secondary education and fertility.

Existing studies on sibling spillover effects in the context of education rely on eligibility cutoffs or admissions criteria to examine educational choices, for example, regarding which college to attend and what field to major in [**25, 5, 2**]. These studies nearly always analyze older-to-younger sibling effects. Therefore, they may capture more than a pure education effect and also reflect parental behavior: parents may adjust their investment in their younger children's education based on the experience of an older child. Studying the spillover effects from younger-to-older siblings avoids this limitation, allowing one to isolate the impact of one sibling's education on another.

This paper contributes to two important areas of research. First, my empirical strategy provides a deeper understanding of how education reforms affect women's fertility. By demonstrating the long-lasting effects of education reforms on the fertility of women who were only indirectly exposed to the reforms via their younger siblings, this paper challenges the notion that education reforms simply delay women's fertility by a "compulsory attendance" effect, i.e., by requiring them to be present in school. Second, my results contribute to the broader debate regarding how parents reallocate resources among unequal siblings, finding no evidence that parents support a child exposed to an education reform at the expense of an older sibling.

Chapter 1

Related literature

To date, most studies on sibling spillovers in the context of education estimate older-to-younger sibling effects. They tend to focus on educational choices, often in developed countries, using policy changes, compulsory schooling laws, or eligibility cutoffs. Altmejd et al. (2021), using college admission requirements and cutoffs in Chile, Croatia, Sweden, and the United States, show that an older sibling's college and major choices significantly affect those of their younger siblings [**5**]. They find that students are more likely to apply to and enroll in their older sibling's college and major. Karbownik and Özek (2021) and Zang et al. (2022), using mandatory school starting ages in, respectively, Florida and North Carolina, find that students whose older siblings were born soon after the school-entry cutoff date (and are therefore "old for grade") perform significantly better academically in elementary and middle school than students whose older siblings were born prior to the cutoff date; the effect is pronounced in low-income households [**31, 58**]. These results are consistent with several earlier studies, including in the United States [**25, 27**], Denmark [**30**], Mexico [**21**], Pakistan [**48**], England [**42**], and Chile [**2**], which find positive older-to-younger sibling spillover effects on educational choices and academic outcomes.

Only a handful of studies examine younger-to-older sibling spillover effects in the context of education. In the above-mentioned study in Florida, Karbownik and Özek (2021) find that in affluent households, having an "old-for-grade" younger sibling leads to poorer academic performance of the older sibling [**31**]. Conversely, Landersø et al. (2020), using the school starting age cutoff in Denmark, find that having an "old-for-grade" younger sibling improves the older sibling's grades on certain types of exams–those for which preparation by drill or memorization is helpful [**35**]. The authors suggest this is due to parents having more time to assist the older sibling with homework and exam preparation.

Godefroy (2023) studies younger-to-older sibling spillovers in Uganda [**24**]. He uses the country's Universal Primary Education (UPE) policy of 1997, which mandated the immediate removal of primary school tuition fees and prompted a sharp rise in primary school enrollment. Adopting an RD design, he compares the fertility of women whose younger siblings were born just before 1984 (the birth year of the first affected cohort) with women whose younger siblings were born later. He finds that a younger brother's exposure to the reform reduces his older sister's fertility by between 7.8% (0.389/4.94) and 12.9% (0.637/4.94).

A growing body of literature examines sibling spillover effects arising from changes unrelated to the target child's education. For example, several studies explore whether an individual's health has implications for their siblings. Heissel (2017) finds that siblings of teenage mothers have worse academic outcomes and higher rates of interactions with law enforcement [**29**]. Black et al. (2021) show that in Florida and Denmark, greater exposure to a disabled sibling reduces an older sibling's academic outcomes [**11**]. Health interventions may also cause spillover effects. Evaluating an iodine supplementation program in Tanzania [**1**], an immunization campaign in Turkey [**4**], and a deworming intervention in Kenya [**44**], researchers show that siblings of participants in these programs have better health and academic outcomes.

One proposed mechanism for the sibling spillover effect is parental reallocation of resources. In a family of two children with unequal needs, parents can divert resources to their disadvantaged child (compensation) or conversely, invest further in their other child (reinforcement). Karbownik and Özek (2021) find evidence of reinforcement: parents invest more in the younger sibling than in their older child if the younger sibling performs well in school [**31**]. Yi et al. (2015) study Chinese twins in which one twin suffered an early health shock and find evidence for both compensation and reinforcement: parents invest more health resources in the disadvantaged twin and more educational resources in the other child [**57**]. Consistent with this finding, Parman (2015) shows that parents pregnant with a child during the 1918 influenza pandemic shifted resources away from their soon-to-be born child, who might suffer a negative health shock in utero, and invested more heavily in their older child's education [**45**]. Ravindran (2019) also finds evidence of reinforcement between unequal siblings: evaluating an early child development program in India, he shows that when participants are exposed to the program at a high intensity, their siblings have worse health and education outcomes, and are more likely to experience child labor compared to the participant [**49**]. He attributes these findings to parents spending more on the child with the most program exposure by paying higher tuition and school fees and attending school meetings. All these studies highlight the importance of the intra-household reallocation of resources on the understanding of sibling spillover effects.

This paper is motivated by Godefroy's (2023) findings. Adopting a similar RD design based on the birth year of affected cohorts, it evaluates sibling spillover effects arising from six different national primary school reforms. These policies were part of a broader initiative implemented globally since the 1970s to improve school accessibility and attendance. The reforms have been shown to produce significant effects for the students directly targeted and for their children. Exposed cohorts experienced reduced rates of child marriage in Ethiopia and Rwanda [**33**]; lower child and infant mortality rates in Malawi [**37**]; lower rates of HIV infection and an increased likelihood of meeting family planning needs in Uganda and Malawi [**10, 13**]; decreased fertility by age 25 in Ethiopia [**16**]; decreased total fertility in Malawi [**59**]; and decreased desired fertility in Uganda, Ethiopia, and Malawi [**10**].

Chapter 2

Background

Table A.1 lists a number of nation-wide primary education reforms implemented between 1970 and 2005 that have been studied or used as natural experiments by economists. In the table, “Reform year” denotes the year of implementation. To determine the year of birth marking the first cohort exposed to a reform (“first cohort treated”), I use the World Bank’s guide on the starting age of primary school students and the duration of primary school. The analysis is restricted to reforms that led to marked increases in school enrollment, years of schooling, or literacy. Other reasons for excluding reforms from the current analysis are listed in Table A.1. Each reform included in the analysis is briefly described below.

2.1. Burundi

Soon after his election in 2005, the president of Burundi pledged to provide free universal primary education. Fees were removed starting in September 2005 and the gross primary enrollment rate increased by 25% for that school year [**55**]. In Burundi, students attend primary school from age 6 to age 13. Therefore, children born in 1992 or later benefited from free schooling (since they were ≤ 13 when the program was implemented); those born in 1992 constitute the ’first cohort treated.’ In the economics literature, Burundi’s implementation of free universal primary education is used as a natural experiment to estimate the impacts of education on women’s literacy, employment, and teenage fertility [**54**].

2.2. Indonesia

The Sekolah Dasar (primary school) INPRES Program was launched in 1973 to bring about higher levels of primary school enrollment. Over the next five years, 61,800 schools were strategically constructed to target the children most in need, and the number of teachers grew by 43% [**20**]. In Indonesia, primary school starts at age 6 or 7 and lasts 6 years. Since the schools began to operate in 1974, a child born in 1968 or later, i.e., who was 6 years old or younger in 1974, was young enough to fully benefit from the program. Conversely, a child born in 1962 or earlier would be at least 12 years old and finished with their primary education in 1974, and therefore would not have been affected by it. This large-scale education reform has frequently been studied. Most notably, Duflo (2001) employs an empirical strategy exploiting both the year of the first cohort partially exposed to the reform as well as the regional intensity of its implementation to estimate the impact of education on labor market outcomes [**20**]. Using a similar strategy, others study the impact of education on fertility [**14**] and bride price [**7**], and more recently, the intergenerational spillovers of the reform on children's health and education [**3, 39**].

2.3. Malawi

The government of Malawi waived primary school fees for first-graders in 1991, second-graders in 1992, then finally for all primary school students in 1994. Primary school enrollment increased by 50% following the removal of fees for all primary school students [**56**]. Since primary school starts at age 6 and lasts through age 13, children born in 1981 or later, i.e. who were ≤ 13 years old in 1994, benefited from the program. Several quasi-experimental studies use this reform to study the impact of maternal education on child mortality [**37, 6**], domestic violence [**50**], desired fertility [**10**], and other health behaviors [**13**].

