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Three Essays on the Economics of Education

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ABSTRACT

This dissertation consists of three essays related to the field of economics of education. In chapter 2, using data from middle school students in China and exploiting the random assignment of students to classrooms within schools, I investigate the causal effect of peer groups on students' scholastic achievement. I find that female student proportion in the classroom positively affects male students' test scores and that the education level of peers' parents improves the academic achievement of both male and female students. Students with highly-educated parents benefit more from classmates with higher parental education compared to students with relatively lower parental education. Investigation of mechanisms reveals that the peer effects can in part be explained by peers' academic quality, classroom atmosphere, and behaviors of students' classroom friends.

Chapter 3 examines the causal impact of female education on fertility utilizing the Universal Primary Education (UPE) program in Malawi as a source of exogenous variation in schooling attainment. The results show that the UPE policy improved rural women's educational attainment by 0.42 years and that an additional year of female education decreased women's number of children ever born and living children by 0.39 and 0.33, respectively. An analysis of potential mechanisms suggests that the decreased fertility rates are driven by the reduction in women's desired number of children, postponement of marriage and motherhood. There is no evidence that increased female education affects the characteristics of husband, women's labor force participation, or modern contraceptive use.

In chapter 4, I investigate the causal effect of maternal education on child mortality in Indonesia by using the one-time change in the length of the 1978 school year as a source of exogenous variation in education. The results show that the education reform increases women's educational attainment by 0.82 years and an additional year of female education leads to a decrease in neonatal mortality by 0.8 percentage points. Mechanisms analysis suggests that

higher female education postpones the timing of marriage and first birth, leads to higher quality of spouse and higher household wealth, and increases the use of prenatal health care and mass media.

CHAPTER 1. INTRODUCTION

This dissertation consists of three essays within the field of education economics. In chapter 2, I investigate the impact of peers' female student proportion and peers' parental education on student academic achievement. Chapter 3 examines the impact of female educational attainment on fertility in Malawi. In chapter 4, I study whether maternal education has an impact on child mortality in Indonesia. Chapter 5 summarizes the findings.

1.1. Classroom Composition and Student Academic Achievement: The Impact of Peers' Parental Education and Peers' Gender

Since the Coleman Report (1966) highlighted that the socioeconomic status of students' peers was one of the important predictors of students' academic achievement, there has been a large body of studies indicating that individual academic achievement could be influenced by classroom composition such as gender, race, academic ability, and exposure to domestic violence (Booij et al., 2017; Carrell and Hoekstra, 2010; Hanushek et al., 2003; Hoxby, 2000; Lavy and Schlosser, 2011; Sacerdote, 2001). In contrast, relatively little is known about the role of peers' socioeconomic background. Only a few studies explicitly examined the effects of peers' socioeconomic status on students' educational achievement and explored the potential mechanisms of the classroom spillovers (Bifulco et al., 2011; Fruehwirth, 2016; Haraldsvik and Bonesrønning, 2014).

In this paper, I exploit the randomized student assignments to examine the existence and potential pathways of the impact of peer parental education and peer female proportion on educational outcomes in Chinese middle schools. Specifically, utilizing the information provided by school principals and head teachers, I identify schools in which students are randomly allocated to classrooms. Then I investigate whether having more students with higher parental education and more female students in the classroom has any impact on students' test scores.

Investigating the magnitude and potential nonlinear patterns of the peer effects in terms of family socioeconomic background and female student proportion would help policymakers achieve improved outcomes at lower costs by optimally grouping students in different classrooms. This is more crucial for developing countries, where limited government budget requires more efficient allocation of educational inputs, implying that even small effects should not be neglected.

The results suggest that having peers with high-educated parents positively affects both male and female students' scholastic performance as measured by test scores. In addition, having a higher proportion of female peers in the classroom improves male students' test scores. I also find that peer effects work in a heterogeneous pattern: students who have a higher parental education benefit more from peer groups characterized by higher levels of parental education compared to students with medium- or less-educated parents. The estimates from using data of schools where students are not randomly assigned to classrooms indicate that neglecting the nonrandom student assignment within schools would induce severe upward bias in estimated peer effects. An exploration of the potential mechanisms shows that higher peer parental education may improve students' academic outcomes through academic quality of peers, students' perception of the classroom atmosphere, and behaviors of students' classroom friends, but these channels are heterogeneous across genders. There is no evidence that the parental education and the proportion of female students at the classroom level influence teachers' weekly working hours and pedagogical methods.

1.2. The Impact of Female Education on Fertility: Evidence from Malawi Universal Primary Education Program

Education not only plays a significant role in shaping individuals' economic well-being but also has important implications for their nonpecuniary outcomes (Oreopoulos and Salvanes, 2011). The relationship between female education and demographic outcomes is an important one, which bears significant policy implications. Many of the existing studies on the fertility

effect of education are obtained from developed countries, while researches on the causal role of education in fertility outcomes in the context of developing countries, especially sub-Saharan countries are still scant. Sub-Saharan countries have the highest fertility rate in the world. Women in sub-Saharan Africa have about five children over their reproductive lifetime, compared to a global average of 2.5 children (United Nations, 2015). Thus, investigating the relationship between female education and reproductive behaviors has important implications for policies that focus on reducing the fertility rate to more sustainable levels in sub-Saharan countries.

Previous studies have shown an inverse relationship between female education and the number of children using data from sub-Saharan African countries (Ainsworth et al., 1996; Kravdal, 2002). It is not clear, however, whether this relationship is causal because the omission of unobserved confounding variables such as cognitive ability that is associated with both education and fertility behaviors may lead to a biased estimate of the relationship. To circumvent the problem of endogeneity of education, this paper exploits an exogenous increase in female education generated by the Universal Primary Education (UPE) policy in Malawi to examine the impact of female education on fertility and other demographic related behaviors.

Utilizing data from the Demographic and Health Surveys (DHS) and the Population and Housing Census (PHC) of Malawi, the paper presents evidence that the policy of removing primary school fees has an economically meaningful impact on educational attainment for women living in rural areas but almost no effect on urban females. The policy increased rural women's schooling by nearly 0.42 years. An additional year of female education decreases the number of children ever born and the number of living children by 0.39 and 0.34, respectively. Furthermore, using the rich set of information provided by the DHS, I complement the findings with a detailed analysis of the potential mechanisms driving the reduction in fertility observed after the reform. The mechanism analysis shows that female education influences fertility by

changing women's desired number of children, delaying women's age at first marriage and age at first birth. I find no evidence that female education improves the quality of the spouse, women's labor market participation, occupation, or modern contraceptive use.

1.3. The Effect of Maternal Education on Child Mortality in Indonesia

Education not only has a direct impact on individuals' own outcomes including income, health, and cognitive ability, but also plays critical role in determining offspring's health (Breierova and Duflo, 2004; Chou et al. 2010; Currie and Moretti, 2003; Grossman, 2006) and cognitive outcomes (Andrabi, Das, and Khwaja, 2012; Dickson, Gregg, and Robinson, 2016). However, the evidence on the impacts of parental education on child health, especially on child mortality is still scarce in lower-middle-income countries. There are several studies that have found a causal relationship between parental education and child health, but only a few of these studies have been able to identify the potential mechanisms underlying it (Currie and Morretti 2003; Grépin and Bharadwaj, 2015; Keats, 2018).

The study investigates whether female education has a causal impact on child mortality using a natural experiment that comes from an exogenous one-time extension of school year that took place in Indonesia in 1978. Academic years in Indonesia used to start in January and to end in December the same year. In mid-1978, the Indonesian government decided to change the start of the school year from January to July, and schools are required to extend the 1978 school year until June of 1979, so that the 1978 academic year was extended by an extra six months. I take advantage of the exogenous variation in the length of school year to study the effect of women's years of education on a range of measures of child mortality.

Using data from the Indonesia Demographic and Health Survey (DHS), the paper shows that the extension of the school year in 1978 in Indonesia leads to a remarkable increase in women's educational attainment. Exploiting this education policy as an instrument for years of completed education, I find evidence that female education has a significant impact on reducing

the risk of child death during the neonatal period; an extra year of education leads to a reduction in neonatal mortality by 0.8 percentage points, which corresponds to a 26% reduction. However, I find little evidence that female education affects infant and under-five mortality.

The mechanism analysis suggests that increased female education raises women's age at first marriage and age at first birth, and decreases the likelihood of getting married and giving birth at teenage age, while higher female education does not significantly influence women's total number of children and fertility preference. Additionally, highly educated women are more likely to have a younger and more educated spouse and have more financial resources. The study also provides evidence that highly educated women are less likely to smoke and more likely to receive prenatal care from a skilled provider, to be assisted by skilled birth attendants at childbirth, and to deliver in a health facility.

CHAPTER 2. CLASSROOM COMPOSITION AND STUDENT ACADEMIC ACHIEVEMENT: THE IMPACT OF PEERS' PARENTAL EDUCATION AND PEERS' GENDER

2.1. Introduction

It is generally acknowledged that a student's academic performance is affected not only by the student's own characteristics such as academic ability, study effort, and family background, but also by attributes and behaviors of his/her peers. Understanding the impact of social interactions on individual's learning outcomes in school is important for educators, parents, and policymakers. Since the Coleman Report (1966) highlighted that the socioeconomic status of students' peers was one of the important predictors of students' academic achievement, there has been a large body of studies indicating that individual academic achievement could be influenced by classroom composition such as gender, race, academic ability, and exposure to domestic violence (Booij et al., 2017; Carrell and Hoekstra, 2010; Hanushek et al., 2003; Hoxby, 2000; Lavy and Schlosser, 2011; Sacerdote, 2001). In contrast, relatively little is known about the role of peers' socioeconomic background. Only a few studies explicitly examined the effects of peers' socioeconomic status on students' educational achievement and explored the potential mechanisms of the classroom spillovers (Bifulco et al., 2011; Fruehwirth, 2016; Haraldsvik and Bonesrønning, 2014).

In this paper, I use the China Education Panel Survey (CEPS) 2013-2014, a national level representative survey of middle school students and exploit the randomized student assignments to examine the existence and potential pathways of the impact of peer parental education and peer female proportion on educational outcomes in Chinese middle schools. Specifically, utilizing the information provided by school principals and head teachers, I identify schools in which students are randomly allocated to classrooms. Then I investigate whether having more students with higher parental education and more female students in the classroom has any impact on students' test scores. The source of plausibly exogenous variation

has been used in previous studies. For example, Gong et al. (2018) use the same data and research setting to investigate the impact of teacher's gender on students' academic outcomes.

A large body of empirical research has shown that a higher percentage of female students in the classroom would improve the academic achievement for both male and female students or either genders (Eren, 2017; Lavy and Schlosser, 2011; Lu and Anderson, 2015). However, the results of previous research are not conclusive. There are a few studies suggesting that boys may learn less with a higher female population in the classroom since boys are easily distracted by opposite-gender peers (Black et al., 2013; Lee et al., 2014). The effect of peer parental education is not clear, either. Interactions in classrooms with peers with higher parental education may transmit positive attitudes toward school, improve homework habits, and even academic skills, which in turn influences students' attitudes and behaviors and other educational outcomes. An alternative view holds that the presence of classmates with high socioeconomic backgrounds will harm the less-advantaged students' performance because the latter may compare themselves with better-off ones and this comparison would weaken their self-confidence, ultimately having a negative effect on their academic achievement (Mayer, 2002). Thus, the total extent to which peer parental education and peer female proportion matter at the classroom level is a question that needs further empirical analysis.

Investigating the magnitude and potential nonlinear patterns of the peer effects in terms of family socioeconomic background and female student proportion would help policymakers achieve improved outcomes at lower costs by optimally grouping students in different classrooms. This is more crucial for developing countries, where limited government budget requires more efficient allocation of educational inputs, implying that even small effects should not be neglected. It is also worth pointing out that the institutional structure of middle schools in China is considerably different from that of the United States. Middle school students change classrooms many times throughout the day in the U.S. In most Chinese middle schools,

however, students typically stay in a fixed classroom for each subject throughout the middle school years, and teachers rotate among classrooms.[1] This arrangement indicates that students' classroom peers are relatively stable.

This paper makes four contributions to the literature on peer effects. First, it contributes to the literature on the empirical analysis of peer effects by adding new evidence on the peer effects of family socioeconomic background and female student proportion on students' academic achievement. The responses of school principals and head teachers to the questionnaire in the survey allow me to identify schools that assign students randomly and to avoid the concern caused by the endogenous sorting of students into classrooms within schools. Second, the paper adds to the literature that investigates peer effects at the classroom level. The identification strategy used in this paper allows me to examine the peer effects at the classroom level instead of the grade level. Students in Chinese middle schools commonly stay in the same classrooms for most courses throughout the middle school years. This feature of educational institutions in China indicates that students are more likely to interact with peers inside the classroom. Thus, the peer composition at the classroom level would be a better approximation of peer groups in which students interact daily. As pointed out by Burke and Sass (2013), it is critical to identify the salient peer group in the estimation of peer effects. Third, the present study exploits a large scale and national sample of middle school students in China, the results are thus more representative from a policy point of view in the setting of Chinese middle schools. Finally, this paper also adds to the relatively scant literature on the analysis of potential pathways through which peer parental education operates. Prior studies have shown that peer parental education may work through the channel of parental class involvement, teachers' effort, and information spillovers (Bifulco et al., 2011; Fruehwirth, 2016). The rich set of

[1] Unlike American schools that provide electives, students in China are often required to take the same classes in middle schools. Another important difference is the class size. Classes are typically around 45-60 students in Chinese middle schools, while class sizes of American middle schools range from 25 to 30.

information in the survey allows me to examine a wide range of channels including students' academic performance, students' perception of classroom atmosphere, behaviors of students' friends, students' studying effort, and teachers' teaching methods.

The results suggest that having peers with high-educated parents positively affects both male and female students' scholastic performance as measured by test scores. One additional year of average peer parental education increases students' test scores by 0.152 standard deviations for male students and 0.138 standard deviations for female students. In addition, having a higher proportion of female peers in the classroom improves male students' test scores. Specifically, a 10 percentage point increase in female student proportion in the classroom raises male students' test score by 0.141 standard deviations. The paper also finds that peer effects work in a heterogeneous pattern: students who have a higher parental education benefit more from peer groups characterized by higher levels of parental education compared to students with medium- or less-educated parents. The estimates from using data of schools where students are not randomly assigned to classrooms indicate that neglecting the nonrandom student assignment within schools would induce severe upward bias in estimated peer effects. An exploration of the potential mechanisms shows that higher peer parental education may improve students' academic outcomes through academic quality of peers, students' perception of the classroom atmosphere, and behaviors of students' classroom friends, but these channels are heterogeneous across genders. There is no evidence that the parental education and the proportion of female students at the classroom level influence teachers' weekly working hours and pedagogical methods.

The remainder of this paper proceeds as follows: Section 2 reviews the relevant and recent studies on peer effects. Section 3 describes the features of the dataset. Section 4 lays out the empirical strategy. Section 5 shows the regression results and Section 6 presents the evidence on the potential mechanisms underlying the peer effects. Section 7 forms the conclusion.

2.2. Literature Review

The impacts of peers' characteristics on individual academic achievement has been extensively investigated in the literature, and researchers have tried a variety of estimation strategies to disentangle the causal peer effects on students' academic performance.[2] A challenge in revealing peer effects is the nonrandom selection of students into classrooms within schools since students with similar characteristics such as ability, tend to congregate in the same classroom (Hoxby, 2000). As some observable and unobservable factors could be correlated with both students' academic achievement and their characteristics, failure to account for the nonrandom sorting of students would result in biased estimates of peer effects. To deal with the potential sorting and self-selection problem, some studies investigate peer effects by using quasi-experimental strategies, which utilize idiosyncratic variations across cohorts within schools. Hoxby (2000) exploits the idiosyncratic variation of the female proportion across cohorts within schools to examine peer effects on account of gender and race in primary and middle schools. Lavy and Schlosser (2011) use data from Israeli elementary, middle, and high school to investigate the gender peer effects on students' cognitive outcomes and behaviors by using variations in gender composition across adjacent cohorts within schools. Bifulco et al. (2011) utilize the Add Health dataset to test the effect of the fraction of disadvantaged minority groups and college educated mothers on the likelihood of students dropping out of high school, and other post-secondary outcomes. Black et al. (2013) draw on the idiosyncratic variation in cohort socioeconomic background composition of middle school students in Norway to examine peer effects on long-run outcomes.

The second strand of the identification strategy utilizes individual fixed effects and variations in peer composition over time. For example, Hanushek et al. (2003) exploit a panel dataset and control for student and school-by-grade fixed effects to estimate peer effects on

[2] See Mansiki (1993), Hanushek et al. (2003), and Sacerdote (2011) for comprehensive discussion.

academic achievement in primary school. Burke and Sass (2013) control for student and teacher fixed effect to investigate the impact of classmates' academic ability on mathematics and reading achievement for Florida public school students.

Another strand of literature makes use of the identification strategy that exploits randomization of student assignment. Eren (2017) uses the randomized data from TFA (Teach for America) and TNTP (The New Teacher Project) to obtain estimates of the effect of peer female proportion and peer academic ability on high school students' outcomes in the context of disadvantaged neighborhoods. Booij et al. (2017) examine the effect of peer ability on academic achievement of undergraduate students at the University of Amsterdam. Feld and Zölitz (2017) investigate the effect of high-achieving peers using the data of students from the School of Business and Economics at Maastricht University.

This paper is related to a handful of studies on the effects of peers' family socioeconomic background. For example, McEwan (2003) finds that the mean education of mothers in the classroom is positively associated with students' academic achievement using the data of eighth-grade students in Chile. Ammermueller and Pischke (2009) use the number of books as a proxy variable for family background, and they find a significant and positive relationship between peers' socioeconomic background and test scores by employing the data of fourth graders in six European countries. Bifulco et al. (2011) find that a higher fraction of students with college-educated mothers in a student's cohort is associated with lower rates of dropping out of high school and higher college attendance rates.

Finally, this paper is also closely related to the existing literature of gender peer effects. Although there is a handful of studies that investigate the effect of the proportion of female students on students' academic achievement, the results are not conclusive. For example, Oosterbeek and van Ewijk (2014) find little evidence of gender peer effects using the dataset of undergraduates at the University of Amsterdam. Lavy and Schlosser (2011) show that the

proportion of girls improves both boys' and girls' cognitive outcomes. Lu and Anderson (2015) document that a student benefits only when he or she is surrounded by more students of the same gender. Eren (2017) finds that gender peer effects are more significant for girls, while Hill (2017) finds to the contrary and shows that gender peer effects are pronounced only for male students.

2.3. Institutional Background and Data

Education in China is a public education system run by the Ministry of Education, which is responsible for setting the national curriculum standards and determines guidelines on curriculum management as well as lesson hours at the national level. Schools could adjust the curriculum plan according to their local context, but this work will be guided and supervised by the local education bureau. According to the Compulsory Schooling Law enacted in 1986, students are required to attend school for a nine-year period, which is made up of six years of primary education and three years of middle school education. Students would be enrolled into middle schools based on their official household registration after graduating from primary school, which is under the surveillance of local governments. Students typically remain in the initial classroom throughout their middle school years.

Due to the promotion of equal and fair opportunity for all students by the Ministry of Education, there is a growing number of schools that have begun to employ the random assignment to place students to classrooms in China. To assign students randomly to classrooms, schools commonly utilize a computer program which incorporates information on students' demographic characteristics, class size, and other dimensions. Then, homeroom teachers and subject teachers will be assigned to classrooms by drawing lots. (Gong et al., 2018).

The data used in this paper are from the China Education Panel Survey (CEPS) 2013-2014, conducted by the National Survey Research Center (NSRC) at Renmin University of China.

The CEPS is the first large-scale nationally representative survey which employs a stratified and multistage sampling design with a probability proportional to size sampling (PPS) method, and is designed to document educational processes and transitions by which students progress through different educational stages and to investigate the effects of educational outcomes during students' lives. Each subsample in the CEPS is drawn through county, school, and classroom level. Two classrooms are randomly selected from grades 7 and 9 in each selected school. The survey covers about 20,000 students in 438 classrooms across 112 middle schools in 28 counties in China during the 2013-2014 academic year.

The CEPS collects information on students' demographic characteristics and family socioeconomic background, such as parental education and family income level. It also contains administrative school records on students' midterm exam test scores in Chinese, Math, and English. In the curricular system of Chinese middle school, Chinese, Math, and English are compulsory courses for every student in middle schools across the whole country. These courses are also the main components of examination for admission to a senior high school. Therefore, the main outcome variable of interest in this study is the normalized average midterm exam test scores for Chinese, Math, and English in the Fall of 2013.

An advantage of the CEPS is that it collects information on how students are assigned to classrooms from school principals and homeroom teachers. School principals are asked to report the assignment methods they used to place students. The option includes (i) pre-enrollment test scores, (ii) students' household registration locations, (iii) random assignment, and (iv) other methods. I restrict the study sample to schools that use (iii). Homeroom teachers were asked whether students in their grade were allocated to classrooms by test scores. I exclude the entire grade if any head teacher in a grade responded "yes." I also exclude students who have missing values on test scores, gender, parental education and other predetermined variables. These conditions result in an estimation sample of 7,647 students in 204 classrooms

in 66 schools.[3] According to the above criteria, about 59% of schools randomly assign students to classrooms in the CEPS.

Table 2.1 presents the summary statistics on the main outcome variables and characteristics of students including gender, age, race, number of siblings, hukou status (local household registration), parental education, whether the student has ever attended kindergarten, and whether the student had the experience of retention while in primary school. Column (1) reports the statistics for the whole sample, and columns (2) and (3) report the statistics for male and female students, respectively. The summary statistics show that male and female students are quite similar in most of those predetermined characteristics, but female students have better academic performance than males, especially in Chinese and English.

2.4. Empirical Strategy

To investigate the reduced form relationship between the peer characteristics and students' scholastic outcomes. I use the following regression model:

$$Y_{isgc} = \alpha + \beta_1 Peerpeduc_{-isgc} + \beta_2 Peerfemale_{-isgc} + \phi X'_{isgc} + \tau_{sg} + \epsilon_{isgc} \quad (1)$$

where Y_{isgc} is the academic achievement of student i in school s, grade g, and classroom c; $Peerfemale_{-isgc}$ is the proportion of female students in student *i*'s classroom excluding student *i*; $Peerpeduc_{-isgc}$ is the average years of parental education in student *i*'s classroom excluding student *i*. Parental education is defined as the highest years of education achieved by either parent of the student. The error term is represented by ϵ_{isgc}. X'_{isgc} is a set of student and teacher controls. Student controls include age, gender, number of siblings, parental education, and dummy variables indicating race, hukou status, whether attended kindergarten, and whether repeated a grade in primary school. Teacher controls include teachers' gender, years

[3] To have enough variation of peer measures within grades in the same schools, I also drop the class that does not have sibling classes in the same grade and school.

Table 2.1. Summary Statistics of Students

Variable	Variable Definition	All (1)	Male (2)	Female (3)
Female	=1 if a student is female =0 otherwise	0.495 (0.50)	-	-
Age	Age in years	13.43 (1.22)	13.46 (1.23)	13.40 (1.21)
Minority	=1 if a student belongs to a minority ethnic group; =0 otherwise	0.10 (0.31)	0.10 (0.30)	0.11 (0.31)
Local	=1 if the hukou of student is local =0 otherwise	0.80 (0.40)	0.80 (0.40)	0.81 (0.40)
Kindergarten	=1 if a student attended kindergarten =0 otherwise	0.82 (0.38)	0.81 (0.39)	0.83 (0.38)
Retention	=1 if a student repeated a grade in primary school; =0 otherwise	0.10 (0.29)	0.11 (0.31)	0.08 (0.27)
Siblings	Number of siblings	0.61 (0.79)	0.58 (0.80)	0.66 (0.79)
Parent Education	Years of parental education	11.38 (3.20)	11.33 (3.23)	11.44 (3.16)
Chinese Score	Chinese test score	83.93 (20.11)	80.46 (21.24)	87.47 (18.22)
Math Score	Math test score	81.61 (31.44)	79.95 (32.23)	83.32 (30.53)
English Score	English test score	83.37 (29.96)	77.61 (31.61)	89.24 (26.95)
Test Score	Average of Chinese, Math, and English test scores	82.97 (24.83)	79.34 (26.00)	86.67 (23.00)
Peer Parental Educatic	Years of education of student peers' parents	11.38 (2.03)	11.33 (2.04)	11.44 (2.02)
Peer Female Proportio	Proportion of female peers	0.495 (0.087)	0.493 (0.090)	0.497 (0.083)
Observations		7,647	3,880	3,767

Notes: Each cell contains the mean with the standard deviation in parentheses.

of education, and years of experience.[4] The term τ_{sg} represents school-by-grade fixed effects controlling for the endogenous sorting of students across schools based on students' observed and unobserved characteristics. I cluster the standard errors at the school-by-grade level to allow for correlation across students within the same grades and schools. The results do not change if I cluster the standard errors at the school level. To simplify interpretation, I standardize students' test scores to a mean of zero and a standard deviation of one by school and grade.

[4] The descriptive statistics of teachers are shown in Table A.1.

The coefficients of interest are β_1 and β_2, which represent the impact of peers on academic outcomes.[5] Self-selection problem would arise if students are placed in classrooms by certain observed or unobserved factors (Manski, 1993). If this is the case, β_1 and β_2 may reflect the sorting of students with certain characteristic rather than peer effects, which might lead to overestimation of the coefficients of the peer composition measures. To address the potential self-selection problem caused by the non-random assignment of students, I focus on the schools in the dataset that randomly assign students to classrooms, which has been described in the last section. The random assignment of students ensures that there is no self-selection of students to classrooms in the same grade of schools. Consequently, $\widehat{\beta_1}$ and $\widehat{\beta_2}$ would be unbiased estimators of peer effects.

As the information on parental educational attainment is self-reported, another potential problem that could bias the estimates is the measurement error of peer parental education.[6] Feld and Zölitz (2017) analyze how measurement error impacts the estimation of compositional peer effects and they show that measurement error will only attenuate the estimate if peer groups are assigned randomly. Consequently, the estimates of peer parental education investigated in this paper present a lower bound of peer effects.

The students' peer parental education and peer female proportion are measured at the classroom level. In a typical middle school in China, students in grade 7 (the starting grade of middle school) are assigned to classrooms before the semester begins. Once assigned, there is

[5] Manski (1993) classifies three types of peer effects: correlated effects, endogenous effects and exogenous effects. Correlated effects arise when similar individuals are assigned into classrooms according to their characteristics. According to Lu and Anderson (2015), the correlated effects could be eliminated by random assignment. Endogenous effects occur if the achievement of peers affects a student's achievement, that student may also have an impact on her peers. In this paper, I will focus on peer effects that combines both the endogenous and exogenous effects without attempting to separate them.

[6] Compared to the students' parental education, measurement error is less likely to be a concern about students' gender.

no reassignment afterwards, which facilitates classmates' interaction in classrooms.[7] Burke and Sass (2013) find that peer effects tend to be more significant at the classroom level relative to the grade level, which implies that the broader peer composition measures (e.g., grade level) may not represent the composition of peer groups in which students interact in school (Hill, 2017). Ammermueller and Pischke (2009) also note that classes are the basic unit where learning takes place and students spend more time with their classmates, while the time they interact with their other schoolmates is rather limited. Therefore, the peer composition at the classroom level should be an appropriate proxy for peer interactions in schools.

2.5. Results

2.5.1 Random Assignment Test

In order to make a causal interpretation of the estimates, the differences in students' predetermined characteristics across classrooms should be uncorrelated with the variation in classroom composition within the same grades and schools. For example, students who have different peer parental education in their classrooms should be similar in observed predetermined characteristics if students are indeed randomly assigned to classrooms in a grade within schools. To examine the randomness of student assignment for the sample, I follow the method of Bifulco et.al (2011) by regressing students' characteristics on students' peer measures controlling for school-by-grade fixed effects. Table 2.2 reports the estimated coefficients from 6 separate regressions of various student characteristics on peers' female proportion and peers' parental education separately. The estimated coefficients show that only two out of 12 coefficients are significantly different from zero.

[7] This arrangement is standard for middle schools in China and is different from the education system in other western countries where middle school students change classrooms quite often during the school day (Lu and Anderson 2015).

Another concern is that school administrators may allocate teachers to classrooms according to the classroom compositions, even if students are randomly assigned to classrooms. Table A.2 presents the results of randomization test for a set of teachers' characteristics including gender, years of education and years of experience. The results indicate that students' peer parental education and peer female proportion are also uncorrelated with teachers' attributes, which lends further support to the identification assumption that students are randomly assigned to classrooms within the same grades and schools in the study sample.

Table 2.2. Random Assignment Test

Dependent Variable	Peer Female Proportion	Peer Parental Education	F-Statistic	p-value
Age	0.150	-0.045**	2.593*	0.080
	(0.159)	(0.022)		
Minority	0.024	-0.005	0.407	0.667
	(0.042)	(0.008)		
Local Hukou	-0.020	0.010	0.276	0.759
	(0.157)	(0.016)		
Number of Siblings	-0.236	-0.045	1.932	0.150
	(0.186)	(0.031)		
Kindergarten	0.007	0.028	1.063	0.349
	(0.133)	(0.020)		
Retention	0.004	-0.020*	1.969	0.145
	(0.079)	(0.011)		

Notes: All specifications include school-by-grade fixed effects. Standard errors are clustered at school-by-grade level and reported in parentheses. The F-statistics is for the joint effect of peer female proportion and peer parental education. ***, **, and * denote significance at the 1, 5 and 10 percent level, respectively.

2.5.2 Extent of Variation in Students' Characteristics

The identification strategy of peer effects in this paper relies on the variation in students' characteristics across classrooms within the same grades and schools. Before estimating the impact of peer effects, I investigate whether there is enough variation in the sample so that the results are not likely to be driven by extreme values of classroom composition. Figures A.1 and A.3 display the distribution of the female student proportion and parental education at the classroom level. Figures A.2 and A.4 present the distribution of the within grade-by-school

standard deviation of these two classroom composition measures and these figures display nonnegligible variations. Additionally, following the method used by Ammermueller and Pischke (2009) I decompose the variation in the female student proportion and students' parental education. Table A.3 shows that the variation within grade-by-school accounts for about 25% of the total variation in fraction of female peers and 4.25% of the total variation in students' parental education.

2.5.3 Baseline Estimation Results

Table 2.3 reports the estimated effects of peer parental education and peer female proportion on students' test scores by estimating various specifications of Eq. (1). Each column represents a separate regression. To facilitate interpretation, test scores are normalized by school and grade to a mean of zero and a standard deviation of one. All specifications include school-by-grade fixed effects. Columns (2), (5), and (8) present the estimates of peer effects with student characteristics. Columns (3), (6), and (9) report the estimates with both student and teacher characteristics. Columns (1), (4), and (7) present the results without student and teacher controls as comparison.

If the variation in peer measures in the same grades and schools is exogenous to students' and teachers' characteristics, then the estimated coefficients of peer effects should remain relatively unchanged if I include more student and teacher covariates in the regression. The estimated coefficients in the first three columns of Table 2.3 are similar in magnitude, which supports the validity of the identification assumption. The estimated coefficient using complete specification in Column (3) suggests that if the peer parental education in a classroom increases by one year, students' test scores would increase by 0.144 standard deviations and that if the female student proportion increases by 10 percentage points, students' test scores would

Table 2.3. Estimates of Peer Effects on Students' Academic Achievement

	All			Male			Female		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Peer Parental Education	0.159***	0.147***	0.144***	0.169***	0.155***	0.152***	0.152***	0.142***	0.138***
	(0.038)	(0.035)	(0.034)	(0.050)	(0.047)	(0.046)	(0.035)	(0.033)	(0.032)
Peer Female Proportion	0.642*	0.634*	0.624*	1.411***	1.428***	1.413***	0.023	0.012	0.011
	(0.369)	(0.366)	(0.365)	(0.410)	(0.409)	(0.414)	(0.389)	(0.392)	(0.392)
Student Control	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Teacher Control	No	No	Yes	No	No	Yes	No	No	Yes
Observations	7,647	7,647	7,647	3,880	3,880	3,880	3,767	3,767	3,767
R-squared	0.070	0.084	0.085	0.046	0.062	0.062	0.063	0.076	0.077

Notes: Test scores are standardized by grade and school. Student control variables include student's age, gender, number of siblings, parental education, and dummy variables indicating race, hukou status, whether attended kindergarten, whether repeated a grade in primary school. Teacher control variables include teacher's gender, years of experience, and years of education. Peer parental education and peer female proportion are measured at classroom level. All specifications include school-by-grade fixed effects. Standard errors are clustered at the school-by-grade level and reported in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

increase by 0.062 standard deviations. Both these estimates are statistically significant at the conventional level.

To examine the heterogeneous effects of peer measures on the test scores for male and female students, I divide the sample into two parts by gender. Columns (6) and (9) present that peer parental education has similar effects for male and female students, the estimated coefficients are 0.152 and 0.138 respectively and both statistically significant at 1% level. The estimated coefficient of peer female fraction, however, is statistically significant only for male students. Specifically, if the female proportion increases by 10 percentage points, male students' test scores would increase by 0.141 standard deviations.