2.4. Nepal

In 1992, the government of Nepal launched several literacy and education reforms, including the Basic and Primary Education Plan, which improved teaching and school quality, increased access to primary school, and helped narrow the socioeconomic and gender gaps in education rates [**41**]. Total government expenditure on education rose from 8.8% in 1990 to 13.3% in 1992 and remained at approximately 13.5% until the project ended in 1997. In Nepal, students begin primary school at age 5 or 6 and lower primary school lasts 5 years. As a result, children born in 1982 or later, i.e., who were ≤ 10 years old in 1992, benefitted from the program.

2.5. Peru

In 1993, the length of mandated education was extended from 6 years of primary- to 11 years of primary plus secondary education. Children who had not yet completed primary school in 1993, i.e. those born in 1982 or later, were affected by the new law; conversely, those who had completed primary school before 1993, i.e. those born in 1981 or earlier, were not mandated to enroll in secondary school. The reform had a highly significant direct impact on women's education [**15**]. It is used as a natural experiment to study the effect of education on intimate partner violence, maternal health, and child mortality [**52, 53, 15**].

2.6. Zambia

In March 2002, the Zambian government launched the Free Primary Education Policy, eliminating all school fees for grades 1 through 7. Enrollment increased by 30% from 2002 to 2004 [**51**]. Since primary school begins at age 7 and lasts through age 13, children born in 1989 or later, i.e., who were 13 years old or younger in 2002, benefitted from the program. This reform has been used in several studies adopting a difference-in-difference methodology to compare regions with free primary education to those without [**33, 13**].

Chapter 3

Identification and specification

3.1. Data

The data come from the Demographic Health Surveys (DHS). The DHS are nationally representative, cross-sectional household surveys of women aged 15-49. The surveys query women about their socioeconomic status, pregnancies, behaviors related to their health and the health of their children, as well as about their attitudes and the cultural norms in their communities. For each country studied, the DHS rounds used were selected according to the following criteria: 1) the survey must contain the Maternal Mortality module, and 2) the difference between the survey year and the "First cohort treated year" must be over 24 years. In the Maternal Mortality module, women were asked to list all siblings born to their biological mother by birth order and to provide information about each sibling's sex and survival status.

Two datasets are used in this analysis. The first consists of all women respondents ≥ 20 years old and born within 8 years before or after the "First cohort treated" year. Descriptive statistics for this dataset are listed in Table A.2. The second dataset is structured so that each observation is a sibling pair. Each pair consists of an older sister ≥ 25 years old and her younger sibling who must be currently alive and born within 8 years before or after the "First cohort treated" year. If the older sister has multiple younger siblings, she is paired with her oldest younger sibling. It was assumed that siblings closer in age are more likely to interact with and influence each other than siblings farther apart. For the same reason, the sample contains only pairs whose difference in age is less than 10 years. Twins were excluded.

Younger-to-older sibling spillover effects are the primary focus; therefore, the cutoff is the threshold dividing younger siblings who were exposed to the reform (either for their full education or for part of it), i.e., born on or after the "first cohort treated" year, from those who were not exposed.

3.2. Empirical strategy

My analysis is divided into two parts corresponding to the two datasets defined above: all women respondents and older sisters within a sibling pair. There are three main outcomes: for the dataset of all women respondents, education (determined by school enrollment, years of schooling, and literacy); and for the dataset of older sisters, education (determined by secondary school enrollment) and fertility (number of children).

I begin by estimating the direct impact of the reform on women respondents' education using a sharp RD design with the following specification:

$$y_i = \alpha_1 + \beta_1 \cdot \text{Born above cutoff}_i + \gamma_1 X_i + f(r_i) + \epsilon_i \tag{3.2.1}$$

where y_i represents respondent i's education level. "Born above cutoff$_i$" is equal to 1 if the respondent is born on or after the "first cohort treated" year. X_i is a vector of control variables consisting of fixed effects for the respondent i's age and fixed effects for the survey years if applicable. $f(r_i)$ is a linear function of the respondent's year of birth r_i.[1]

Second, I estimate the impact of a sibling's education on their older sister using the dataset of sibling pairs. I employ a sharp RD design comparing women with a younger sibling born just before the "first cohort treated" year to women with a younger sibling born just after the "first cohort treated" year. Except for whether or not their younger sibling was exposed to the reform, the women in these two groups have similar predetermined characteristics. The specification is the following:

$$y_p = \alpha_2 + \beta_2 \cdot \text{Sibling born above cutoff}_p + \gamma_2 X_p + f(s_p) + u_p \tag{3.2.2}$$

where y_p represents the outcome of the older sister from the sibling pair p. "Sibling above cutoff$_p$" is equal to 1 if the younger sibling from pair p is born on or after the "first cohort treated" year. As a result, β_2 captures the effect of a younger sibling's exposure to a reform on their older sister's education and fertility.

X_p is a vector of control variables for sibling pair p, specifically fixed effects for the older sister's age, fixed effects for the age difference within the pair, and fixed effects for the survey years if applicable. $f(s_p)$ is a polynomial function of the younger sibling's year of birth s_p.[2] I also extend my analysis to quadratic polynomials of s_p to allow for greater variability of pre- and post-cutoff trends.

A key assumption for this analysis is that the discontinuity in outcomes for the older sister at the time of the cutoff is a result of the younger sibling's schooling, and not of broader political or economic changes. Additionally, the older sister must not be able to manipulate her younger sibling's treatment, e.g., decide whether or not they receive additional schooling.

[1] $f(r_i) = \theta_1(r_i - \text{cutoff}) \cdot \mathbf{1}_{\text{treated}} + \Theta_1(r_i - \text{cutoff}) \cdot (1 - \mathbf{1}_{\text{treated}})$
[2] $f(s_p) = \theta_2(s_p - \text{cutoff}) \cdot \mathbf{1}_{\text{treated}} + \Theta_2(s_p - \text{cutoff}) \cdot (1 - \mathbf{1}_{\text{treated}})$

Chapter 4

Results

4.1. Direct effect of reforms on education

The reforms had a significant and immediate impact on students. Figures A.1a-f depict women's educational levels by their year of birth. Discontinuities immediately following the cutoff, denoted by a vertical line, represent the direct effect of the reform. The reforms generated a sharp increase in school enrollment, years of schooling, and literacy levels across countries. Immediately following the reforms, time in school increased by nearly a year in Indonesia, Nepal, and Zambia (Figures A.1b, d, f) and literacy levels jumped in Burundi, Indonesia, Nepal, and Zambia (Figures A.1a, b, d, f).

Estimating specification (3.2.1), I find that women who were exposed to a reform achieved 1.2 (Burundi), 0.3 (Indonesia), 1.2 (Nepal), 0.2 (Peru), and 0.7 (Zambia) more years of education than women who were not (Table A.3). In Malawi, exposure to the reform did not show a significant impact on women's years of schooling. This may be due to the phased-in approach taken to implement the reform. Nevertheless, following the reform, Malawi experienced increases in school enrollment and literacy rates (Figure A.1c). All these results are consistent with prior studies that use the same identification strategy as this analysis: girls in the first cohorts affected by new tuition-free primary schooling or by changes in compulsory schooling laws gained 0.6 years of schooling in Ethiopia [**10**], 0.2 years in Peru [**15**], between 0.3 and 0.5 years in Malawi [**10**], between 0.6 and 1.2 years in Uganda [**32, 37**], and between 0.9 and 1.3 years in Burundi [**54**].

4.2. Effect of younger sibling on older sister's education

The first question of interest is whether the older sister from sibling pair p enrolled in secondary school as a result of her younger sibling's exposure to a reform. I find a distinct increase in secondary school enrollment of older sisters just after the cutoff in Indonesia, Malawi, and Zambia (Figures A.3b, c, f). In Indonesia and Malawi, this effect is driven by younger brothers, while in Zambia, it is driven by younger sisters.

Estimating specification (3.2.2), I find that a younger brother's exposure to a reform increases his older sister's probability of enrolling in secondary school by 2.6 percentage points in Indonesia and 3.3 percentage points in Malawi, after controlling for age of the sister and age difference between the siblings, and assuming a linear functional form (Tables A.4, A.5). Based on the mean percentage of women enrolling in secondary school in these samples, these results translate into relative increases in the older sister's probability of enrolling in secondary school of 6.1% (0.026/0.426) in Indonesia and 16.8% (0.033/0.197) in Malawi. Using results from section 4.1 for Indonesia, I find that a younger brother's additional year of schooling is associated with a 18.3% (0.026/0.426/0.334) increase in the probability that his older sister enrolls in secondary school.