The above results come from using data of students who are randomly assigned to classrooms in schools. To test the potential direction of bias caused by the nonrandom assignment of students, I re-estimate Eq. (1) using data from schools where students are not randomly assigned to classrooms. Table A.4 reports the results. Compared to the baseline results using random assignment of students, the magnitudes of estimated coefficients of peer parental education and peer female proportion increase by 43% and 350% for the whole sample, which suggests that failing to account for nonrandom assignment of students within schools would lead the estimates to be biased upward. For example, if students are grouped by test scores within schools and female students perform better than male students (as suggested by the summary statistics), the effect of the proportion of female peers would be overestimated without accounting for the endogenous sorting within schools. The validity of the baseline results relies on the random assignment of students to classrooms, which is supported by the balance test of student baseline characteristics. However, one may concern that the baseline results could be biased because school principals may report conducting a random assignment but did not place students randomly. To alleviate this concern, following the approach used by Gong et al. (2018), I randomly drop schools from the sample and see whether regression results

change dramatically. The estimates should not be expected to deviate from the baseline estimates significantly if the baseline study sample mainly contains schools conducting random assignment. To keep adequate sample size, I drop two schools each time, and repeat the process 2000 times. Figure A.5 plots the distribution of these estimated coefficients for male and female students. The distributions are centered on the baseline estimates, which indicates that the results are less likely to be contaminated by including schools that may not assign students randomly

The sample used in the baseline analyses includes students both in grade 7 and grade 9. One may argue that students in different grades may be affected differently by peer group composition. To test this possibility, I estimate the Eq. (1) by using the subsample of grade 7 and grade 9, separately. As Table A.5 shows, peer parental education has statistically significant impacts on male and female students. The magnitudes of the estimates of peer female proportion are similar among male students of both grades, although the coefficient is not precisely estimated for 7^{th} male graders. Collectively, the peer measures investigated in this paper have similar impacts on students in different grades.

To investigate the existence of potential heterogeneous effects of peer measures on test scores for different subjects, I regress Eq. (1) using Chinese, Math, and English test scores as dependent variables, respectively. Panel A in Table 2.4 shows that peer parental education and the proportion of female peers both have positive impacts on test scores of all three subjects for the whole sample. Consistent with the results in Table 2.3, Panels B and C in Table 2.4 show that peer female proportion has positive significant impacts only on male students' test score, and the magnitude of the effects is larger for Math and English relative to Chinese. The estimated coefficients of peer parental education on Chinese and Math test scores are similar for male and female students, while the magnitude of the coefficients is relatively smaller in English for female students.

To examine the heterogeneous effects of peer parental education on students with different levels of parental education, I add categorical variables of parental education in the regression by dividing students into three categories according to their parental educational attainment. Specifically, I define parents as high-educated if at least one of the student's parents has a college degree, and define parents as medium-educated if the parents' highest educational attainment is senior high school, and regard parents as less-educated if they only have a middle school degree or less. Additionally, I generate three interaction terms by interacting the above three categorical variables with peer parental education. Table 2.5 reports that students in all three groups of parental education benefit from higher peer parental education and that students

Table 2.4. Estimates of Peer Effects on Chinese, Math and English Scores

	Chinese Score	Math Score	English Score
	(1)	(2)	(3)
Panel A: All Sample			
Peer Parental Education	0.162***	0.155***	0.122***
	(0.037)	(0.043)	(0.041)
Peer Female Proportion	0.310	0.675	0.848**
	(0.380)	(0.429)	(0.384)
Observations	7,647	7,647	7,647
R-squared	0.120	0.048	0.120
Panel B: Males			
Peer Parental Education	0.164***	0.159***	0.149***
	(0.050)	(0.057)	(0.054)
Peer Female Proportion	1.057**	1.513***	1.633***
	(0.481)	(0.476)	(0.447)
Observations	3,880	3,880	3,880
R-squared	0.049	0.064	0.063
Panel C: Females			
Peer Parental Education	0.164***	0.149***	0.098**
	(0.036)	(0.042)	(0.038)
Peer Female Proportion	-0.282	0.002	0.222
	(0.387)	(0.484)	(0.410)
Observations	3,767	3,767	3,767
R-squared	0.069	0.070	0.076

Notes: Test scores are standardized by grade and school. Student control variables include student's age, gender, number of siblings, parental education, and dummy variables indicating race, hukou status, whether attended kindergarten, whether repeated a grade in primary school. Teacher control variables include teacher's gender, years of experience, and years of education. Peer parental education and peer female proportion are measured at classroom level. All specifications include school-by-grade fixed effects. Standard errors are clustered at the school-by-grade level and reported in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

in the top group benefit the most, compared to those in other two groups. The estimated coefficients of peer parental education vary from 0.112 to 0.232 and from 0.134 to 0.154 for male and female students who are in different categories. All these estimated coefficients are statistically different from zero. The different interaction intensity among students in different groups may be one of the reasons for better-off students benefiting more than other groups of students. For example, students with better socioeconomic background tend to interact more with classmates who also have higher family background compared to their disadvantaged counterparts, which makes them maximize the positive externality effect of better peers.

Table 2.5. Heterogeneity of Peer Effects on Students' Academic Achievement

	All		Male		Female	
	(1)	(2)	(3)	(4)	(5)	(6)
Peer Female Proportion	0.624*	0.653*	1.413***	1.402***	0.011	0.060
	(0.365)	(0.365)	(0.414)	(0.411)	(0.392)	(0.390)
Peer Parental Education	0.144***		0.152***		0.138***	
	(0.034)		(0.046)		(0.032)	
Less Educated × Peer Parental Education		0.122***		0.112**		0.134***
		(0.038)		(0.052)		(0.036)
Medium Educated ×Peer Parental Education		0.137***		0.149***		0.129***
		(0.035)		(0.049)		(0.034)
High Educated ×Peer Parental Education		0.187***		0.232***		0.154***
		(0.037)		(0.048)		(0.037)
Observations	7,647	7,647	3,880	3,880	3,767	3,767
R-squared	0.085	0.085	0.062	0.065	0.077	0.077

Notes: Test scores are standardized by grade and school. Student control variables include student's age, gender, number of siblings, parental education, and dummy variables indicating race, hukou status, whether attended kindergarten, whether repeated a grade in primary school. Teacher control variables include teacher's gender, years of experience, and years of education. Peer parental education and peer female proportion are measured at classroom level. High Educated equals to 1 if at least one of the student's parents have a college degree. Medium Educated equals to 1 if student's parents only have a high school degree. Less Educated equals to 1 if student's parents only have a junior high school degree or less. All specifications include school-by-grade fixed effects. Standard errors are clustered at the school-by-grade level and reported in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

2.5.4 Nonlinearities

To investigate whether students benefit from peer parental education in a nonlinear manner, I replace the years of peer parental education with the proportions of peers with high-, medium-,

and less-educated parents in Eq. (1). This specification is similar to the two-way interaction model of Burk and Sass (2013) and Feld and Zölitz (2017). High-, medium-, and less-educated parents are defined the same way as in the previous section. I also add interaction terms to the regression using students' parental education categorical variables and proportional measures of peer parental education. For example, the coefficient of Less Educated × Proportion of Medium Educated can be interpreted as showing how students with less-educated parents are affected by increasing the proportion of students with medium-educated parents, while keeping the proportion of students with high-educated parents constant. Due to the collinearity of the proportions, the proportion of peers with less-educated parents on each individual type constitutes the reference category and will be omitted in the regression.

Column (1) in Table 2.6 shows that if the proportion of peers with high-educated parents increases by 10 percentage points, and the proportion of peers with less-educated parents decreases by 10 percentage points (holding the proportion of peers with medium-educated parents constant), it would cause students' test scores to increase by 0.095, 0.107, and 0.148 standard deviations for students with less-, medium-, and high-educated parents, respectively. Columns (2) and (3) show that male and female students in the middle and bottom group benefit equally, while peer effects are larger among male students in the top group if there is an increase in the proportion of students with high-educated parents. In contrast, an increase in the proportion of peers with medium-educated parents has statistically significant positive impacts only on female students who are in the bottom group.

I also investigate the nonlinear effects of peer female proportion on students' test scores by dividing peer female proportion into three categories by three terciles. The bottom third group serves as the reference group. The nonlinear results show that the impact of peer female proportion rises with the increase in the share of female students for males. Consistent with the findings in the baseline results, peer female proportion does not have a significant impact on

female students. Table 2.7 presents the nonlinear effects of peer parental education and peer female proportion on Chinese, Math, and English test scores, and the results show that the pattern of the nonlinear peer effects is similar among these three subjects.

Table 2.6. Nonlinearity of Peer Effects on Students' Academic Achievement

	All	Male	Female
	(1)	(2)	(3)
Middle Third Female Proportion	0.134	0.171***	0.023
	(0.108)	(0.034)	(0.222)
Top Third Female Proportion	0.172	0.363***	0.001
	(0.129)	(0.096)	(0.231)
Less Educated × Proportion of Medium Educated	0.389	0.312	0.679*
	(0.365)	(0.488)	(0.398)
Medium Educated × Proportion of Medium Educated	0.140	0.094	0.494
	(0.380)	(0.504)	(0.404)
High Educated × Proportion of Medium Educated	0.874*	0.932	0.800
	(0.498)	(0.648)	(0.483)
Less Educated × Proportion of High Educated	0.948***	0.928**	0.958***
	(0.312)	(0.405)	(0.311)
Medium Educated × Proportion of High Educated	1.071***	1.191***	0.967***
	(0.273)	(0.378)	(0.260)
High Educated × Proportion of High Educated	1.478***	1.831***	1.155***
	(0.308)	(0.394)	(0.304)
Observations	7,647	3,880	3,767
R-squared	0.085	0.065	0.077

Notes: Test scores are standardized by grade and school. Student control variables include student's age, gender, number of siblings, parental education, and dummy variables indicating race, hukou status, whether attended kindergarten, whether repeated a grade in primary school. Teacher control variables include teacher's gender, years of experience, and years of education. Peer parental education and peer female proportion are measured at classroom level. High Educated equals to 1 if at least one of the student's parents have a college degree. Medium Educated equals to 1 if student's parents only have a high school degree. Less Educated equals to 1 if student's parents only have a junior high school degree or less. All specifications include school-by-grade fixed effects. Standard errors are clustered at the school-by-grade level and reported in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

By and large, the proportion of peers with high-educated parents has positive effects on all students regardless of their own parental education level, while the effect of increase in the proportion of peers with medium-educated parents is limited. Students with high-educated parents would benefit more from a higher proportion of students with high-educated parents

Table 2.7. Nonlinearity of Peer Effects on Chinese, Math, and English Scores

	All			Male			Female		
	Chinese	Math	English	Chinese	Math	English	Chinese	Math	English
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Middle Third Female Proportion	0.151*	0.162	0.116	0.244***	0.164**	0.137*	0.068	-0.002	0.059
	(0.089)	(0.111)	(0.157)	(0.086)	(0.065)	(0.077)	(0.168)	(0.255)	(0.254)
Top Third Female Proportion	0.173	0.155	0.210	0.386***	0.331***	0.396***	0.033	-0.058	0.067
	(0.110)	(0.137)	(0.172)	(0.144)	(0.125)	(0.118)	(0.175)	(0.272)	(0.260)
Less Educated × Proportion of Medium Educated	0.207	0.552	0.432	0.143	0.735	0.235	0.492	0.630	0.752*
	(0.406)	(0.412)	(0.430)	(0.563)	(0.518)	(0.599)	(0.478)	(0.447)	(0.417)
Medium Educated × Proportion of Medium Educated	-0.150	0.326	0.323	-0.217	0.441	0.247	0.250	0.466	0.679
	(0.444)	(0.435)	(0.436)	(0.602)	(0.551)	(0.602)	(0.478)	(0.479)	(0.448)
High Educated × Proportion of Medium Educated	0.518	1.273**	0.871	0.653	1.389**	0.861	0.383	1.090*	0.861*
	(0.577)	(0.535)	(0.534)	(0.791)	(0.663)	(0.749)	(0.605)	(0.550)	(0.479)
Less Educated × Proportion of High Educated	1.123***	0.933**	0.830**	1.013**	0.811	1.061**	1.234***	1.001**	0.626*
	(0.311)	(0.390)	(0.363)	(0.424)	(0.505)	(0.470)	(0.321)	(0.399)	(0.347)
Medium Educated × Proportion of High Educated	1.213***	1.159***	0.901***	1.334***	1.225**	1.118**	1.119***	1.071***	0.732**
	(0.283)	(0.349)	(0.326)	(0.393)	(0.479)	(0.437)	(0.279)	(0.350)	(0.310)
High Educated × Proportion of High Educated	1.652***	1.593***	1.232***	1.995***	1.838***	1.746***	1.352***	1.307***	0.810**
	(0.341)	(0.376)	(0.343)	(0.454)	(0.463)	(0.455)	(0.340)	(0.399)	(0.322)
Observations	7,647	7,647	7,647	3,880	3,880	3,880	3,767	3,767	3,767
R-squared	0.121	0.050	0.120	0.051	0.068	0.064	0.070	0.070	0.075

Notes: Test scores are standardized by grade and school. student's age, gender, number of siblings, parental education, and dummy variables indicating race, hukou status, whether attended kindergarten, whether repeated a grade in primary school. Teacher control variables include teacher's gender, years of experience, and years of education. Peer parental education and peer female proportion are measured at classroom level. High Educated equals to 1 if at least one of the student's parents have a college degree. Medium Educated equals to 1 if student's parents only have a high school degree. Less Educated equals to 1 if student's parents only have a junior high school degree or less. All specifications include school-by-grade fixed effects. Standard errors are clustered at the school-by-grade level and reported in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

than students of the other two groups, especially for male students. The pattern of nonlinearity obtained in the above results is generally in line with the findings of Burk and Sass (2013) and Fruehwirth (2016).[8] The nonlinearity results imply that more mixing of socioeconomic groups would lead to gains for students in the bottom group, but this is at the cost of better-off students. Nonetheless, reducing socioeconomic segregation in classrooms is still worth considering as more mixing would produce more equality.

2.6. Evidence on Mechanisms

The results presented in the previous section show that both male and female students have better academic performance when they are surrounded by peers with higher parental education and that male students appear to benefit more relative to female students if there are more female classmates in their classroom. These results, however, only show "reduced form" estimates and do not reveal the channels through which peer effects work. Peer parental education could impact students' academic achievement through the spillovers from students' scholastic skills and abilities. It is also possible that high-educated parents are more involved in the education of their children, and thus put pressure on teachers for more effective teaching (Fruehwirth, 2016). Peer female proportion could indirectly affect a student's achievement by altering the classroom atmosphere and teachers' teaching practices (Lavy and Schlosser, 2011). For example, a decrease in the fraction of male students in the classroom may decrease the time teachers spend on disciplining students, thereby increasing the time for teaching classes, if girls have less disruptive behaviors than boys (Lazear, 2001). In this section, I estimate Eq. (1) by exploiting the rich set of survey questions in the CEPS and use the dependent variables constructed from responses of students and teachers to the questionnaires about students'

[8] Burk and Sass (2013) find that high-ability students benefit more than others from an increase in the proportion of high-ability peers. Fruehwirth (2016) find that students with college-educated parents get larger spillovers from having more peers with college-educated parents compared to children with parents who have a high school degree or less.

perception of classroom atmosphere, behaviors of students' friends, students' studying efforts, and teachers' teaching effort and their pedagogical methods.[9] I am aware that the mechanisms analyzed in this paper cannot preclude other possible indirect effects of peers' characteristics on academic achievement, but they can provide some suggestive insights into the potential mediating factors that drive peer effects on students' academic performance.

2.6.1 Peers' Initial Academic Achievement

I start by examining whether peer's academic performance can explain the impacts of peer female proportion and peer parental education. The summary statistics show that female students appear to have higher test scores than males. Moreover, students with higher parental education may also have better academic performance. Therefore, one obvious mechanism is the achievement externalities of female students and students with higher parental education. However, one drawback of the CEPS data is that it does not contain an accurate measure of students' initial academic achievement, such as their test scores in primary school. To circumvent this problem, following Lavy et al. (2011), I use the proportion of repeaters in the classroom as a proxy variable for classmates' initial academic achievement, since the status of being a repeater in primary school is determined before the students' entry into middle schools and is closely correlated with academic performance.[10] The results are shown in Table 2.8. Columns (1), (3), and (5) replicate columns (2), (4), and (6) in Table 2.3 for comparison, and columns (2), (4), and (6) additionally include controls for classmates' repetition status. The results show that the coefficient of peer female proportion does not change significantly, while the coefficients of peer parental education decrease from 0.152 to 0.132 for the male subsample and decrease from 0.138 to 0.124 for the female subsample, which suggest that peers' initial

[9] To facilitate the interpretation, I normalize students' responses to those questions with a mean of zero and a standard deviation of one, but for the questions measured by hours. The descriptive statistics are provided in Table A.1.