In Zambia, a sister's reform exposure increases her older sister's probability of enrolling in secondary school by 4.6 percentage points (Table A.6). This translates into an 11% relative increase and is robust to controlling for age of the older sister and age difference between the siblings. Assuming a quadratic functional form, the effect rises to 6.3 percentage points, implying a 15.1% relative increase in the probability of the older sister enrolling in secondary school. Using results from section 4.1 for Zambia, I find that a younger sister's additional year of schooling is associated with a 15.7% (0.046/0.418/0.700) increase in the probability that her older sister enrolls in secondary school.

4.3. Effect of younger sibling on older sister's fertility

Across several countries, a younger sibling's exposure to a reform significantly decreases their older sister's fertility (Figure A.5). There is an especially clear discontinuity for Malawi, Nepal, Peru, and Zambia.

In Malawi, a younger brother's exposure to a reform reduces his older sister's fertility by 0.184 children after controlling for age of the sister and age difference between the siblings, and assuming a linear functional form (Table A.7). Relative to the mean, this represents a 4% reduction in the older sister's fertility.

In Nepal, the eight-year bandwidths do not generate significant results, despite a slight discontinuity (Figure A.5d). However, using four-year bandwidths, I find that a younger brother's exposure to the reform significantly reduces his older sister's fertility by 0.389 children, representing an 11.2% relative decrease in the number of children born to her (Table A.8). Using results from section 4.1 for Nepal, I find that a younger brother's additional year of schooling is associated with a 9.5% (0.389/3.48/1.173) decrease in the number of children born to his older sister.

In Peru, there is a notable spillover on fertility arising from younger sisters (Table A.9). Combining younger sisters and brothers, I find that a younger sibling's exposure to the reform reduces their older sister's fertility by 0.123 percentage points, representing a 4.7% relative decrease in the number of children born to her. Using results from section 4.1 for Peru, I find that a younger sibling's additional year of schooling is associated with a 20% (0.123/2.61/0.235) decrease in the number of children born to their older sister.

In all samples, 10-20% of older sisters were born on or after the "first cohort treated" year. This occurs due to close-in-age sibling pairs where the younger sibling is born near the 8-year limit after the cutoff. Since these women were exposed to the reform, I repeat my analyses after removing them. The results show that the above-mentioned discontinuities remain. Similarly, when estimating specification (3.2.2) without these women, I find that the results do not change markedly.

4.4. Effect of younger sibling on other outcomes

It is well documented that delays in age at first birth result in lower total fertility [**38, 9**]. However, I do not find that a younger sibling's exposure to a reform resulted in later age at first birth for their older sister (Figure A.7). Similarly, a younger sibling's exposure to a reform does not change their older sister's health-related behaviors, such as adopting family planning methods or visiting a health facility in the last year.

4.5. Direct effect of reforms on fertility

To put into context the magnitude of the sibling spillover effects, I complete one final analysis. I use the initial dataset of all women to estimate the impact of the reforms on the fertility of women directly targeted. The reforms appear to have no direct impact on women's fertility (Figure A.9). Consistent with Figure A.9, estimates from specification (3.2.1) (where y_i is woman i's number of children), show that the reforms had no significant impact on women's fertility when assuming a linear functional form (Table A.10). Research on these specific reforms have shown similar results: Bui (2023) finds that the 1993 compulsory schooling law in Peru had no significant impact on women's fertility, for instance [**15**]. In this context, the sibling spillovers on fertility presented in section 4.3 are important to fully understand the impact of an education reform on fertility.

4.6. Robustness

There are three threats to an RD design and analysis: a discontinuous change from variables other than the treatment status, manipulation of the treatment variable, and important prior- or post-cutoff discontinuities of the assignment variable. Regarding a discontinuous change in covariates, in the current analysis there are few predetermined observable characteristics that can be tested.

To check for manipulation of the treatment variable, I perform the McCrary test on the samples of all women using the most recent DHS survey round. The test ensures that the sample distribution is continuous, since a discontinuity at the threshold could indicate sorting on the running variable (the year of birth). In this analysis, such a discontinuity could indicate, for example, that some respondents falsified their year of birth in order to be exposed to the reform. I do not find evidence of manipulation: the sample distributions appear mostly continuous (Figures A.11a-f).

To check whether there are important discontinuities that occur prior to or following the cutoff, I repeat the analysis of sibling spillover effects on older sisters' education and fertility using four-year intervals pre- and post-cutoff. The sibling spillover effects on secondary school enrollment in Indonesia and in Zambia remain significant and large with the four-year bandwidths. The sibling spillover effects on fertility remain significant and large in Malawi and Peru with four-year bandwidths. Additionally, for all the above mentioned countries, the direction and magnitude of the coefficients remain consistent with the eight-year bandwidth results.

Chapter 5

Discussion

5.1. Findings

The results of this analysis show that a younger sibling's exposure to primary education reforms has spillover effects on their older sister. Across Indonesia, Malawi, and Zambia, the primary school reforms lead to higher rates of secondary school enrollment among older sisters. Across Malawi, Nepal, Peru, and Zambia, the reforms significantly reduce fertility among older sisters. For both education and fertility outcomes, the effects remain large and in the same direction throughout different specifications. The magnitude of the spillover effects appears consistent. Regarding older sisters' education, the magnitude of the increase in secondary school enrollment attributed to a younger sibling's additional year of schooling is similar between Indonesia (18.3%) and Zambia (15.7%). The magnitude of the decrease in fertility attributed to a younger sibling's additional year of schooling is comparable between Nepal (9.5%) and Peru (20%).

The impact of education on fertility is a widely researched topic (see Psaki et al. (2019) for a systematic review of the literature [**47**]). Some studies show that education reduces fertility, while a few find that it has no effect [**43, 23**]. In this analysis, across six countries there does not seem to be a direct impact of the reforms on fertility. However, significant effects of the reforms on fertility are revealed when one analyzes sibling spillover effects. My results are consistent with those of Godefroy (2023) which show that while the 1997 UPE reform in Uganda did not have a direct impact on women's fertility, it had a large and significant spillover effect on fertility from younger brothers to older sisters [**24**]. Together with Godefroy's (2023) findings, the current analysis demonstrates that when evaluating an education reform, only capturing targeted participants' outcomes likely underestimates the impact on women's fertility.

Interestingly, in the current analysis, there are sibling spillover effects on fertility even in countries where there is no direct or spillover effect on education. This is important because it addresses two main theories that link education with fertility: the compulsory attendance effect and the human capital effect. The compulsory attendance effect (also known as the "incarceration effect") asserts that girls delay marriage and childbearing only because they are obligated to be present in school [**12**]. For instance, Kirdar et al. (2016) find that a lengthening of the mandatory schooling period in Turkey led girls to delay marriage and childbearing until they finished school, but that they married and had children quickly afterward [**34**]. As a result, the authors conclude that the change in the women's health behaviors are only due to the compulsory attendance effect. While there may be a longer lasting human capital effect, that could be shown, for instance, by reduced total fertility, this would be measurable only when the women from the study are older.

In my analysis, I find that some reforms significantly affect women's fertility, and by design, this result cannot be due to a compulsory attendance effect. Since this is a spillover study, the compulsory attendance effect is applicable only to the few older sisters who were born on or after the "first cohort treated" year and were therefore exposed to the reform. When I remove these women from the analysis, my findings on fertility do not change. Even without being exposed to a reform, women changed their health behavior as a result of their younger sibling's exposure. This is compelling evidence of a human capital effect of schooling reforms on women's fertility.

5.2. Mechanisms

The education of a younger sibling can have an impact on the education and fertility of their older sister through the latter's exposure to new knowledge, networks, and resources. Additional years of primary education improves students' knowledge of and access to health and economic resources, and there is evidence that this knowledge and access is shared among family members [**28, 42**]. Therefore, the positive effects of an education reform may extend from a younger to an older sibling, particularly if they frequently interact or live together; evidence from the samples of all women suggests that a non-trivial number of older sisters live with their younger brothers. Furthermore, it is plausible that an education reform has community-wide effects on social norms related to educational attainment, family size, or health behaviors.

Another possible mechanism driving the sibling spillover effects observed in this analysis is parental reallocation of resources. Studies investigating sibling spillover effects of health or education changes to one child of a sibling group show that parental investments may either compensate for or, alternatively, reinforce the resulting inequities among the siblings [**57, 1**]. Ravindran (2019) and Barrera-Osorio et al. (2011) identify a large reinforcement effect in their research on early development and conditional cash transfer programs, respectively; both find that siblings of treated participants develop worse health or academic outcomes as a result of parents investing more heavily in the treated child [**49, 8**]. In fact, Barrera-Osorio's et al (2011) suggest that families with a participant in such a program disadvantage the other siblings by taking educational opportunities away from them [**8**]. In my analysis, I do not find evidence of such a reallocation of resources. I find that older sisters with a younger sibling exposed to an education reform have a higher probability of enrolling in secondary school than those with a younger sibling not exposed to the reform, suggesting that parents did not reinforce their treated child's advantages and opportunities at the expense of their older daughter.

Conclusion

Evaluations of education reforms should assess not only the children directly affected by them, but also those who may be affected indirectly through spillover effects; neglecting the latter may lead to an underestimation of a reform's full impact. In my analysis, I find that national education reforms have a significant spillover effect on the education and fertility of the older sisters of targeted children. Compelling topics for future research in this area include the longer-term economic and labor market outcomes of these older sisters. The results of such studies would help to fully capture the extent to which indirect exposure to education reforms affect women, and could further demonstrate that education reforms may have wider impacts than have been until now appreciated.

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Appendix A

Tables and figures

Table A.1. Nation-wide education reforms in low- and middle-income countries

Policy	Country	Reform year	First cohort treated	Reason not chosen	Reference
Free primary education	Burundi	2005	1992		Wild and Stadelmann (2020)
Primary school construction	Indonesia	1974	1968		Duflo (2001)
Free primary education	Malawi	1994	1981		Makate and Makate (2016)
Basic Education Plan	Nepal	1990	1982		
Compulsory education	Peru	1993	1982		Bui (2023)
Free primary education	Zambia	2002	1989		
Grades 1-10 fee removal	Ethiopia	1995	1984	Varying reform intensity	Chicoine (2019)
Free primary education	Kenya	2003	1989	Varying reform intensity	Lucas and Mbiti (2012)
Free primary education	Lesotho	2000	1994	Reform too recent	Moshoeshoe (2020)
Free primary education	Nigeria	1976	1970	Reform ended in 1981	Osili and Long (2008)
Compulsory education	Taiwan	1968	1956	No sibling data	Chou et al. (2010)
Compulsory Schooling Law	Turkey	1998	1987	No sibling data from DHS or MICS	Erten and Keskin (2018)
Free primary education	Uganda	1997	1984	See Godefroy (2023)	Keats (2018)
Organic Law of Education	Venezuela	1980	1968	No sibling data from DHS or MICS	Patrinos and Sakellariou (2004)
Free primary education	Vietnam	1991	1977	No sibling data from DHS or MICS	Dang (2018)
School construction	Zimbabwe	1980	1967	No marked increase	Grépin and Bharadwaj (2015)

Table A.2. Descriptive statistics for the samples of all women

Country	Burundi		Indonesia		Malawi		Nepal		Peru		Zambia	
Threshold	1991.5		1967.5		1980.5		1981.5		1981.5		1988.5	
	Pre	Post	Pre	Post	Pre	Post	Pre	Post	Pre	Post	Pre	Post
Year of birth	1987.7	1994.3	1963.8	1971.4	1976.9	1984.6	1977.7	1985.1	1977.5	1985.5	1984.7	1992.2
Attended school	0.58	0.80	0.87	0.95	0.79	0.91	0.41	0.63	0.97	0.99	0.89	0.95
Years of school	4.06	6.02	6.39	7.93	4.77	6.18	2.77	4.54	9.26	10.26	6.71	8.03
# Children	2.70	0.81	3.29	2.34	4.80	2.96	3.05	1.92	2.40	1.18	3.57	1.69
Observations	4505	3525	39141	43188	9385	13004	4967	5309	21400	22219	7252	7033

Table A.3. Direct impact of reforms on women's primary education

	Burundi		Indonesia		Malawi		Nepal		Peru		Zambia	
Reform	1.663***	1.168***	0.333***	0.334***	0.024	0.009	1.973***	1.173**	0.231**	0.235**	0.703***	0.700***
	(0.399)	(0.414)	(0.076)	(0.075)	(0.120)	(0.120)	(0.128)	(0.561)	(0.110)	(0.111)	(0.146)	(0.146)
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
R2	0.049	0.051	0.042	0.043	0.047	0.047	0.058	0.058	0.017	0.017	0.031	0.032
# Obs	8030	8030	82328	82328	22389	22389	10276	10276	43619	43619	14276	14276
Mean	4.92	4.92	7.19	7.19	5.58	5.58	3.68	3.68	9.76	9.76	7.36	7.36

Table A.4. Indonesia: Oldest younger brother's year of birth and older sister's education

	4-year bandwidths				8-year bandwidths			
Brother born in or after 1968	0.041***	0.054***	0.052***	0.068**	0.036***	0.048***	0.051***	0.048***
	(0.010)	(0.016)	(0.016)	(0.027)	(0.009)	(0.011)	(0.011)	(0.017)
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Difference FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Linear polynoms	No	No	Yes	Yes	No	No	Yes	Yes
Quadratic polynoms	No	No	No	Yes	No	No	No	Yes
R2	0.028	0.029	0.029	0.029	0.043	0.044	0.044	0.044
# Observations	15013	15013	15013	15013	28740	28740	28740	28740
Mean dependent var.	.388	.388	.388	.388	.410	.410	.410	.410

Table A.5. Malawi: Oldest younger brother's year of birth and older sister's education

	4-year bandwidths				8-year bandwidths			
Brother born in or after 1981	0.019	0.022	0.017	0.058	0.019	0.032^{**}	0.033^{**}	0.026
	(0.014)	(0.023)	(0.023)	(0.038)	(0.013)	(0.016)	(0.016)	(0.024)
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Difference FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Linear polynoms	No	No	Yes	Yes	No	No	Yes	Yes
Quadratic polynoms	No	No	No	Yes	No	No	No	Yes
R2	0.031	0.032	0.034	0.034	0.038	0.039	0.039	0.039
# Observations	4636	4636	4636	4636	9394	9394	9394	9394
Mean dependent var.	.185	.185	.185	.185	.197	.197	.197	.197

Table A.6. Zambia: Oldest younger brother's year of birth and older sister's education

	4-year bandwidths				8-year bandwidths			
Sister born in or after 1989	0.080***	0.046	0.042	0.099*	0.073***	0.049**	0.046*	0.063*
	(0.021)	(0.033)	(0.033)	(0.054)	(0.019)	(0.023)	(0.023)	(0.035)
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Difference FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Linear polynoms	No	No	Yes	Yes	No	No	Yes	Yes
Quadratic polynoms	No	No	No	Yes	No	No	No	Yes
R2	0.013	0.015	0.015	0.016	0.017	0.019	0.020	0.020
# Observations	3733	3733	3733	3733	6555	6555	6555	6555
Mean dependent var.	.411	.411	.411	.411	.418	.418	.418	.418

Table A.7. Malawi: Oldest younger brother's year of birth and older sister's fertility

	4-year bandwidths				8-year bandwidths			
Brother born in or after 1981	-0.044	-0.231**	-0.216*	-0.208	-0.057	-0.182**	-0.184**	-0.157
	(0.075)	(0.116)	(0.115)	(0.187)	(0.065)	(0.080)	(0.080)	(0.121)
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Difference FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Linear polynoms	No	No	Yes	Yes	No	No	Yes	Yes
Quadratic polynoms	No	No	No	Yes	No	No	No	Yes
R2	0.137	0.139	0.139	0.140	0.239	0.240	0.240	0.240
# Observations	4636	4636	4636	4636	9318	9318	9318	9318
Mean dependent var.	4.79	4.79	4.79	4.79	4.57	4.57	4.57	4.57

Table A.8. Nepal: Oldest younger brother's year of birth and older sister's fertility