[10] I define a student as a repeater if he/she has repeated at least one grade in primary school.

achievement accounts for 13% and 10% of the variation of test scores for male and female students.

Table 2.8. Estimates of Peer effects on Students' Academic Achievement Controlling for Initial Achievement

	All		Male		Female	
	(1)	(2)	(3)	(4)	(5)	(6)
Peer Parental Education	0.144***	0.126***	0.152***	0.132***	0.138***	0.124***
	(0.034)	(0.032)	(0.046)	(0.039)	(0.032)	(0.035)
Peer Female Proportion	0.624*	0.606*	1.413***	1.396***	0.011	-0.009
	(0.365)	(0.353)	(0.414)	(0.416)	(0.392)	(0.374)
Peer Achievement Control	No	Yes	No	Yes	No	Yes
Observations	7,647	7,647	3,880	3,880	3,767	3,767
R-squared	0.085	0.085	0.062	0.063	0.077	0.078

Notes: Test scores are standardized by grade and school. Student control variables include student's age, gender, number of siblings, parental education, and dummy variables indicating race, hukou status, whether attended kindergarten, whether repeated a grade in primary school. Teacher control variables include teacher's gender, years of experience, and years of education. Peer parental education and peer female proportion are measured at classroom level. All specifications include school-by-grade fixed effects. Standard errors are clustered at the school-by-grade level and reported in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Although the peer initial academic performance is not measured perfectly, the results still provide suggestive evidence that peer academic achievement partly explains the positive effects of peer parental education. It is worth noting that there is still a large part of the impact of peer measures not explained by peer retention status, which indicates that peer parental education and peer female proportion could influence students' academic achievement through other indirect channels.

2.6.2 Classroom Atmosphere and Inter-Student Relationship

Besides the effect of peer academic achievement, students with higher parental education are more likely to be well behaved, which could benefit the classroom atmosphere. Additionally, female students are widely believed to be less disruptive than males and thus help create a supportive classroom environment for learning (Lazear, 2001). It is possible that the improved classroom atmosphere and healthy relationships with other classmates caused by changes in classroom composition facilitate students' social interaction and promote the learning process,

thus improving students' performance in tests. To examine this potential pathway, I use the following three items in the student questionnaire related to classroom environment and inter-student relationship as dependent variables: (i) "I feel that the classroom atmosphere is satisfying;" (ii) "I feel that my classmates behave in a kind way to me;" (iii) "I feel close to people in this school." Students are required to evaluate to what extent they agree with these statements on a scale of 1 (strongly disagree) to 4 (strongly agree).

The estimates in Table 2.9 show that higher peer parental education improves male and female students' perception of classroom environment and also positively affects male students' feeling about inter-student relationship. An increase in female student proportion appears to have a positive effect on male students' perception of classroom environment, but the estimates are not statistically significant. Contrary to the findings of Lavy and Schlossser (2011), I do not find supporting evidence that a higher proportion of female classmates would be more helpful to classroom atmosphere.[11]

2.6.3 Behaviors of Students' Classroom Friends

In the last section, I investigate the effect of peer measures on self-assessed classroom environment and inter-student relationships. It is worth noting that the student may not spend the same amount of time on interacting with each of his/her classmates, and is likely to be more interacted with and influenced by his/her friends who are in the same classroom. If a student notices his/her friends caring about school and earning good academic performance, it is likely that he/she will push him- or herself harder academically in order to keep up with friends. Thus, students' social network in the classroom may play a role in influencing students' academic achievement. Previous studies also suggest that friends have a remarkable effect on students' educational outcomes and their involvement in risky behaviors regarding health, such as

[11] One potential interpretation of why male students are more likely to be affected by a higher proportion of female classmates relative to female students is that male students are more prone to adverse effects from competition of the same gender (Hill 2017). But because of data limitation, I cannot empirically examine this potential channel.

Table 2.9. Estimates of Peer Effects on Classroom Atmosphere

	Classroom atmosphere is satisfying	My classmates are friendly to me	I feel close to people in school
	(1)	(2)	(3)
Panel A: All			
Peer Parental Education	0.126**	0.048*	0.086**
	(0.053)	(0.027)	(0.038)
Peer Female Proportion	0.694	-0.116	-0.034
	(0.565)	(0.320)	(0.372)
Observations	7,442	7,440	7,389
R-squared	0.121	0.066	0.101
Panel B: Males			
Peer Parental Education	0.118**	0.085**	0.119**
	(0.057)	(0.037)	(0.048)
Peer Female Proportion	0.957	0.044	0.008
	(0.711)	(0.432)	(0.433)
Observations	3,734	3,740	3,712
R-squared	0.108	0.063	0.097
Panel C: Females			
Peer Parental Education	0.143**	0.007	0.053
	(0.061)	(0.041)	(0.051)
Peer Female Proportion	0.382	-0.230	-0.096
	(0.511)	(0.373)	(0.444)
Observations	3,708	3,700	3,677
R-squared	0.163	0.095	0.133

Notes: All outcome variables are standardized with a mean of zero and a standard deviation of one. Student control variables include student's age, gender, number of siblings, parental education, and dummy variables indicating race, hukou status, whether attended kindergarten, whether repeated a grade in primary school. Teacher control variables include homeroom teacher's gender, years of experience, and years of education. Peer parental education and peer female proportion are measured at classroom level. All specifications include school-by-grade fixed effects. Standard errors are clustered at the school-by-grade level and reported in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

drinking and smoking (Fletcher and Ross, 2018; Lavy and Sand, 2018). The academic performance and behaviors of a student's friends in the same classroom could be affected by the classroom composition, which in turn affects the student's own academic performance. students' social network in the classroom may play a role in influencing students' academic achievement. Previous studies also suggest that friends have a remarkable effect on students' educational outcomes and their involvement in risky behaviors regarding health, such as drinking and smoking (Fletcher and Ross, 2018; Lavy and Sand, 2018). The academic

performance and behaviors of a student's friends in the same classroom could be affected by the classroom composition, which in turn affects the student's own academic performance.

The CEPS ask students to list their 5 best friends and to respond to the following questions: "How many of your best friends mentioned above fit the following description? (i) Doing well in academic performance; (ii) Expecting to go to college; (iii) Skipping classes; (iv) Getting punished for violating school rules; (v) Always fighting with others." The answer to each question is scaled of 1 (None of them) to 3 (Most of them).

The results in Panels B and C of Table 2.10 show that there is a positive relationship between peer parental education and the number of students' friends who have good academic performance for both male and female students. Peer parental education negatively correlates with the number of students' friends who have behaviors harmful to studying. Furthermore, peer female proportion has significant positive impacts on academic performance and educational aspiration of male students' friends. The sign of the estimated coefficient of peer female proportion is mixed for female students and the magnitude of the coefficients is less significant compared to male students. A caveat in the results is that about 30% of students' friends reported by the respondent are not in the same classroom of the respondent.[12] Thus, the relationship between peer measures and behaviors of students' friends is an association relationship, not a causal one.

2.6.4 Study Effort

The CEPS also collects information on how much time students spend on a variety of studying and entertainment activities. To test whether students' study efforts vary with the peer measures, I create variables to measure how many hours per week students devote to studying activities (doing homework assigned by parents or by cram school and reading extra-curricular books)

[12] Students are asked to list 5 best friends and answer whether each of those friends is in the same classroom of the respondent. However, students are inquired to evaluate their friends' behaviors as a whole rather than individually. Therefore, I am not able to tease out students' evaluations about their friends who stay in the same classroom.

Table 2.10. Estimates of Peer Effects on Friends' Behaviors

	Do well in academic performance	Expect to go to college	Skip classes	Punished for violating school rules	Always fight with others
	(1)	(2)	(3)	(4)	(5)
Panel A: All					
Peer Parental Education	0.091**	0.060	-0.059*	-0.079**	-0.071**
	(0.040)	(0.041)	(0.033)	(0.033)	(0.035)
Peer Female Proportion	0.677**	0.964***	-0.453	0.067	-0.077
	(0.326)	(0.343)	(0.283)	(0.281)	(0.267)
Observations	7,484	7,452	7,456	7,474	7,479
R-squared	0.094	0.129	0.076	0.093	0.080
Panel B: Males					
Peer Parental Education	0.125**	0.062	-0.102*	-0.126**	-0.120*
	(0.060)	(0.047)	(0.060)	(0.056)	(0.063)
Peer Female Proportion	0.946**	1.643***	-0.796	-0.417	-0.487
	(0.422)	(0.446)	(0.488)	(0.518)	(0.491)
Observations	3,786	3,770	3,769	3,778	3,780
R-squared	0.094	0.143	0.086	0.087	0.074
Panel C: Female					
Peer Parental Education	0.071*	0.067	-0.021	-0.044**	-0.034
	(0.043)	(0.048)	(0.024)	(0.020)	(0.024)
Peer Female Proportion	0.337	0.242	-0.153	0.314	0.234
	(0.395)	(0.252)	(0.181)	(0.220)	(0.212)
Observations	3,698	3,682	3,687	3,696	3,699
R-squared	0.119	0.062	0.056	0.047	0.046

Notes: All outcome variables are standardized with a mean of zero and a standard deviation of one. Student control variables include student's age, gender, number of siblings, parental education, and dummy variables indicating race, hukou status, whether attended kindergarten, whether repeated a grade in primary school. Teacher control variables include homeroom teacher's gender, years of experience, and years of education. Peer parental education and peer female proportion are measured at classroom level. All specifications include school-by-grade fixed effects. Standard errors are clustered at the school-by-grade level and reported in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

and entertainment activities (watching TV, playing video games, and surfing on the Internet). To investigate the effect of peer measures on habitual absenteeism and tardiness, I use students' responses to the following two questions: (i) "I am always late for classes;" (ii) "I always skip classes." The answer to each question is scaled from 1 (strongly agree) to 4 (strongly disagree).

Panel B of Table 2.11 shows that peer parental education does not affect the time spent on studying significantly, while it negatively affects the time spent on entertainment activities for male students. The female student proportion positively affects the time that male students devote to studying activities including doing homework and reading books. Column (1) in Panel B presents that if peer female proportion increases by 10 percentage points, male students would devote additional 2.4 hours to studying activities per week. Columns (3) and (4) in Panel C suggest that there are negative relationships between peer parental education and habitual tardiness and absenteeism for female students, while the latter is not statistically significant. Lavy and Schlosser (2011) find that the proportion of girls can improve students' cognitive outcomes, which stems from a decrease in disruptive behavior in classes with fewer male students, instead of the changes in students' own learning effort caused by the changes in peer gender composition. In contrast, my findings provide new evidence that the female proportion in the classroom could improve male students' scholastic outcomes by increasing male students' studying effort.

2.6.5 Teaching Efforts and Pedagogical Methods

Teachers' teaching practices might provide another explanation for the externalities of classroom compositions. For example, high-educated parents may care more about their children and communicate more with teachers, which would push teachers to spend more time in preparation for teaching or to adjust their teaching methods.

To test the effects of female student proportion and students' parental education on teacher's teaching efforts, I examine two questions in the survey for teachers: (i) "How many

Table 2.11. Estimates of Peer Effects on Student Study Effort

	Studying	Entertainment	Tardiness	Absenteeism
	(1)	(2)	(3)	(4)
Panel A: All				
Peer Parental Education	0.372	-1.053**	-0.074**	-0.056*
	(0.405)	(0.526)	(0.036)	(0.034)
Peer Female Proportion	15.259***	1.997	-0.106	-0.289
	(4.747)	(8.038)	(0.272)	(0.281)
Observations	7,591	7,229	7,566	7,562
R-squared	0.047	0.106	0.064	0.057
Panel B: Males				
Peer Parental Education	0.045	-1.486*	-0.039	-0.037
	(0.571)	(0.828)	(0.047)	(0.039)
Peer Female Proportion	23.539***	-3.916	-0.172	-0.501
	(8.534)	(12.680)	(0.323)	(0.397)
Observations	3,849	3,667	3,837	3,834
R-squared	0.062	0.094	0.081	0.072
Panel C: Females				
Peer Parental Education	0.647	-0.793	-0.110**	-0.057
	(0.541)	(0.627)	(0.051)	(0.044)
Peer Female Proportion	8.079	5.897	-0.414	-0.274
	(6.539)	(5.179)	(0.409)	(0.331)
Observations	3,742	3,562	3,729	3,728
R-squared	0.066	0.142	0.096	0.073

Notes: Outcome variables in columns (3) and (4) are standardized with a mean of zero and a standard deviation of one. Student control variables include student's age, gender, number of siblings, parental education, and dummy variables indicating race, hukou status, whether attended kindergarten, whether repeated a grade in primary school. Teacher control variables include homeroom teacher's gender, years of experience, and years of education. Peer parental education and peer female proportion are measured at classroom level. All specifications include school-by-grade fixed effects. Standard errors are clustered at the school-by-grade level and reported in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

hours did you work last week?"; (ii) "Do you often discuss the subject you teach with your colleagues?"

To examine the effect of classroom compositions on teachers' pedagogical methods, I examine the following question: "How often do you apply each of the following teaching method (lectures, group discussion, and interaction with students) to the sample class?" Teachers are required to indicate how often they use each teaching method on a scale of 1 (never) to 5 (always).

The estimates in Table 2.12 show that teacher's teaching efforts and pedagogical methods are not significantly affected by students' parental education and gender proportion. This result

is in line with the findings of Feld and Zölitz (2017) and Booij et al. (2017), who also find no evidence that teachers adjust their teaching practices to the composition of the classroom.

Table 2.12. Estimates of the Effects of Classroom Composition on Teachers' Teaching Effort and Pedagogical Methods

	Total Working hours	Discuss the subject with colleagues	Apply the method of lectures	Apply the method of group discussion	Apply the method of interaction with students
	(1)	(2)	(3)	(4)	(5)
Average Parental Education	-0.080	0.005	-0.007	0.006	-0.021
	(0.084)	(0.007)	(0.109)	(0.106)	(0.108)
Average Female Proportion	-0.148	-0.074	-0.959	0.362	0.554
	(0.484)	(0.051)	(0.997)	(0.597)	(0.752)
Observations	585	593	591	595	595
R-squared	0.462	0.228	0.354	0.381	0.261

Notes: All specifications include school-by-grade fixed effects. Teacher control variables include teacher's gender, years of experience, and years of education. Standard errors are clustered at the school-by-grade level and reported in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

2.7. Conclusion

It is widely recognized that peer effects play an important role in educational production, but it has been a challenging task to estimate the impact of peers on educational outcomes because of inherent selection problems and nonrandom assignment of peers. This paper adds to a growing list of studies on peer effects in education by investigating the effects of peer parental education and peer female proportion using data from the China Education Panel Survey (CEPS). Random assignment of students to classrooms within the same grade and school allows to obtain credible estimation of the peer effect.

Peer parental education and peer female proportion in the classroom have positive impacts on students' academic outcomes and the effect of peer female proportion is significant only for male students. Peer effects work in a similar pattern for students in the seventh and ninth grades, with peer parental education improving test scores of both male and female students across different subjects. The results also show that peer effects operate in a heterogeneous and nonlinear manner. Students with college-educated parents benefit more in comparison to those

with relatively lower educated parents if there are more students with college-educated parents in the classroom. The effects of peer female proportion are larger in classrooms with higher proportion of female students. The potential mechanisms of underlying the peer effects are also explored. It is shown that the effect of peer parental education is in part explained by the initial academic performance of classmates and by the improvement of classroom environment and inter-student relationship in the classroom. For male students, peer female proportion and peer parental education affect the academic performance and behaviors of students' classroom friends. Additionally, the peer female proportion improves male students' achievement by increasing their studying effort (e.g., spending more time on doing homework). Finally, gender composition and parental education at the classroom level have no impact on teachers' teaching effort measured by weekly working hours or teaching methods.

The results of this paper have some policy implications for organizing the classroom composition. The study suggests that allocating female students evenly to classrooms in the same grades and schools would be preferable for students' academic achievement. This is because male students' performance is improved by a higher proportion of female students at the classroom level, but female students are not significantly impacted by it. Although a male student benefits by being surrounded by more female classmates, this would reduce the performance of male students in other classrooms with fewer female students. Thus, a balanced allocation of female students across classrooms could spread the positive effects of female students on male students' performance, without impacting female students.

The findings on peer parental education indicate that tracking students by their socioeconomic background would enhance the students' academic performance at the aggregate level. But such a tracking policy would increase the inequality of outcomes between better-off students and socioeconomically-disadvantaged students. If the policy objective is to raise academic achievement among students with lower socioeconomic background, mixing

students with different socioeconomic status is the correct strategy. Furthermore, as pointed out by Vardardottir (2015), if the marginal effect of test scores on long-run outcomes such as income, is higher for students with lower socioeconomic background than their counterparts with higher socioeconomic status, reducing socioeconomic segregation is even more preferable. It is possible that peer female proportion and peer parental education could have unintended consequences on outcomes not studied in the current paper because of data limitation. Therefore, further research on peer effects on outcomes beyond test scores will help build on these insights to craft effective policy to organize the classroom.

CHAPTER 3. THE IMPACT OF FEMALE EDUCATION ON FERTILITY: EVIDENCE FROM MALAWI UNIVERSAL PRIMARY EDUCATION PROGRAM

3.1. Introduction

Many developing countries have adopted widening access to education as a major policy goal as it is widely accepted that investing girl's education has extensive externalities for improving economic and social development. According to Lawrence Summers, former Chief Economist of the World Bank, "educating girls yields a higher rate of return than any other investment in the developing world (Summers, 1994)." Education not only plays a significant role in shaping individuals' economic well-being but also has important implications for their nonpecuniary outcomes (Oreopoulos and Salvanes, 2011). The relationship between female education and demographic outcomes is an important one, which bears significant policy implications.

It is well-documented that women's education is negatively associated with their number of children, but without accounting for the problem of endogeneity of education this relationship may not be causal. Some recent studies have exploited credibly exogenous changes in schooling resulting from education reforms to estimate the causal effect of female education on fertility, but the findings are not consistent (Black et al., 2008; Lavy and Zablotsky, 2015; McCrary and Royer, 2011; Monstad et al., 2008; Osili and Long, 2008).

Many of the existing studies on the fertility effect of education are obtained from developed countries, while researches on the causal role of education in fertility outcomes in the context of developing countries, especially sub-Saharan countries are still scant. Sub-Saharan countries have the highest fertility rate in the world. Women in sub-Saharan Africa have about five children over their reproductive lifetime, compared to a global average of 2.5 children (United Nations, 2015). Thus, investigating the relationship between female education and reproductive behaviors has important implications for policies that focus on reducing the fertility rate to more sustainable levels in sub-Saharan countries.

The purpose of this paper is to investigate the extent to which female education affects fertility in Malawi, which is a small lower-income country in south eastern Africa.[1] Although the mean number of children born alive over a woman's lifetime declined from 6.7 in 1992 to 5.7 in 2010, Malawi continues to register high rates of childbearing and population growth. In the period of 1966-2017, Malawi's population more than quadrupled to 17.4 million. The population will increase to 26 million in 2030 even if the fertility rate declines to 4.6 by 2020 (Population Reference Bureau, 2012). Continued population growth will challenge the country's sustainability of development to meet the Millennium Development Goals, despite the government's current efforts to advance the economic growth.

Previous studies have shown an inverse relationship between female education and the number of children using data from sub-Saharan African countries (Ainsworth et al., 1996; Kravdal, 2002). It is not clear, however, whether this relationship is causal because the omission of unobserved confounding variables such as cognitive ability that is associated with both education and fertility behaviors may lead to a biased estimate of the relationship. To circumvent the problem of endogeneity of education, this paper exploits an exogenous increase in female education generated by the Universal Primary Education (UPE) policy in Malawi to examine the impact of female education on fertility and other demographic related behaviors. The UPE policy is a large-scale, nationwide program designed to increase educational attainment through eliminating fees of primary school throughout the country, thereby making a significant change in the educational opportunities available to Malawian school-age children. Using the UPE policy in Malawi as an instrument for education, I present new evidence on the relationship between female educational attainment and fertility, and additionally examine some potential mechanisms through which female education could have affected fertility.

[1] In 2017, the Gross National Income (GNI) per capital in Malawi is $340, which is lower than $1,025, the threshold between lower-income and lower-middle-income countries (World Bank, 2017).

Utilizing data from the Demographic and Health Surveys (DHS) and the Population and Housing Census (PHC) of Malawi, the paper presents evidence that the policy of removing primary school fees has an economically meaningful impact on educational attainment for women living in rural areas but almost no effect on urban females. The policy increased rural women's schooling by nearly 0.42 years. These educational gains are associated with an increase in the probability of completing primary schooling and at least some years of secondary schooling. An additional year of female education decreases the number of children ever born and the number of living children by 0.39 and 0.34, respectively. Furthermore, using the rich set of information provided by the DHS, I complement the findings with a detailed analysis of the potential mechanisms driving the reduction in fertility observed after the reform. The mechanism analysis shows that female education influences fertility by changing women's desired number of children, delaying women's age at first marriage and age at first birth. I find no evidence that female education improves the quality of the spouse, women's labor market participation, occupation, or modern contraceptive use.

This paper makes two main contributions to the literature. First, it provides new evidence on the causal impact of female education on fertility in Malawi, which adds to the studies on the fertility effect of education in the context of a developing country with high birth rates. While there is a growing literature examining the causal effect of education on fertility outcomes, only a few studies focus on lower-income sub-Saharan African countries using credible estimation strategies. Employing the latest wave of the DHS, the paper shows that higher female education has a negative effect on women's completed number of birth at their mid-30s, which extends the finding of previous researches that has shown that increased female education reduces women's early fertility (e.g., number of children at age 25) using data from Nigeria and Uganda (Osili and Long, 2008; Keats, 2018).

Second, the paper contributes to the literature on demographic effects of education by exploring the potential mechanisms through which female education influences fertility. The mechanism analysis shows that reduced fertility is driven by the reduction in women's desired number of children and the postponement of marriage and motherhood. In contrast to previous empirical studies (Cygan-Rehm and Maeder, 2013; Keats, 2018), education does not affect women's labor market participation and their propensity of having high-paid jobs in Malawi, which indicates that the mechanisms underlying the effects of female education are context specific across countries. The mechanism analysis indicates that the opportunity cost channel associated with female education plays a limited role in reducing fertility in a context where the majority of the population live in rural areas and agriculture is the dominant industry absorbing the female labor force.

The paper is organized as follows. Section 2 reviews the literature. Section 3 provides background on the Malawi UPE program. Section 4 describes the data. Section 5 introduces the empirical strategy. Section 6 presents the results. Section 7 concludes the paper.

3.2. Literature Review

Previous studies in economics suggest that there are a number of potential channels via which female schooling could influence fertility behaviors. Higher female education leads to higher earnings, which should be positively related to fertility because women with higher incomes can afford more children. However, higher female schooling may lead to a decrease in fertility for several reasons. First, increased women's earnings induced by higher education increases the opportunity cost of time-intensive activities, like childbearing and childrearing. As a result, women might substitute away from time-intensive activities to devote more time to labor market participation (Becker, 1981; Willis, 1973). Second, parents with higher education tend to invest more children's human capital, which increases the cost of rearing children. The trade-off between quality and quantity leads parents to choose to have fewer but better-quality

children (Becker and Lewis, 1973).[2] Third, declines in child mortality associated with higher female education may reduce the number of births needed to achieve a given family size. (Lam and Duryea, 1999). Fourth, education may also reduce fertility by increasing women's knowledge about contraception and by making better use of contraceptive devices (Grossman, 1972; Rosenzweig and Schultz, 1989). Furthermore, education may directly lower fertility through the "incarceration effect", which indicates that schooling and marriage are incompatible events since keeping girls in school prevents them from getting married and giving birth (Black et al., 2008). Finally, education may increase women's bargaining power and change their traditional role and status within the family, which enhances women's involvement in fertility decision-making (Mason, 1986; Thomas, 1990).

The major challenge in estimating the effect of education on fertility is that unobserved factors affecting schooling choices are potentially correlated with factors influencing the decision to have children. Some studies utilize plausible exogenous changes in education caused by the expansion of schooling or age-at-entry policies to circumvent the endogeneity problem. The results of those studies using data from industrialized countries are relatively mixed. For example, Black et al. (2008) find that the compulsory schooling laws decrease the incidence of teenage motherhood in the US and Norway. Monstad et al. (2008) exploit the extension of compulsory schooling in Norway in 1959 from 7 to 9 years and find no evidence that more education results in decreased completed fertility although education postpones the age at first birth. Their findings are consistent with Geruso and Royer (2018) who use data from UK and find that female education lowers teen fertility rates but has no impact on completed fertility by age 45. In contrast, McCrary and Royer (2011) find no effect of female education on the timing of first birth exploiting data from Texas and California and using the

[2] The effect could be enhanced by positive assortative mating. Women with more education are likely to be married to a husband with more education (Behrman and Rosenzweig, 2002)

school entry policy as an instrument for education. Employing compulsory schooling reforms as an instrument, Cygan-Rehm and Maeder (2013) find that education reduces completed fertility in Germany, while Fort et al. (2016) find a positive effect of female education on fertility in Continental Europe and a negative effect in England. Lavy and Zablotsky (2015) take advantage of the lifting of travel restriction on Israeli Arabs and find that increased female schooling decreases completed fertility.

In the setting of sub-Saharan Africa, there are only a few studies using quasi-experimental strategies to explicitly investigate the causal effect of education on early fertility (Osili and Long, 2008; Grepin and Bharadwaj, 2015; Keats, 2018). The results of these studies generally suggest a negative relationship between female education and fertility-related outcomes. For instance, exploiting the differences in program exposure by district and birth cohort caused by the introduction of universal primary education in Nigeria, Osili and Long (2008) find that one additional year of female education decreases women's birth at age 25 by 0.26 to 0.48 births. Grepin and Bharadwaj (2015) draw on the expansion of secondary schools in Zimbabwe as a natural experiment to estimate the effect of maternal education on child mortality and a set fertility-related variable. They find that increased education leads to delayed age at marriage and first birth. Using similar empirical strategy, Keats (2018) exploits the primary school fees elimination policy in 1997 in Uganda and finds that women with more schooling delay the timing of first marriage and first birth, and reduce their total number of children at age 25.

3.3. Background

3.3.1. Malawi Universal Primary Education (UPE) Program

Primary school enrollment in Malawi has been steadily increasing since Malawi gained independence in 1964. In 1994, after the first multi-party elections, the new government

introduced the Universal Primary Education (UPE) program to increase the primary education enrollment by eliminating the tuition and fees of primary school for all grades.[3]

Before the introduction of UPE policy, tuition and fees acted as a prominent barrier to education for poor children, girls, rural residents, and other disadvantaged groups in Malawi. In 1993, tuition and fees of primary education averaged 29 kwachas and accounted for 15.5% of the household total cost of primary education, and school uniforms accounted for 60.3% of that cost (Chimombo, 1999).[4] Besides abolishing tuition fees, uniforms were made optional and the government paid for basic textbooks and exercise books fees.[5] Although the cost of attending primary school is low compared to the reported per capita GDP of $120 in 1994, it still represents a considerable cost for the average family given the inequitable distribution of income.[6]

The elimination of primary school fees resulted in a significant response. The primary school gross enrolment rate increased from 83.4% in 1993/1994 to 134% in 1994/95 and the primary school net enrolment rate increased from 71.4% to 95% (Chimombo, 2009).[7] As the primary school gross enrolment rate measures the share of children of any age that are enrolled in primary school, it could be bigger than 100%.[8] Furthermore, the enrollment of the second to

[3] Malawi was also one of the first countries in sub-Saharan Africa to start free primary education after the 1990 Jomtien World Conference on Education for All.

[4] In 1994, 29 kwachas were equal to 7.25 US dollars ($1=K4 in 1994).

[5] Private schools were also included in the fee abolition policy.

[6] In 1994, the Gini index is about 0.6 in Malawi (Cornia et al., 2017).

[7] Total enrollment increased from less than 2.0 million in 1993/1994 to nearly 3.0 million in 1994/1995 and to about 3.2 million in 2001. About 2.3 million are enrolled in Standards 1 to 4 (Nellemann, 2004).

[8] Primary school net enrolment rate is the share of children of official primary school age that are enrolled in primary school, which is smaller than or equal to 1.

last grade (Standard 7) and final grade (Standard 8) of primary schooling increased by 47 and 76 percent respectively during the same period (UNICEF, 2009).[9]

The UPE program was implemented after the election of a new government which is a result of the first democratic election since independence. The policy was implemented in September 1994, around four months after the election. The promise of the abolition of primary school fees as a means of increasing access to education was high on the agenda of most political parties during the 1994 general election. Once in power, the party that won the elections immediately fulfilled its pledge.[10] To guarantee that the benefits of this education policy spread to each Malawian citizen regardless of their location and socioeconomic status, the Ministry of Education launched a mass media campaign to ensure that the public was aware of the policy. To accommodate the influx of new student, the government recruited and trained 20,000 new teachers and significantly increased budgetary allocation to the education sector to build schools and pay teachers' salary. As a share of the total government budget, education spending rose from 13 percent in 1994/95 to 20 percent in 1997/98 (Kattan, 2006).

3.3.2. Malawi Education System

Malawi's education system operates on an 8-4-4 system and is made up of eight years primary schooling (referred to as Standard 1 to Standard 8), 4 years of secondary schooling (referred to as Form 1 to Form 4), and 4 years of university education. Although primary education became free in 1994, it was not compulsory in law. At the end of primary school, pupils sit for the Primary School Leaving Certificate Examination (PLSCE), which determines their eligibility

[9] The government adopted an open door policy that allowed children to enroll or reenroll in any grade irrespective of age. This policy resulted in an influx of children, many of whom were overage, into the primary school system. The increase in the enrollment could be driven by the entry of students who re-enrolled following an early school dropout.

[10] The UPE policy played an important role in the elections since increasing access to primary schooling through the abolition of fees would be highly visible and was guaranteed by the financial support of international agencies. Thus, the government has strong motivation to fully implement this education program.

for entry into secondary school. Participation in secondary school remains limited and gross enrollment ratio of secondary school in Malawi is only 16% of secondary-school-aged youth in 2007 (Grant, 2015). For most people in Malawi, primary education is the highest level of education they achieve.[11]

The official age of entry into primary school is 6 years old, and the school calendar runs from October to July. A student who progressed through school on time and without any interruptions would be expected to finish primary school at age 14. However, delayed school entry is very common in Malawi, which results in many students who are enrolled behind the appropriate grade for age (Grant, 2015). Malawi National Statistics Office in 2003 reported that 65% of primary school pupils were overage for their grade. Kadzamira and Chibwana (2000) found that, in rural areas, the mean age of Standard 1 pupils was 7.2 for girls and 7.5 for boys in 1997. The late entry implies that students are more likely to finish their primary education at age around 15 instead of 14, and that girls who were aged 15 or younger in 1994 should be exposed to the UPE reform, while girls who were 16 or older in 1994 were not likely to be exposed to the education policy.

3.4. Data

This paper uses data from two sources to examine the relationship between female education on fertility outcomes. The primary data source is the recent three waves (2004, 2010, 2015) of Demographic and Health Surveys (DHS) of Malawi. The DHS of Malawi is a nationally representative survey and provides cross-sectional information on a variety of topics about reproductive aged (15-49) women's lives, including their socioeconomic and demographic characteristics. The DHS also contains a detailed fertility history of respondents, which helps determine women's number of births at any given age.

[11] For tertiary education, Malawi has one of the lowest proportions of enrolled tertiary student per 100,000 inhabitants in the whole Sub-Saharan Africa. In 2001, Malawi's tertiary education system enrolls approximately 4000 students, which constitutes roughly 0.3 percent of students of eligible age.

The second data source is the 2008 Population and Housing Census (PHC) undertaken by the National Statistical Office of Malawi. The 2008 Population and Housing Census is the fifth in a series of decennial censuses that have been conducted in Malawi since the country attained its independence in 1964.[12] Similar to the DHS, the PHC contains individual pre-determined characteristics and information on schooling attainment and women's fertility. The key advantage of the census data is its large sample size. However, the PHC suffers from the lack of information on birthdays of the respondents' children, which makes it is impossible to determine the age at which mothers gave birth to their offspring. Therefore, the PHC cannot be utilized for the analysis of the impact of female education on the timing of birth, although it can be employed to analyze the impact of education on total fertility. As the DHS provides more detailed information of the respondent, the paper uses the DHS in the main analysis and reports the estimation results employing the data from PHC in the appendix as a robustness check.