	4-year bandwidths				8-year bandwidths			
Brother born in or after 1982	-0.139	-0.401*	-0.389*	-0.781**	-0.039	-0.126	-0.130	-0.358
	(0.126)	(0.215)	(0.213)	(0.343)	(0.118)	(0.155)	(0.156)	(0.226)
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Difference FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Linear polynoms	No	No	Yes	Yes	No	No	Yes	Yes
Quadratic polynoms	No	No	No	Yes	No	No	No	Yes
R2	0.050	0.059	0.060	0.062	0.162	0.101	0.102	0.103
# Observations	1214	1214	1214	1214	2479	2479	2479	2479
Mean dependent var.	3.48	3.48	3.48	3.48	3.36	3.36	3.36	3.36

Table A.9. Peru: Oldest younger brother's year of birth and older sister's fertility

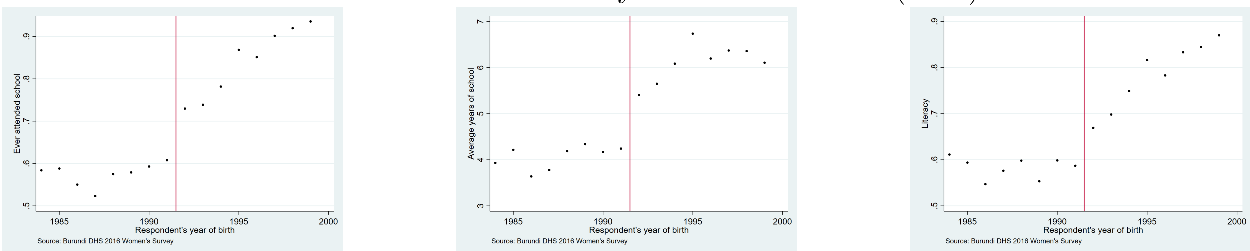
	4-year bandwidths				8-year bandwidths			
Brother born in or after 1982	-0.053	-0.174*	-0.170*	-0.364**	-0.063	-0.126*	-0.123*	-0.113
	(0.062)	(0.102)	(0.101)	(0.166)	(0.058)	(0.074)	(0.074)	(0.107)
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Difference FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Linear polynoms	No	No	Yes	Yes	No	No	Yes	Yes
Quadratic polynoms	No	No	No	Yes	No	No	No	Yes
R2	0.063	0.065	0.066	0.066	0.141	0.143	0.143	0.143
# Observations	5071	5071	5071	5071	9942	9942	9942	9942
Mean dependent var.	2.64	2.64	2.64	2.64	2.61	2.61	2.61	2.61

Table A.10. Direct impact of reforms on women's fertility

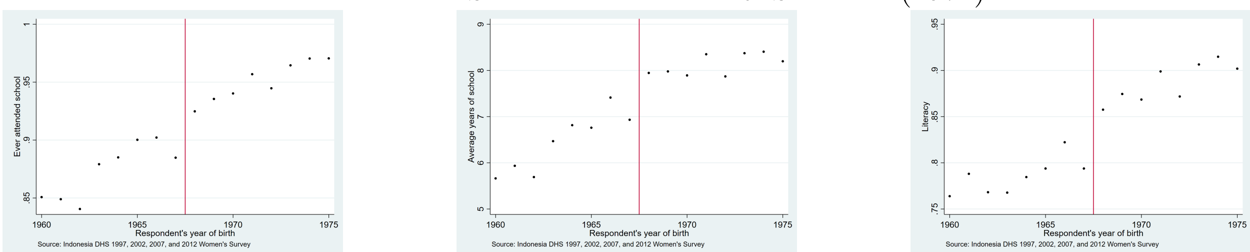
	Burundi		Indonesia		Malawi		Nepal		Peru		Zambia	
Reform	-0.394***	-0.111	0.025	0.024	0.064	0.071	-0.538***	-0.097	-0.041	-0.041	-0.042	-0.041
	(0.096)	(0.101)	(0.025)	(0.025)	(0.049)	(0.050)	(0.043)	(0.143)	(0.035)	(0.035)	(0.053)	(0.053)
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
R2	0.403	0.409	0.174	0.174	0.352	0.352	0.279	0.280	0.214	0.214	0.382	0.382
# Obs	8030	8030	82329	82329	22389	22389	10276	10276	43619	43619	14285	14285
Mean	1.86	1.86	2.79	2.79	3.73	3.73	2.46	2.46	1.77	1.77	2.64	2.64

Fig. A.1. Direct impact of reforms on all women's education level

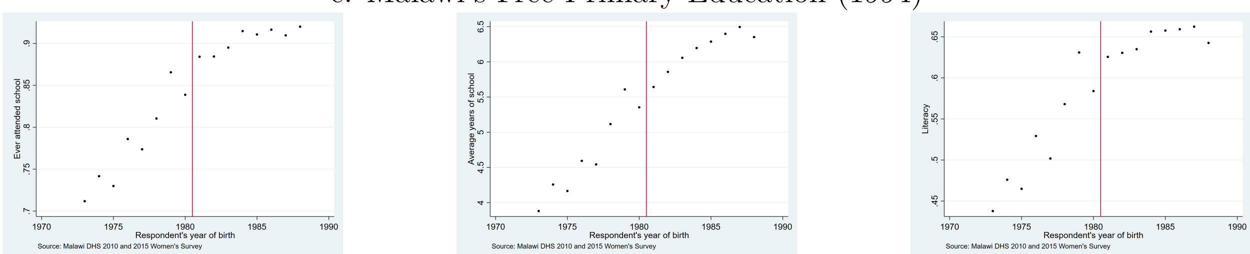
a: Burundi's Free Primary Education reform (2005)



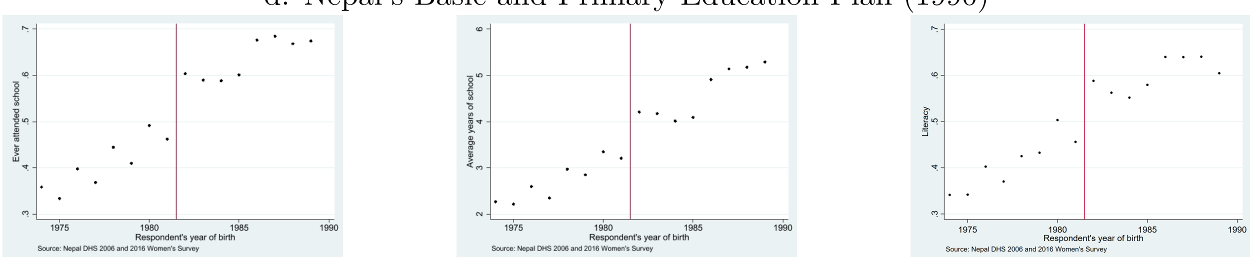
b: Indonesia's Sekolah Dasar INPRES reform (1974)



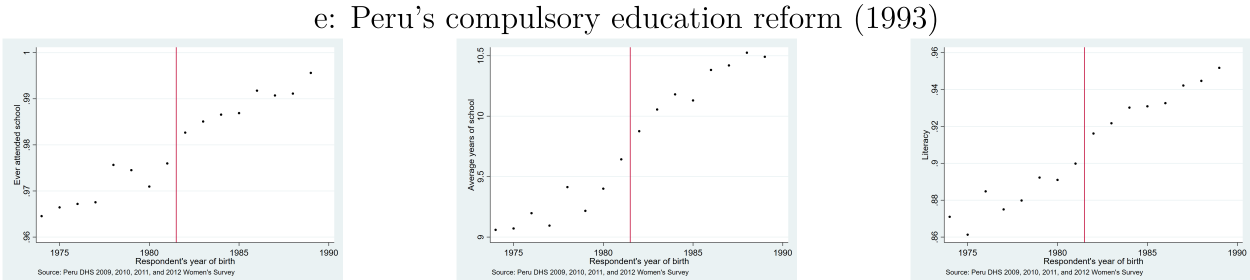
c: Malawi's Free Primary Education (1994)



d: Nepal's Basic and Primary Education Plan (1990)



e: Peru's compulsory education reform (1993)



f: Zambia's Free Primary Education reform (2002)

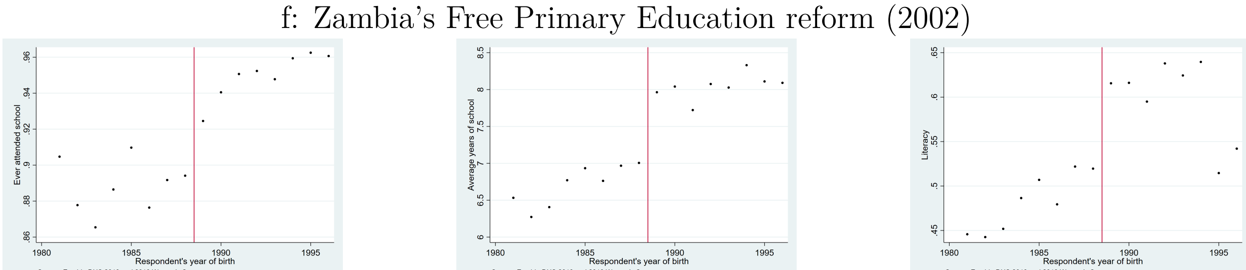
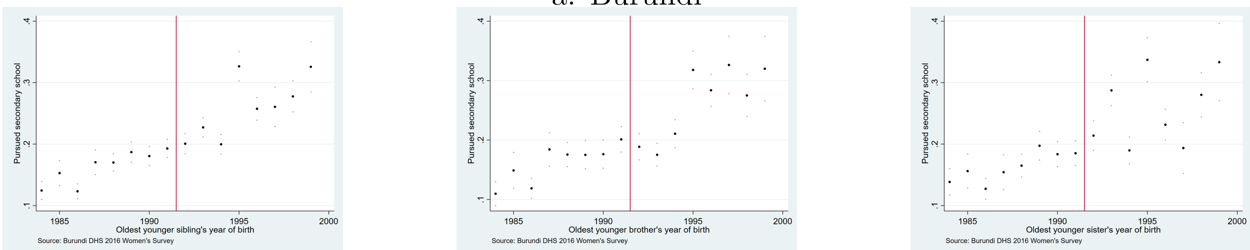
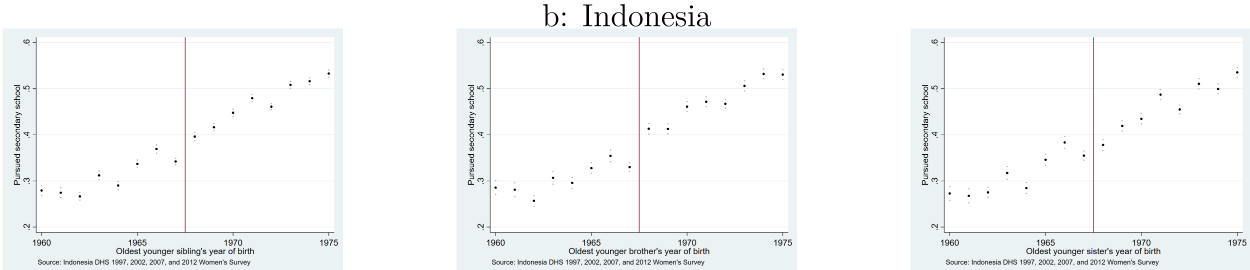


Fig. A.3. Women's secondary school enrollment by their oldest younger sibling's year of birth, disaggregated by sex of sibling (middle and left columns)

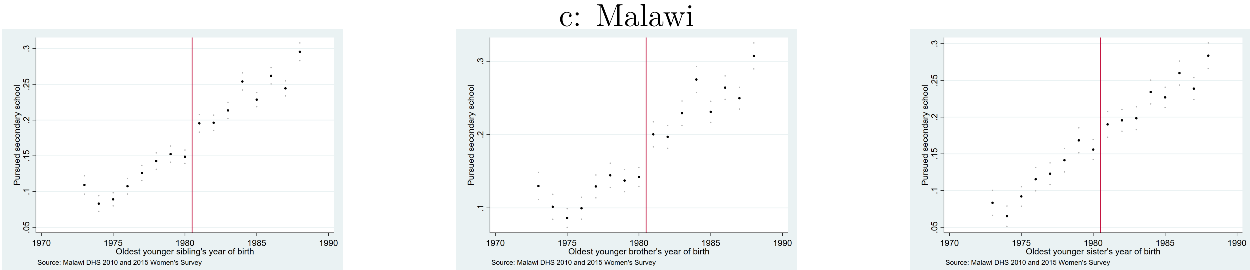
a: Burundi



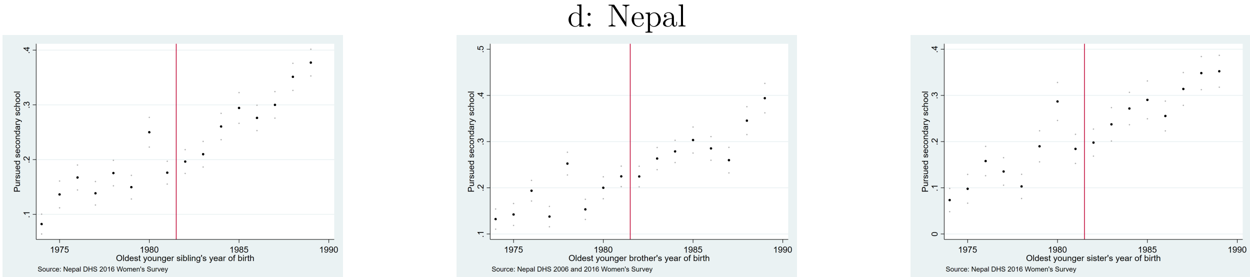
b: Indonesia



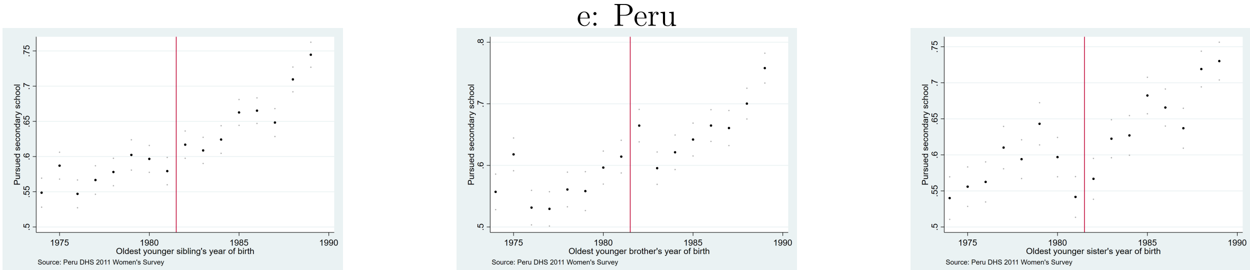
c: Malawi



d: Nepal



e: Peru



f: Zambia

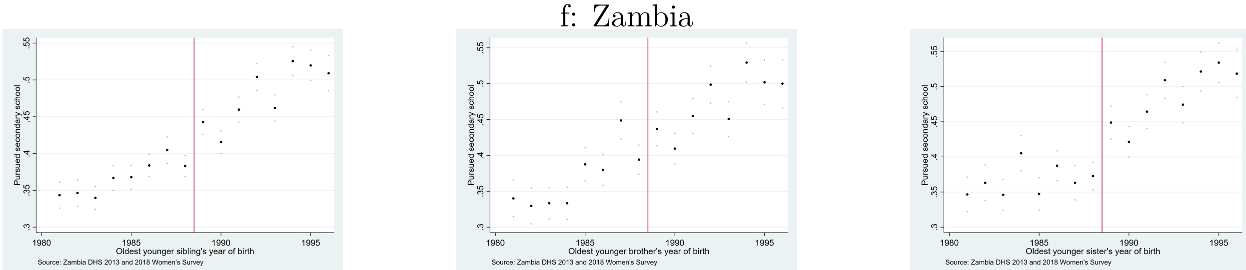
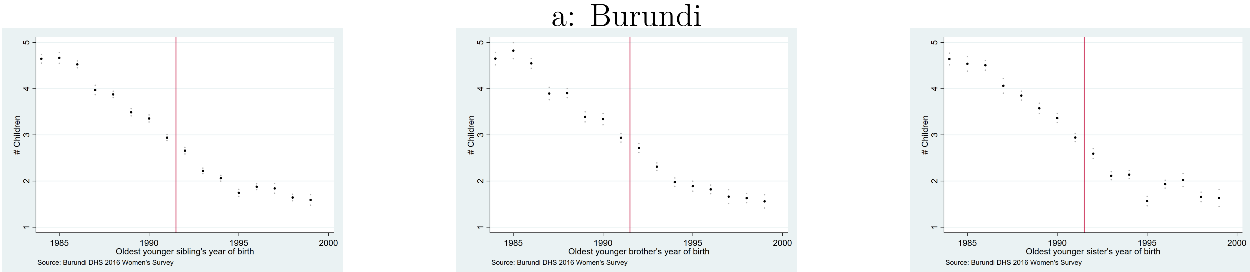
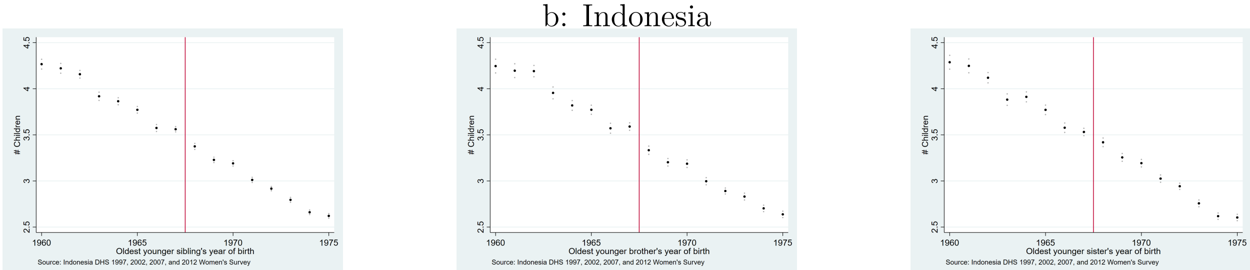