To examine the effect of the education policy, I divide women into treatment group and control group according to woman's age in 1994. Women aged 15 or younger in 1994 would be affected by the policy. Women aged 16 or older in 1994 would not be affected because they were too old to attend primary school. However, some individuals aged 16 in 1994 may also be affected by the policy because grade repetitions are common in primary school system of Malawi. To reduce the measurement error in the treatment, I exclude individuals aged 16 in 1994 (pivotal cohort) in the baseline analysis. I include individuals born 6 years before and after the pivotal cohort in the estimation sample (The selection of the years of cohorts will be discussed below). The treatment group includes women aged 10-15, and the control group includes women aged 17-22 when the education reform became effective in 1994. Individuals

[12] The first post-independence census was conducted in 1966, followed by the 1977, 1987, and 1998 censuses.

in the study sample were born between 1972 and 1984.[13] In the section of robustness check, I test the sensitivity of the baseline estimates by including the individuals aged 16 in 1994 or by excluding individuals aged 15 and 16 in 1994 in the analysis.

The independent variable of interest is education, which is measured as years of completed schooling by combining information on educational level attended and the grade attained. The outcome variable is measured as the number of children ever born to each female respondent. Another alternative measure of fertility is the number of living children to the respondent. The above two fertility measures are both reported in the data from the DHS and PHC. The estimation results using the PHC are reported in the appendix.

Summary statistics of key variables of the DHS by exposure to the 1994 UPE policy are presented in Table 3.1. As shown in Table 3.1, women in the treatment group are 7 years younger than their counterparts in the control group. The average years of education of females in the analysis is about 5 years. Women exposed to the reform have more years of education, higher primary school completion rate, and a higher rate of completing some secondary school compared to women not exposed to the reform. The t-tests in column (4) shows that the two groups are similar regarding marital status, ethnicity and religion. Approximately 85% of women in the survey live in rural areas. The descriptive statistics of the data of the PHC are presented in Table B.1 in the appendix.

3.5. Empirical Strategy

The relationship between fertility and female schooling is analyzed using the following basic regression model:

$$Y_i = \beta_0 + \beta_1 Schooling_i + X_i'\theta + \gamma_r + \sigma_t + \epsilon_i \quad (1)$$

where Y_i stands for the fertility outcome of woman i, $Schooling_i$ refers to individual i's educational attainment which is measured by the years of completed education. The vector X_i

[13] Women in the study sample were 20-32 in 2004, 24-36 in 2008, 26-38 in 2010, and 31-43 in 2015.

Table 3.1. Summary Statistics (DHS)

	All	Treated group (Ages 10-15 in 1994)	Control group (Ages 17-22 in 1994)	Difference (2)-(3)
	(1)	(2)	(3)	(4)
Predetermined Variable				
Age	31.28	28.47	35.35	-6.87***
	(5.67)	(4.56)	(4.54)	(0.07)
Christian (=1 if religion is Christian, =0 otherwise)	0.61	0.61	0.61	0.00
	(0.49)	(0.49)	(0.49)	(0.00)
Currently Married (=1 if currently married, =0 otherwise)	0.77	0.78	0.75	0.03***
	(0.42)	(0.41)	(0.43)	(0.01)
Chewa (=1 if ethnicity is Chewa, =0 otherwise)	0.31	0.31	0.32	-0.01
	(0.46)	(0.46)	(0.46)	(0.01)
Rural (=1 if living in rural areas, =0 otherwise)	0.85	0.84	0.86	-0.03***
	(0.36)	(0.37)	(0.35)	(0.01)
Years of Education	4.95	5.53	4.10	1.44***
	(3.79)	(3.70)	(3.77)	(0.05)
Completion rate of primary school	0.26	0.30	0.19	0.11***
	(0.44)	(0.46)	(0.40)	(0.01)
Completion rate of some secondary school	0.17	0.21	0.12	0.09***
	(0.38)	(0.41)	(0.32)	(0.01)
Outcome variable				
Number of children ever born	3.97	3.31	4.93	-1.62***
	(1.98)	(1.64)	(2.03)	(0.03)
Number of total living children	3.45	2.94	4.19	-1.25***
	(1.76)	(1.50)	(1.83)	(0.02)
Ideal number of children	4.15	3.97	4.40	-0.43***
	(1.29)	(1.22)	(1.35)	(0.02)
Age at first marriage	17.78	17.67	17.94	-0.28***
	(3.4)	(3.17)	(3.70)	(0.05)
Age at first birth	18.59	18.44	18.80	-0.38***
	(2.98)	(2.80)	(3.21)	(0.04)
Number of months of marriage to birth	18.27	17.41	19.48	-2.141***
	(17.45)	(15.33)	(20.02)	(0.28)
Use modern contraceptive	0.42	0.44	0.43	0.01
	(0.49)	(0.50)	(0.50)	(0.01)
Husband-wife age difference	5.72	5.46	6.12	-0.65***
	(5.42)	(5.07)	(5.90)	(0.09)
Husband education	7.54	7.75	7.21	0.55***
	(3.48)	(3.48)	(3.45)	(0.06)
Currently working	0.66	0.64	0.69	-0.04***
	(0.47)	(0.48)	(0.46)	(0.01)
Occupation: agricultural	0.57	0.56	0.57	-0.01
	(0.50)	(0.50)	(0.49)	(0.01)
Occupation: professional/technical/managerial	0.05	0.05	0.05	0.00
	(0.21)	(0.21)	(0.22)	(0.00)

(Table cont'd.)

Table 3.1 (cont'd.)

	All	Treated group (Ages 10-15 in 1994)	Control group (Ages 17-22 in 1994)	Difference (2)-(3)
Outcome variable				
Occupation: sales	0.17	0.17	0.16	0.01
	(0.37)	(0.38)	(0.37)	(0.01)
Number of observations	19,324	11,445	7,879	

Notes: Standard errors reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01

includes individual characteristics including age in 1994, religion, ethnicity, and marital status. The error term ϵ_i represents unobserved individual attributes which are likely to be correlated with both the individual's years of schooling and fertility. The regression controls for district fixed effects γ_r, survey year fixed effects σ_t, and district specific time trends.[14] District fixed effects filter out unobserved characteristics that are shared by all individuals in a given district of residence. District specific time trends capture the trends over time in fertility across districts. Standard errors are clustered at the district-by-birth cohort level. The results are similar to the baseline findings when standard errors are clustered at the birth cohort level.[15] β_1 is the coefficient of interest, which captures the effect of education on individual's number of children.

Simple OLS estimation of Eq. (1) would result in biased estimates of the coefficient β_1 if schooling is not exogenously determined. To address the problem of endogeneity of education, the study exploits the exogenous variation in the educational attainment induced by the primary school fees elimination in Malawi in 1994 and applies an instrumental variable (IV) strategy to estimate the causal fertility effect of education. The effect of education on fertility is

[14] Malawi is composed of three regions (the Northern, Central, and Southern regions) which are divided into 28 districts.

[15] The results are available upon request.

estimated using a two-stage least squares (2SLS) estimation strategy. The first-stage equation estimates the following relationship:

$$Schooling_i = \delta_0 + \delta_1 Z_i + \delta_2 Z_i \times (16 - Age_{1994}) + \delta_3 (1 - Z_i) \times (16 - Age_{1994}) + X_i' \gamma + \theta_r + \sigma_t + \epsilon_i \quad (2)$$

where Z is the reform-related instrumental variable (=1 if the woman aged 15 or younger in 1994; =0 otherwise). The term 16-Age_{1994} is the running variable which represents respondents' distance to the pivotal cohort who were aged 16 in 1994. The two interaction terms control for the linear trends in education attainment at the birth cohort level. The predicted value of $Schooling_i$ from Eq. (2) are then used to estimate the second stage:

$$Y_i = \beta_0 + \beta_1 \widehat{Schooling_i} + \beta_2 Z_i \times (16 - Age_{1994}) + \beta_3 (1 - Z_i) \times (16 - Age_{1994}) + X_i' \gamma + \theta_r + \sigma_t + \epsilon_i \quad (3)$$

In Eq. (3), β_1 can be interpreted as the causal effect of education on fertility outcomes.

Although increasing the bandwidth would allow to estimate the model with a larger sample size, variation obtained from individuals who are far from the pivotal cohort may produce biased results. On the other hand, shortening the bandwidth would lead to imprecise estimates of the treatment effect because of a smaller sample size. In the main analysis, I use a bandwidth of 6 years near the cutoff, which is obtained from the first-stage regression by using the optimal bandwidth selection method developed by Imbens and Kalyanaraman (2012).[16] The estimates barely change with different bandwidths, which is shown in the section on robustness analysis.

The estimation approach is consistent with a fuzzy regression discontinuity design, which provides estimates that are as credible as those from randomized experiment under relatively weak assumptions (Lee and Card, 2008). Women who were born close the pivotal cohort would be expected to be similar in all observed and unobserved characteristics other than their exposure to the UPE policy. Therefore, any discontinuities in the outcome of interest at the

[16] The CCT (Calonico et al., 2014) optimal bandwidth is 3.

threshold can be attributed to the causal effects of the changes in schooling. Although the exclusion restriction is not testable directly, an implication is that women near the threshold should be similar along their predetermined characteristics. To validate this assumption, I examine whether the UPE policy is significantly associated with the following women's characteristics including whether the respondent is Christian, whether the ethnicity of the respondent is Chewa, and whether the respondent is currently married. Table B.2 shows that most coefficients of the UPE policy are not statistically significant, which suggests that the control variables are smoothly distributed across the discontinuity.[17]

One potential factor possibly confounding the estimates of the effect of education on fertility is that the quality of teaching may be deteriorated by the reform in the short run because of the shortage of qualified teachers and crowded classrooms. After school fees were abolished in 1994, a large number of unqualified primary school teachers were recruited in order to keep up with the increase in enrollments (Al-Samarrai and Zaman, 2007). From the 1993/1994 to 1994/1995 academic years, the average student-teacher ratio declined from 68 to 62, but the ratio of students to qualified teachers increased from 82 to 108 over the same period. Unqualified teachers are expected to have a negative impact on the quality of teaching, which would potentially lead to a downward bias in the effect of female education.

3.6. Results

3.6.1. The Effect of The UPE Policy on Female Schooling

I start the empirical analysis by examining whether the education reform has any influence on women's educational attainment. Figure 3.1 provides a graphical presentation of the average number of years of education for birth cohorts before and after the pivotal cohort using data of the DHS. As shown in Figure 3.1, there is a marked discontinuity in the average years of

[17] There are two coefficients are statistically significant at the 10% level, while the magnitudes of those coefficients are relatively small.

schooling between the control and treatment cohorts at the threshold, which suggests that the UPE policy was effective in boosting women's educational attainment. Figure B.1 shows a similar pattern using data of the PHC.

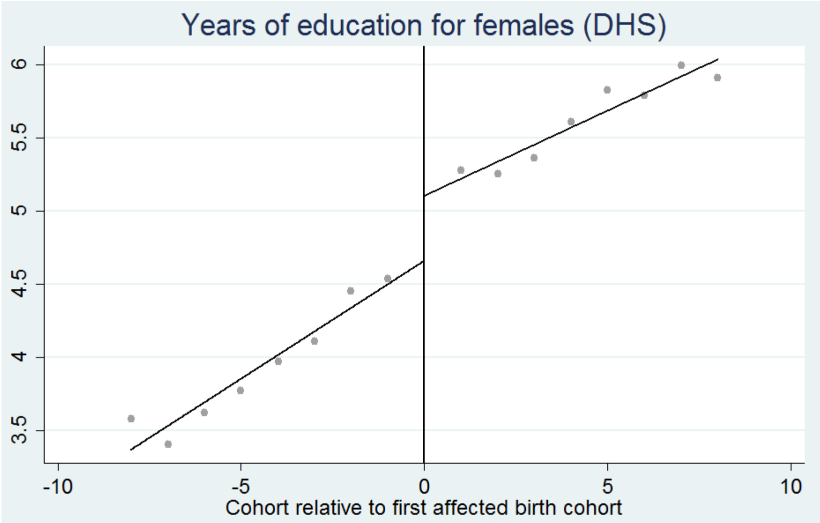

Figure 3.1. Female years of education-DHS

Table 3.2 displays the estimates of the impact of the UPE policy on educational achievement based on data of the DHS for the pooled sample, and for the urban and rural females, respectively. All specifications control for district fixed effects, survey year dummies, and linear district-specific time trends, while only columns (4)-(6) control for women's covariates. For the purpose of comparison, columns (1)-(3) report the estimates without individual controls. Column (4) of Table 3.2 shows that the UPE policy has a significant positive impact on women's years of schooling for the full sample. The policy leads women's educational attainment to increase by 0.33 years. However, columns (5)- (6) indicate that the effects of the education policy are heterogenous across rural and urban women. Column (5) shows that the UPE policy increases educational attainment by 0.42 years (10 percent increase) for women living in rural areas, after controlling for covariates. The first-stage F-statistics is above the rule-of-thumb threshold of ten. Columns (6) of Table 3.2 presents that women living in urban area are not significantly affected by the UPE policy, probably because school fees are not a constraining factor for women living urban areas who have better financial resources. The

impact of UPE policy on women's schooling attainment is confirmed by a similar analysis using data of the PHC with a larger sample size (see Table B.3). In the following study, the paper restricts the study sample to females living in rural areas.

Table 3.2. First Stage Results: The Impact of the UPE Policy on Female Schooling Attainment

	All	Rural	Urban	All	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Reform 1994	0.277**	0.408***	-0.270	0.333***	0.422***	-0.023
	(0.128)	(0.133)	(0.339)	(0.118)	(0.123)	(0.325)
Controls	No	No	No	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
District Trends	Yes	Yes	Yes	Yes	Yes	Yes
First Stage F-stat	4.67	9.34	1.89	8.00	11.73	0.01
Observations	19,324	16,367	2,957	19,324	16,367	2,957
Mean of dep. var	4.94	4.41	7.80	4.94	4.41	7.80

Notes: Standard errors are clustered at the district-by-birth cohort level and reported in parentheses. Individual controls include religion, ethnicity, and marital status. Each specification controls for district fixed effects, district specific time trends, and year of survey fixed effects. * p<0.1, ** p<0.05, *** p<0.01

Figure B.2 shows that the introduction of UPE policy not only increased rural women's propensity of completing primary education but also improved the propensity of completing some secondary education, which suggests that the primary school fees elimination policy affects women's educational achievement beyond the primary schooling level even though school fees are only eliminated for primary schooling. Columns (1) and (2) of Table B.4 show that the UPE policy increased rural women's probability of completing primary schooling by 4 percentage points (27 percent increase), and improved their probability of having attended secondary education by 3.8 percentage points (48 percent increase) using the DHS, which is consistent with the results shown in columns (3) and (4) using the PHC. By and large, estimates using both datasets suggest that the reform has a favorable effect on women's education in rural areas.

I use the similar specification to test whether the UPE policy affects male educational attainment. The results in Table B.5 show that the association between the exposure to the UPE policy and men's years of schooling is not statistically significant. In Malawi, parental preference for educating their sons over daughters was quite pronounced if a family can only afford education for some of their children, which indicates that girls are more likely to drop out of school in comparison to boys due to financial reasons. The policy of eliminating primary school fees significantly reduced parents' financial burden of educating their children, thus probably made girls benefit more from the policy.

3.6.2. The Effect of Education on Women's Fertility

This section turns to the discussion of the causal effect of female education on fertility outcomes. As shown in Figures B.2 and B.3, there is a discontinuous decrease in the number of children ever born and the number of living children at the threshold, which coincides with the jump in women's educational attainment. This pattern of fertility at the threshold is corroborated by Figures B.3 and B.4 using data from the PHC.

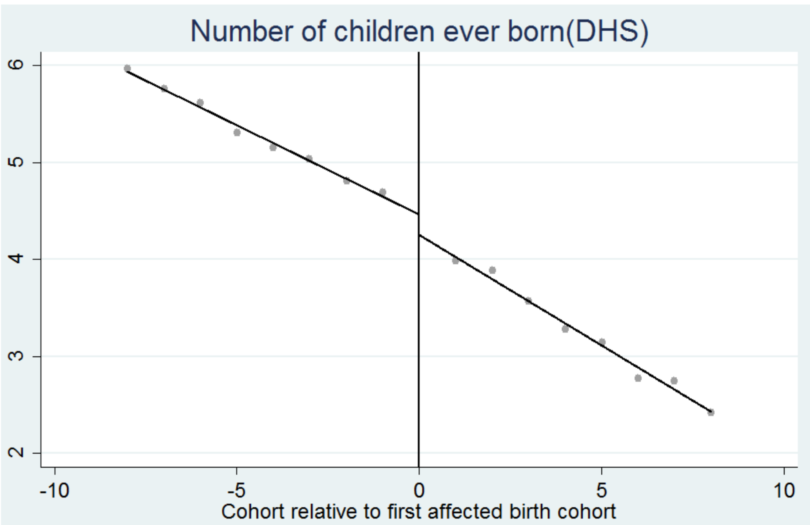

Figure 3.2. Number of Children Ever Born- DHS

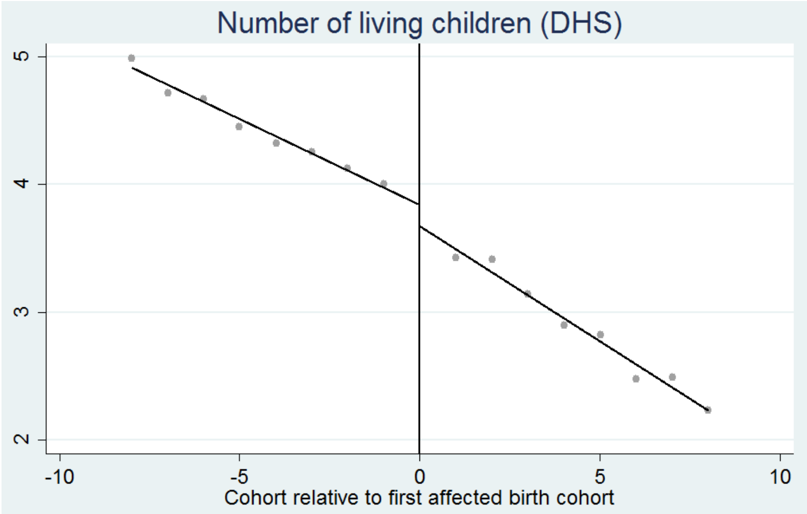

Figure 3.3. Number of Living Children- DHS

Table 3.3 reports the estimated coefficients of education on fertility outcomes using the DHS. For the purpose of comparison, I present the OLS estimates in columns (1) and (4). The OLS estimates show a negative and statistically significant associated relationship between female education and fertility. Specifically, one additional year of female education is associated with 0.124 fewer children ever born and 0.087 fewer living children, which corresponds to a reduction of 2.1% and 2.5% at the mean, respectively.

Table 3.3. The Impact of Female Education on Fertility

	Number of Children Ever Born			Number of Living Children		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	Reduced Form	2SLS	OLS	Reduced Form	2SLS
Education	-0.124***		-0.392***	-0.087***		-0.335***
	(0.003)		(0.141)	(0.003)		(0.129)
Reform 1994		-0.166***			-0.141***	
		(0.049)			(0.046)	
District FE	Yes	Yes	Yes	Yes	Yes	Yes
District Trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,367	16,367	16,367	16,367	16,367	16,367
First Stage F-stat			11.73			11.73
Mean of dep. var	3.822	3.822	3.822	3.324	3.324	3.324

Notes: Standard errors are clustered at the district-by-birth cohort level and reported in parentheses. Individual controls include religion, ethnicity, and marital status. Each specification controls for district fixed effects, district specific time trends, and year of survey fixed effects. * p<0.1, ** p<0.05, *** p<0.01

The 2SLS estimates are presented in columns (3) and (6). One additional year of schooling leads to 0.39 fewer children ever born and 0.34 fewer living children. Both of these estimates are statistically different from zero at one percent level. These results show that the magnitude of OLS estimates are smaller than that of 2SLS estimates, which indicates that there are likely some unobserved factors positively correlated with both education and fertility, leading to bias toward 0 in OLS estimates.[18] An alternative explanation is that 2SLS estimates capture the local average treatment effects (LATE) of female education, which is the effect of education on fertility for women who changed their educational attainment because they have been affected by the reform (compliers). The marginal effect of education for compliers is likely to be larger since the primary effect of the UPE policy is at the bottom of the schooling distribution.

Columns (2) and (5) of Table 3.3 report the reduced-form estimates of the effects of the UPE policy on fertility. The estimates indicate that individuals who were affected by the policy have lower fertility, which corroborates the evidence that increased female schooling resulting from the UPE policy caused a decline in fertility. Specifically, the UPE policy decreases women's number of children ever born by 0.17 and the number of living children by 0.14.

I also conduct a similar analysis using data of the PHC. As shown in Table B.6, one additional year of education decreases women's number of children ever born and number of living children by 0.50 and 0.34, respectively, which is consistent with the baseline results employing data of the DHS.

3.6.3. Robustness Checks

To evaluate the robustness of the baseline results, I conducted a number of sensitivity analysis. First, I investigate whether the result is robust to the inclusion of the pivotal cohort in the

[18] The presence of error in available measures of schooling can also introduce a bias toward zero, thus creating the appearance of a weaker correlation between the two variables than may exist in reality.

regression by assigning the pivotal cohort as the control group. The other alternative specification to reduce the measurement error in the treatment is to exclude both cohorts aged 15 and 16 in 1994. Columns (2) and (3) of Table 3.4 show that the estimated coefficients of these two specifications are similar to the baseline estimates.

Second, I further examine whether the results are sensitive to the change in the order of polynomial function of birth cohorts. In the baseline analysis, I control for piece-wise linear polynomial function of birth cohorts. However, treatment and control group might differ in their unobserved characteristics, and these differences might not be captured by the linear cohort trends. Allowing for quadratic polynomial function of the birth cohort is one way to increase the flexibility of this control variable. Column (4) of Table 3.4 shows that the results obtained from this exercise are in line with those obtained from the baseline specification.

Third, as mentioned in the section of empirical method, the choice of bandwidth of the pre- and post-reform cohorts introduces a trade-off between efficiency and bias. To test whether the estimates are sensitive to the choice of bandwidth, I try different bandwidths varying from 8 to 3 before and after the pivotal cohort. Columns (5) to (9) of Table 3.4 show that the estimates are statistically significant and that the magnitudes of the coefficients are in line with the baseline findings. It is worth noting that the first-stage F-statistics are smaller than 10 when the bandwidth is 3 or 4. This is not surprising because the smaller sample size as a result of narrower bandwidths would lead the coefficient to be less precisely estimated although a narrower bandwidth can minimize the bias associated with secular time trends. In general, the estimates of the effect of education on fertility outcomes are stable for different bandwidths. Additionally, the estimates are consistent with the baseline findings when I perform the above robustness checks employing data from the PHC (See Table B.7).

Table 3.4. The Impact of Female Education on Fertility Using Various Specifications

	Baseline	Include cohorts aged 16	Exclude cohorts aged 15 and 16	Quadratic function	Bandwidth= 8	Bandwidth= 7	Bandwidth= 5	Bandwidth= 4	Bandwidth= 3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Number of children ever born									
Education	-0.392***	-0.530**	-0.502***	-0.499***	-0.264***	-0.352***	-0.482***	-0.444**	-0.433**
	(0.141)	(0.250)	(0.179)	(0.133)	(0.082)	(0.129)	(0.164)	(0.197)	(0.207)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,367	18,233	16,625	16,367	21,248	18,748	13,607	11,025	8,076
First Stage F-stat	11.73	4.35	9.02	12.14	23.98	13.03	10.86	6.51	6.15
Panel B: Number of living children									
Education	-0.335***	-0.533**	-0.324**	-0.537***	-0.287***	-0.315***	-0.436***	-0.443**	-0.454**
	(0.129)	(0.268)	(0.157)	(0.117)	(0.105)	(0.121)	(0.155)	(0.195)	(0.212)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,367	18,233	16,625	16,367	21,248	18,748	13,607	11,025	8,076
First Stage F-stat	11.73	4.35	9.018	12.14	23.98	13.03	10.86	6.51	6.15

Notes: Standard errors are clustered at the district-by-birth cohort level and reported in parentheses. Individual controls include religion, ethnicity, and marital status. Each specification controls for district fixed effects, district specific time trends, and year of survey fixed effects. * p<0.1, ** p<0.05, *** p<0.01

Fourth, to ensure that the baseline estimates are not driven by a particular wave of the data, I estimate the effect of education for each wave of the DHS separately. As shown in Table 3.5, although some coefficients are not estimated precisely likely because of the smaller sample sizes, the sign and magnitude of the estimates are consistent with the baseline estimates shown in Table 3.3. Fifth, I examine whether the fertility effect of education is robust to alternative measures of fertility. Specifically, I replace the outcome variable with women's number of births by age 24 to 27. To do that, I only use the recent two waves of the DHS (2010 and 2015). The study sample includes women aged between 27 and 37 in 2010, and women aged between 32 to 42 in 2015. Table 3.6 shows that female education has a negative impact on women's overall fertility at age between 24 and 27, although the estimate of the women's number of births at age 25 is not statistically significant. One concern is that early fertility may be less indicative of women's completed fertility over the life cycle. To alleviate this concern, I examine whether female education affects women's total fertility at age 34 by restricting the study sample to women aged between 34 and 40 in the DHS of 2015. In this case, I can only include 3 cohorts before and after the pivotal cohort in the study sample. Column (10) shows that one additional year of female education decreases women's number of births by 0.28,

Table 3.5. The Impact of Female Education on Fertility Using Three Waves Separately

	Number of Children ever born			Number of living children		
	2004	2010	2015	2004	2010	2015
	(1)	(2)	(3)	(4)	(5)	(6)
Education	-0.463	-0.387***	-0.413	-0.283	-0.221*	-0.551*
	(0.533)	(0.145)	(0.295)	(0.376)	(0.121)	(0.333)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
District Trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4139	6919	5309	4139	6919	5309

Notes: Standard errors are clustered at the district-by-birth cohort level and reported in parentheses. Individual controls include religion, ethnicity, and marital status. Each specification controls for district fixed effects, district specific time trends, and year of survey fixed effects. * p<0.1, ** p<0.05, *** p<0.01

Table 3.6. The Impact of Female Education on Alternative Fertility Measures

	Number of births by 24		Number of births by 25		Number of births by 26		Number of births by 27		Number of births by 34	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	RF	2SLS	RF	2SLS	RF	2SLS	RF	2SLS	RF	2SLS
Reform 1994	-0.126***		-0.085		-0.119**		-0.134**		-0.174	
	(0.048)		(0.054)		(0.052)		(0.054)		(0.143)	
Education		-0.245**		-0.166		-0.230**		-0.259**		-0.275
		(0.108)		(0.105)		(0.111)		(0.114)		(0.266)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,161	10,161	10,161	10,161	10,161	10,161	10,161	10,161	2,717	2,717
First Stage F-stat		10.91		10.91		10.91		10.91		3.73

Notes: Standard errors are clustered at the district-by-birth cohort level and reported in parentheses. Individual controls include religion, ethnicity, and marital status. Each specification controls for district fixed effects, district specific time trends, and year of survey fixed effects.
* p<0.1, ** p<0.05, *** p<0.01

which is similar in magnitude to the total fertility at age between 24 and 27 although the coefficient is not estimated precisely due to a large drop in sample size.

Lastly, I utilize an alternative estimation strategy to examine the impact of the UPE policy on fertility. As shown in the previous section, the UPE policy does not affect women living in the urban area, therefore I exploit the variation between rural and urban women as well as the variation between pre- and post-cohorts. I estimate the following difference-in-differences specification:

$$Y_i = \gamma_0 + \gamma_1 Rural_i + \gamma_2 Cohort_i + \gamma_3 Rural_i \times Cohort_i + X_i' \theta + \epsilon_i \quad (4)$$

where $Rural_i$ is dummy variable indicating whether women i currently reside in a rural area. $Cohort_i$ equals 1 if for women aged 16 or less in 1994 and equals 0 for women aged 17 or more in 1994. The parameter γ_3 is an estimate of the reduced form effect of the exposure to the reform. Table B.8 presents that the UPE policy decreases women's number of children ever born by 0.3 and number of living children by 0.15. The dif-in-dif estimate confirms the reduced form estimates provided by the baseline regression.

3.6.4. Placebo Tests

The validity of the identification strategy relies on the assumption that the education policy only affects fertility through its effect on individual's educational attainment. One potential concern is that the reform exposure variable may pick up some structural changes or unspecified time trends instead of the true treatment effect of UPE. To address this concern, I create placebo reforms by moving the year of the reform two, three, and four years back and forward in comparison to the actual year. The placebo reforms should have non-significant impacts on fertility. As reported in Table 3.7, the estimate of each placebo reform is generally small in magnitude and statistically insignificant, which provides supportive evidence that

Table 3.7. Placebo Test

	2 years back	3 years back	4 years back	2 years forward	3 years forward	4 years forward
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Number of children ever born						
Placebo reform	0.074	0.088	0.044	0.051	0.053	-0.037
	(0.056)	(0.075)	(0.076)	(0.038)	(0.040)	(0.036)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
District Trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,126	15,055	14,544	17,617	17,590	17,660
Panel B: Number of children living children						
Placebo reform	0.093*	0.121	0.022	0.048	0.051	-0.025
	(0.053)	(0.074)	(0.070)	(0.035)	(0.037)	(0.033)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
District Trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,126	15,055	14,544	17,617	17,590	17,660

Notes: Standard errors are clustered at the district-by-birth cohort level and reported in parentheses. Individual controls include religion, ethnicity, and marital status. Each specification controls for district fixed effects, district specific time trends, and year of survey fixed effects. * p<0.1, ** p<0.05, *** p<0.01

the main results are due to the implementation of UPE policy as opposed to other unobserved societal changes. The pattern of the estimates barely changes using the data from the PHC (See Table B.9).

3.6.5. Potential Mechanisms

The analysis has provided consistent evidence that female education has a negative effect on fertility. In this section I analyze the potential channels underlying the relationship between female education and fertility. As discussed in the literature review, education may have a negative impact on fertility in multiple ways including labor force participation (Becker, 1981; Willis, 1973), positive assortative marriage matching, e.g., spouse's education (Behrman and Rosenzweig, 2002), quality-quantity tradeoff (Becker and Lewis, 1973), knowledge and use of modern contraceptive methods (Grossman, 1972; Rosenzweig and Schultz, 1989), and postponement of the timing of the first birth and marriage (Black et al., 2008). While it is not possible to test all of these possible mechanisms, the rich set of information provided by the

DHS allows me to test some of those pathways underlying the fertility effect of female education, such as fertility preferences, age at first marriage and first birth, modern contraceptive use, husband's characteristics, and females' labor force participation and occupation. To examine these potential mechanisms, I estimate 2SLS equations similar to Eq. (3), in which the outcome variable is replaced by these intermediate variables.

Increased female education may shift women's fertility preference toward improving quality of their offspring, which makes women prefer a smaller family size. Column (2) of Table 3.8 shows that higher female education significantly decreases women's desired number of children. Specifically, each year of female education reduces the ideal number of children by 0.29. Female education could also decrease fertility by postponing the age at first marriage and first birth. Columns (4) and (6) of Table 3.8 show that each year of education postpones women's age at first marriage and age at first birth by 0.40 years and 0.41 years, respectively. Column (8) indicates that increased female education also postpones the first birth in marriage. One additional year of female education increases the interval between marriage and the first birth by 3.35 months. The delay of the timing of marriage and the first birth can be partly explained by the "incarceration effect" of education because keeping girls in school prevents them from getting married and giving birth (Black et al., 2008). Previous studies also suggest that female schooling reduces fertility by improving their knowledge about the modern contraceptive use (Rosenzweig and Schultz, 1989). However, as shown in column (10), female education has no impact on women's probability of using modern contraceptive methods.

I next proceed to examine whether women's education affects characteristics of their husband. Due to positive assortative mating it would be expected that women with higher

Table 3.8. The Impact of Female Education on Reproductive Behaviors

	Ideal number of Children		Age at first marriage		Age at First birth		Months of marriage to first birth		Using modern contraceptives	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Education	-0.074***	-0.290***	0.270***	0.398**	0.212***	0.413**	-0.378***	3.346*	0.006***	-0.024
	(0.003)	(0.100)	(0.008)	(0.191)	(0.008)	(0.205)	(0.047)	(1.940)	(0.001)	(0.044)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,367	16,367	16,367	16,367	16,034	16,034	13,992	13,992	16,367	16,367
First Stage F-stat		11.73		11.73		12.60		10.67		11.73
Mean of Dep. Var	4.166	4.166	17.604	17.604	18.606	18.606	18.076	18.076	0.425	0.425

Notes: Standard errors are clustered at the district-by-birth cohort level and reported in parentheses. Individual controls include religion, ethnicity, and marital status. Each specification controls for district fixed effects, district specific time trends, and year of survey fixed effects. * p<0.1, ** p<0.05, *** p<0.01

educational attainment may have married better-educated men, who may prefer quality of children to quantity. Column (2) of Table 3.9 shows that a one-year increase in female education is positively associated with 0.29 years of husband education, but the estimate is not statistically significant. Column (4) shows that female education does not affect the age gap between husband and wife significantly. Overall, there is no strong evidence that women with more education married younger or more educated men.

The classic economic model of fertility implies that education increases the opportunity cost of women's time by increasing women's labor market participation and wage, prompting them to have fewer children (Becker, 1981). As shown in columns (5), (7), (9), and (11) of Table 3.9, the OLS estimates suggest that higher female education increases women's labor participation and propensity of having a professional, technical, managerial, and sales job, and decreases women's propensity of being self-employed in agriculture sector. However, the 2SLS estimates in columns (6), (8), (10), and (12) show that the significant effects of female education are vanished when accounting for the endogeneity of education.