Fig. A.5. Women's fertility by their oldest younger sibling's year of birth, disaggregated by sex of sibling (middle and left columns)

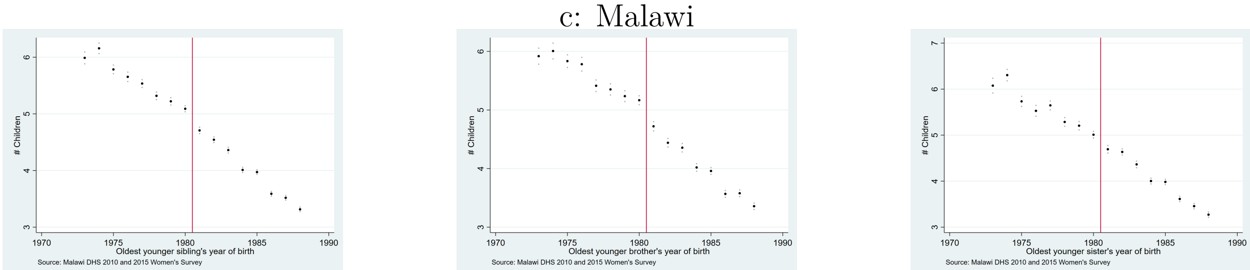
a: Burundi



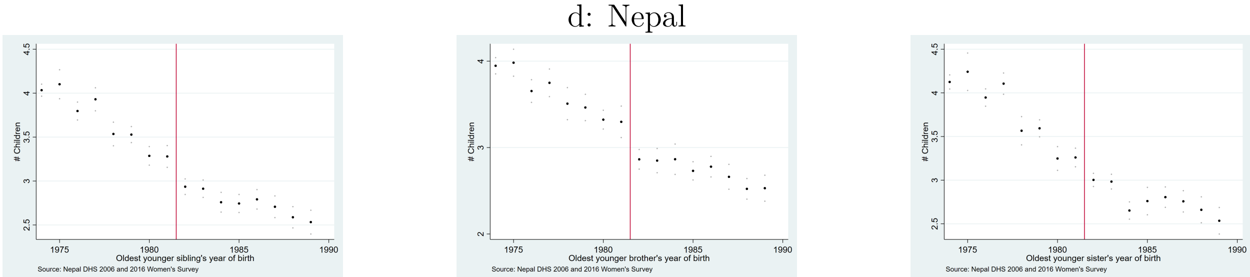
b: Indonesia



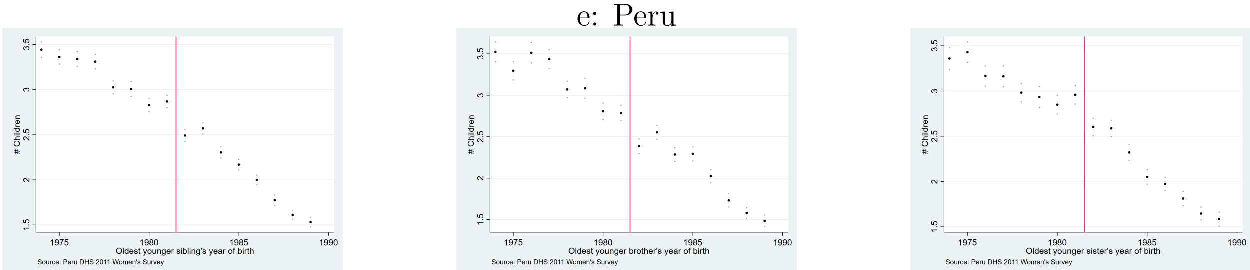
c: Malawi



d: Nepal



e: Peru



f: Zambia

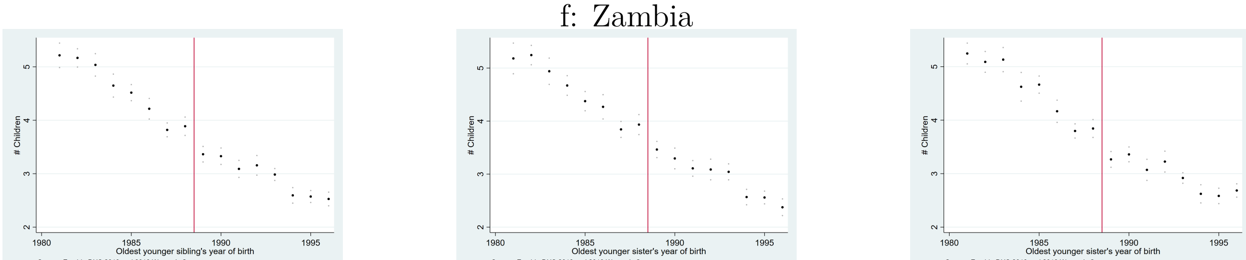
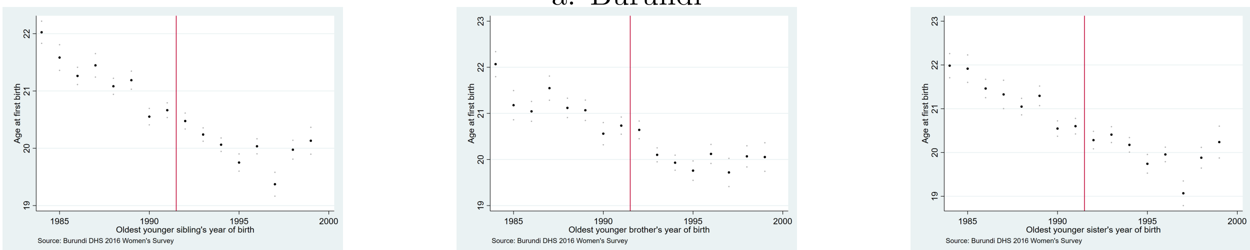
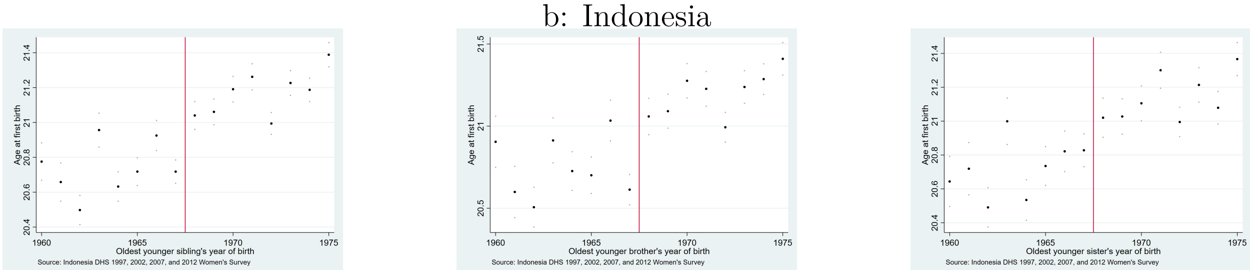


Fig. A.7. Women's age at first birth by their oldest younger sibling's year of birth, disaggregated by sex of sibling (middle and left columns)

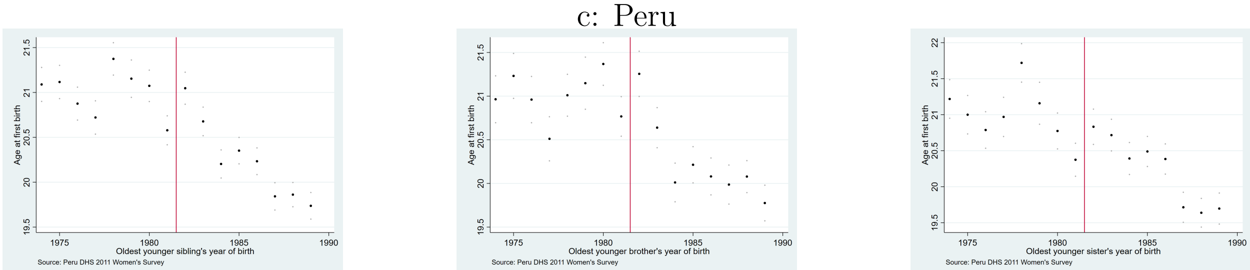
a: Burundi



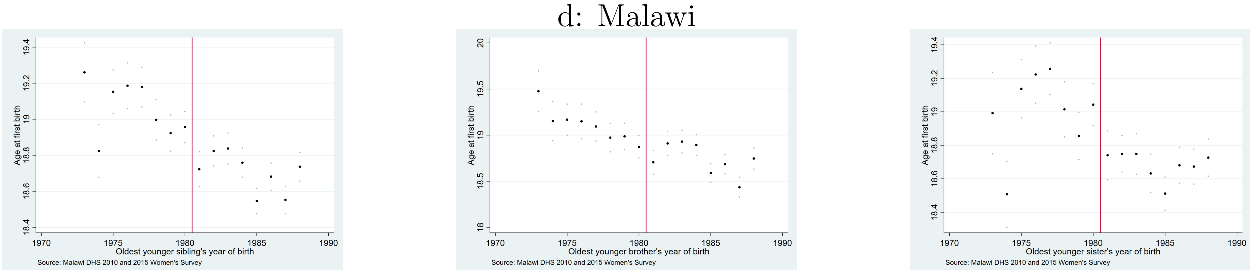
b: Indonesia



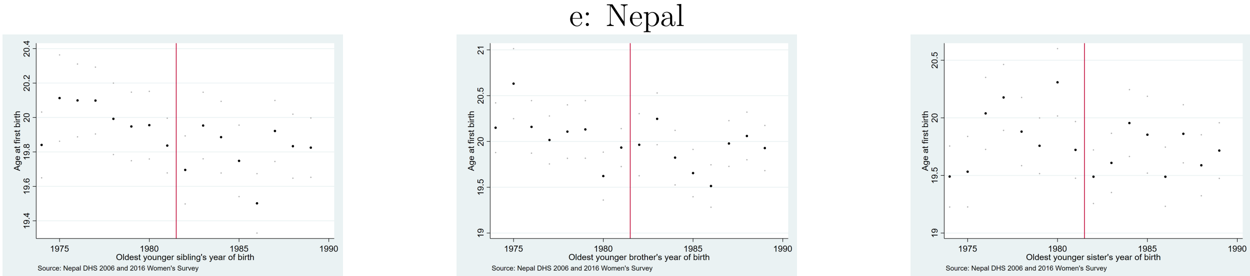
c: Peru



d: Malawi



e: Nepal



f: Zambia

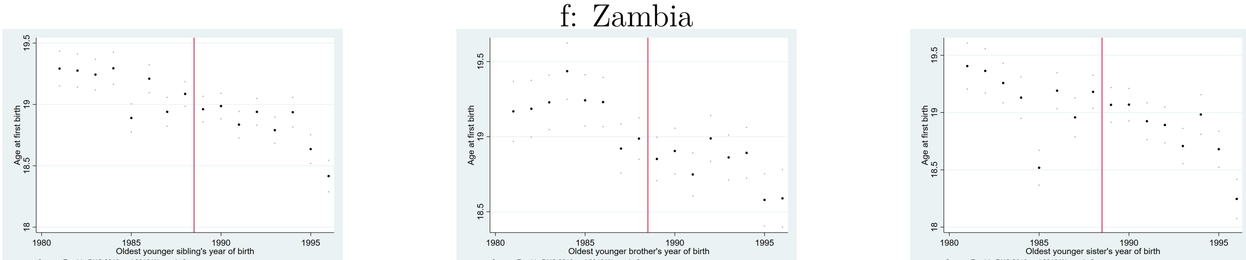
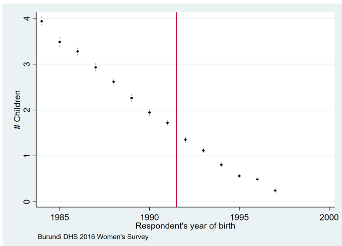
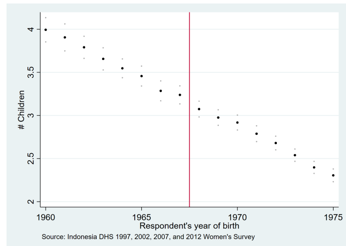


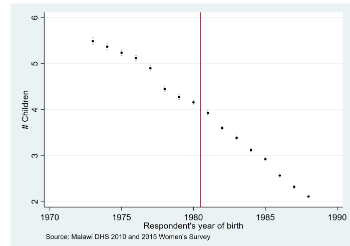
Fig. A.9. Direct impact of reforms on all women's fertility



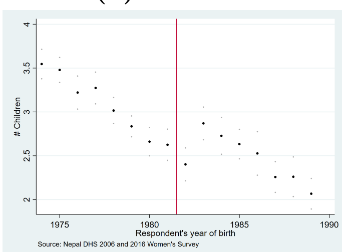
(a) Burundi



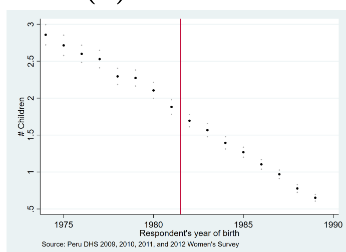
(b) Indonesia



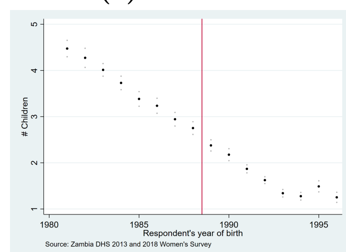
(c) Malawi



(d) Nepal

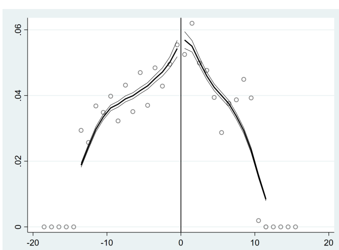


(e) Peru

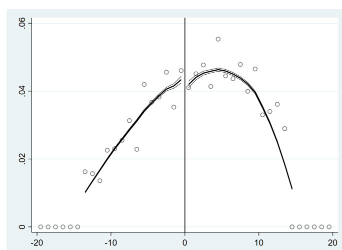


(f) Zambia

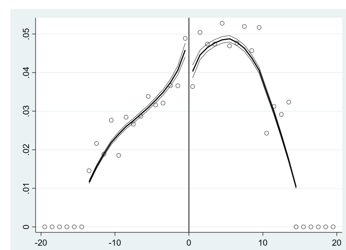
Fig. A.11. McCrary density test on samples of all women



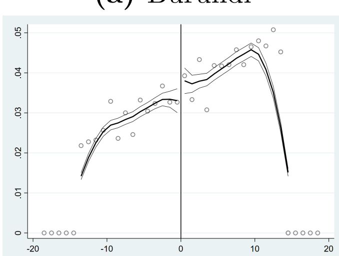
(a) Burundi



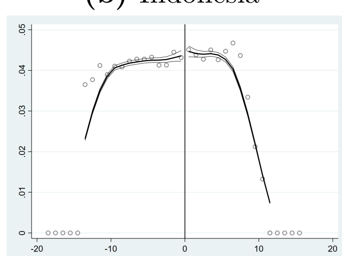
(b) Indonesia



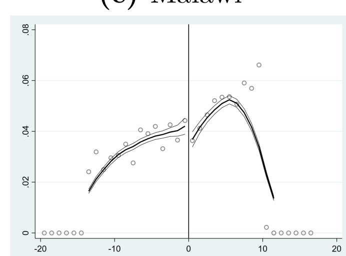
(c) Malawi



(d) Nepal



(e) Peru



(f) Zambia