One potential explanation for why increased education plays a restricted role in influencing women's labor market participation is that there are few employment options outside of agriculture in Malawi and the majority of rural women who are working are just self-employed in agriculture sector. The labor force participation channel may be limited if the low modern wage employment hinders the absorption of educated young women into the labor market. Summarizing the above evidence, female education has no effect on women's labor force participation and occupation, which suggests that the decrease in fertility is not likely caused by an increase in women's opportunity cost of childbearing and childrearing. This result is consistent with the finding of Lavy and Zablotsky (2015) that women's labor force participation does not play a role in explaining the relationship between female education and fertility for Israeli-Arab women, who have low labor force participation rates.

Table 3.9. The Impact of Female Education on Assortative Mating and Labor Market Participation

	Husband education		Age difference between husband and wife		Working		Occupation: Agricultural		Occupation: professional/ technical/managerial		Occupation: sales	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Education	0.422***	0.288	-0.141***	-0.012	0.005***	-0.002	-0.022***	-0.059	0.010***	-0.007	0.008***	0.028
	(0.008)	(0.191)	(0.014)	(0.321)	(0.001)	(0.025)	(0.001)	(0.053)	(0.001)	(0.015)	(0.001)	(0.036)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,106	13,106	13,635	13,635	16,360	16,360	12,344	12,344	12,344	12,344	12,344	12,344
First Stage F-stat		13.38		12.33		11.47		6.81		6.81		6.81
Mean of Dep. Var	5.68	5.68	7.18	7.18	0.65	0.65	0.64	0.64	0.03	0.03	0.14	0.14

Notes: Standard errors are clustered at the district-by-birth cohort level and reported in parentheses. Individual controls include religion, ethnicity, and marital status. Each specification controls for district fixed effects, district specific time trends, and year of survey fixed effects. * p<0.1, ** p<0.05, *** p<0.01

3.7. Conclusion

This article uses an exogenous variation in education caused by the Universal Primary Education (UPE) policy in Malawi to examine the extent to which female education impacts fertility. The estimation strategy identifies the local average treatment effect of female education on fertility, which is the effect of schooling for those women who changed their education decisions due to the UPE policy.

The results show that the UPE policy positively affects rural women's educational attainment and that increased female education has a negative impact on fertility. Specifically, the education policy increases rural women's schooling attainment by 0.42 years. An additional year of female schooling decreases women's number of children ever born and number of living children by 0.39 and 0.34, respectively. A variety of specification and placebo tests reveal the robustness of the findings. The results are consistent with the causal findings of previous studies using data from sub-Saharan countries (Grepin and Bharadwaj, 2015; Keats, 2018; Osili and Long, 2008). Compared to previous studies, the current paper additionally finds that higher female education appears to lower women's completed number of births at their mid-30s. The results of the paper suggest that promoting female education might be an effective policy of decreasing fertility in Malawi.

Additionally, using the rich set of information in the DHS, I investigate possible mechanisms by which education might affect fertility. The results indicate that increased female education decreases women's desired number of children, postpones their marriage age and maternal age. However, there is no evidence that female education influences women's labor market participation and occupation, which suggests that education plays a limited role in reducing

fertility through the channel of increasing opportunity cost of women's time in an agriculture-based economy. I also do not find evidence that female education improves the quality women's husband or use of modern contraceptives.

CHAPTER 4. THE EFFECT OF FEMALE EDUCATION ON CHILD MORTALITY IN INDONESIA

4.1. Introduction

It is widely acknowledged that education, as a critical component of human capital, has favorable effects on individuals' social and economic outcomes including income (Bonjour et al., 2003; Oreopoulos, 2006), health (Clark and Royer, 2013; Brunello, Fabbri, and Fort, 2013; Lleras-Muney, 2005), cognitive ability (Banks and Mazzonna, 2012), and political and civic involvement (Larreguy and Marshall, 2017). While education has such a direct influence on individual's own life outcomes, previous literature also suggests that education plays a critical role in determining offspring's health (Breierova and Duflo, 2004; Chou et al. 2010; Currie and Moretti, 2003; Grossman, 2006) and cognitive outcomes (Andrabi, Das, and Khwaja, 2012; Dickson, Gregg, and Robinson, 2016). However, the evidence on the impacts of parental education on child health, especially on child mortality is still scarce in lower-middle-income countries. There are several studies that have found a causal relationship between parental education and child health, but only a few of these studies have been able to identify the potential mechanisms underlying it (Currie and Morretti 2003; Grépin and Bharadwaj, 2015; Keats, 2018).

The purpose of this study is to investigate whether female education has a causal impact on child mortality using a natural experiment that comes from an exogenous one-time extension of school year that took place in Indonesia in 1978. Academic years in Indonesia used to start in January and to end in December the same year. In mid-1978, the Indonesian government decided to change the start of the school year from January to July, and schools are required to

extend the 1978 school year until June of 1979, so that the 1978 academic year was extended by an extra six months. I take advantage of the exogenous variation in the length of school year to study the effect of women's years of education on a range of measures of child mortality.

The current paper follows recent empirical studies that focus on the effects of maternal education on child health employing the fuzzy regression discontinuity design (Grépin and Bharadwaj, 2015; Keats, 2018), and contributes to the existing literature in two main aspects. First, this study investigates the causal relationship between female education and child mortality in the setting of a middle-income developing country in Southeast Asia by using a natural experiment. The current paper differs from related studies by Grépin and Bharadwaj (2015) and Keats (2018) who focus on infant mortality and under-five mortality, while the current study additionally investigates neonatal mortality which accounts for 50% of under-five mortality during the study's sample period. Second, this paper explores a variety of mechanisms that lie behind the causal effect of maternal education on child mortality, including women's fertility behaviors, labor force participation, occupation, spouse's characteristics, women's empowerment in the household, and prenatal health care utilization.

Health economics literature suggests that there are a number of potential channels via which female schooling leads to an improvement in child health. First, given that education and income are positively related, women with higher education can afford more or better-quality health care services (Currie and Moretti, 2003). This channel could be enhanced by positive assortative mating since more educated women are more likely to marry higher-earning men with more education, which will further raise family income (Behrman and Rosenzweig, 2002). Second, more educated women may have a higher ability to acquire and process information,

which could lead women to use modern techniques of child care and disease prevention, and to help parents improve productive efficiency in child health production through making informed decisions on their children's nutrition and healthcare (Grossman, 2006; Lindeboom et al., 2009). Third, higher maternal education may affect the child health by influencing women's autonomy and bargaining power in the household, which is likely to channel family resources toward wellbeing of mother and child (Mocan and Cannonier, 2018). Finally, higher female education might result in a range of health-promoting behaviors such as reducing the consumption of cigarettes and alcohol (Currie and Moretti, 2003) and lead women to prefer fewer but healthier children thus invest more in each child (Becker and Lewis, 1973), which would cause lower child mortality.

The major challenge to investigate the causal relationship between maternal education and child health is to deal with the endogenous problem caused by some unobserved factors that are both correlated to maternal education and child health outcomes. Some studies utilize the plausible exogenous change in education caused by the expansion of schooling or age-at-entry policies to circumvent the endogenous problem (Breierova and Duflo, 2004; Currie and Moretti, 2003; Grépin and Bharadwaj, 2015; Lindeboom et al., 2009; Lundborg, Nilsson and Rooth, 2014; Keats 2018; McCrary and Royer, 2011). The results of those studies using data from industrialized countries are relatively mixed. For example, Currie and Moretti (2003) examine the impacts of maternal education on birth weight of infants using college opening in the U.S as an instrument. They find that maternal education improves infant birth weight. Lundborg et al. (2014) make use of the Swedish compulsory school reform as an instrument and find that maternal education improves offspring's health and cognitive ability, while paternal education

does not appear to have such effects. Lindeboom et al. (2009) exploit the minimum school leaving age extension in the UK as an instrument for parental education. Their analysis reveals that parental education has little effects on the birth weight of their offspring even though maternal education reduces financial difficulties. McCrary and Royer (2011) use the variation in years of education based on the date of birth in the U.S to examine the impacts of maternal education on fertility and infant health measured by birth weight, prematurity, and mortality. They also find little effects of maternal education.

In the setting of developing countries, empirical evidence generally shows that female education has favorable effects on child health. Breierova and Duflo (2004) estimate the impacts of parental education on fertility and child mortality in Indonesia by exploiting the primary school construction during 1973-1979 as an instrument for parental schooling. They find that increased female education decreases a variety of child mortality measures. Chou et al. (2010) employ an exogenous variation of education caused by a compulsory schooling expansion from six to nine years in Taiwan to explore the impact of parental education on infant health. They find that increased parental education leads to a lower child death rate in the neonatal and post-neonatal periods. Using similar methodologies, two studies focus on the effects of female schooling for Sub-Saharan African countries. Grépin and Bharadwaj (2015) estimate the effect of women’s education on child mortality employing the Zimbabwean secondary school enrollment expansion in 1980 and find that maternal education significantly decreases infant mortality and under-five mortality. Keats (2018) exploits the elimination of primary school fees in Uganda as a natural experiment and finds that increased maternal

education decreases the likelihood of children to be stunted, but he finds no effects on child mortality rates.

Using data from the Indonesia Demographic and Health Survey (DHS), the present paper shows that the extension of the school year in 1978 in Indonesia leads to a remarkable increase in women's educational attainment. In particular, the policy leads women to receive additional 0.82 years of education and increases women's completion rates of primary school and some secondary school by 6.6 and 8.2 percentage points, separately. Exploiting this education policy as an instrument for years of completed education, I find evidence that female education has a significant impact on reducing the risk of child death during the neonatal period; an extra year of education leads to a reduction in neonatal mortality by 0.8 percentage points, which corresponds to a 26% reduction. However, I find little evidence that female education affects infant and under-five mortality.

The study explores a number of potential channels through which female education influences the child mortality outcomes by exploiting the rich set of information in the DHS. The mechanism analysis suggests that increased female education raises women's age at first marriage and age at first birth, and decreases the likelihood of getting married and giving birth at teenage age, while higher female education does not significantly influence women's total number of children and fertility preference. Additionally, highly educated women are more likely to have a younger and more educated spouse and have more financial resources. The study also provides evidence that highly educated women are less likely to smoke and more likely to receive prenatal care from a skilled provider, to be assisted by skilled birth attendants at childbirth, and to deliver in a health facility. However, there is little evidence that female

education increases women's empowerment in terms of household decision making and attitudes against domestic violence.

The remainder of the paper is organized as follows: Section 2 provides the background of the school year extension policy. Section 3 describes the data. Section 4 introduces the identification strategy. Section 5 reports the estimation results. Section 6 concludes the paper.

4.2. School Year Extension in 1978-1979

In this section, I present a brief summary of the school year extension policy in Indonesia. Before 1978, academic years in Indonesia ran from January to December the same year. In mid-1978, the government required schools to extend the 1978 academic year until June 1979 to coincide the academic year with the fiscal year (Parinduri, 2014). Academic years have run from July to June next year since the policy was implemented in 1978. Therefore, students who were in primary school in 1978 stayed in the same grade until June 1979, while children who entered primary school after 1978 did not experience the extended school year.[1]

Indonesian children normally start to attend primary school at age 7 (Breierova and Duflo, 2004). Since longer school year was implemented in 1978, individuals who were born in 1971 or earlier should be influenced by the extension of school year as a result of the policy, while individuals who were born in 1972 or later would not be affected by the longer school year because they were too young to attend school when the government extended the length of academic year in 1978. The policy was implemented immediately and not expected by the public so that it is not likely that parents would precisely manipulate their children's entry to

[1] In the extra six months of the school year, teachers were just asked to review what they have learned in that academic year (Parinduri, 2014).

school around the 1978 academic year. Students who were born around 1971 should be comparable, and the only difference is the treatment status of the policy.

One concern about the validity of the school year extension policy is that the effect of the policy could be confounded by other education policies during the same period. If this is the case, the exclusion restriction of the instrument would be violated. To the best of my knowledge, there are no other government programs that only affect the older cohort at the cutoff. It is worth noting that the Indonesia government launched a remarkable school construction program (the Sekolah Dasar INPRES program) between 1973 and 1979. During that time, the Indonesian government constructed over 60,000 primary schools throughout the country (Breierova and Duflo, 2004). However, the primary school construction program does not confound the estimation strategy because it does not systematically influence students who entered primary schools around 1978-79 academic years differently.

4.3. Data

The data used in this study are from three waves (2002, 2007, 2012) of Indonesia Demographic and Health Survey (DHS). The DHS is a nationally representative survey of reproductive-aged women (aged 15-49) containing rich information on socioeconomic and demographic characteristics, which is ideal for this study because it includes individual’s information on the month and year of birth. The core questionnaire covers information on an individual’s full reproductive histories, educational attainment, marital and employment status, fertility behaviors, knowledge and use of modern contraceptive methods, and women’s autonomy and attitudes toward domestic violence.

Child mortality information is collected from the birth records of each interviewed woman which contains survivorship status of their children, and age at death for children who died. Based on this information, I create three binary indicators to measure child mortality outcomes: neonatal mortality (=1 if the infant died before reaching 28 days, =0 otherwise), infant mortality (=1 if the child died by age 1, =0 otherwise), and under-five mortality (=1 if the child died by age 5, =0 otherwise).

To investigate the effect of education on female fertility behaviors, I obtain data from the DHS on the number of children ever born, the ideal number of children, age at first marriage and age at first birth, the number of children at age 25 and whether the respondent was married or gave birth when they were aged between 15 and 18. In addition, the DHS collects data on women's labor force participation, occupation, and characteristics of their husbands such as age and educational attainment.

The DHS data also include information on women's empowerment by inquiring women whether they have the power to make decisions in the household. For example, women are asked whether they make decisions alone or along with their husband for the following items: women's earning, own health care, major household purchases, and visiting women's relatives. I created binary variables for each item.[2] To gauge women's attitude against domestic violence, The DHS asks women whether it is justified for a man to beat his wife in the event of burning food, arguing with husband, neglecting children, going out without telling husband, and

[2] Binary indicator will be coded as 1 if women make a decision alone or along with her husband, and as 0 otherwise.

refusing sex. I also create a composite index for women decision making and attitudes toward domestic violence, separately.[3]

Moreover, the DHS provides data on whether the respondent received prenatal care from a skilled provider, whether assisted by a skilled birth attendant, whether delivered in a health facility and the number of months pregnant at time of first prenatal care visit, while this information is only limited to respondents who have children younger than 5 years old at the time of the survey.

To ensure that the individuals around the cutoff are comparable, I focus on individuals who were born near the first affected cohort. Specifically, I restrict the study sample to women who were born up to 40 months (3.3 years) before and after 1971 across the three survey years.[4] This leaves a sub-sample of 24,119 observations. Women born before 1971 (including 1971) or earlier would be considered as the treatment group, while women born after 1971 would be regarded as the control group. The selection of the width of the window around the pivotal cohort will be discussed in the next section. In total, these women give birth to 65,257 children when they were surveyed by the DHS. Table 4.1 provides summary statistics for women by treatment status in the study sample. Women in the study sample have 8.2 years of education on average; 80% of women have completed primary education and 53% of women have completed some secondary education. Women in treatment and control group appear to have similar educational attainment.

[3] The index is a z-score calculated by averaging the z-scores of corresponding items.

[4] We also restrict the study sample to women who have born children and aged over 20 years since women are more likely to complete their schooling by that age.

Table 4.1. Summary Statistics

	Treatment group			Control group			All		
	N	Mean	S.D.	N	Mean	S.D.	N	Mean	S.D.
Panel A: Outcome related to mothers									
Age	11,410	37.11	3.94	12,727	33.84	3.98	24,137	35.38	4.29
Years of education	11,401	8.20	4.28	12,718	8.35	4.05	24,119	8.28	4.16
Complete primary education	11,409	0.79	0.40	12,726	0.83	0.38	24,135	0.81	0.39
At least complete some secondary education	11,409	0.53	0.50	12,726	0.55	0.5	24,135	0.54	0.5
Number of children ever born	11,410	2.90	1.68	12,727	2.53	1.48	24,137	2.7	1.59
Ideal number of children	11,410	3.38	1.72	12,727	3.26	1.67	24,137	3.31	1.69
Age at first birth	10,877	21.65	4.64	12,040	21.52	4.33	22,917	21.58	4.48
Age at first marriage	11,310	20.36	4.85	12,569	20.26	4.5	23,879	20.31	4.67
Number of children under 25	11,410	1.41	1.21	12,727	1.36	1.13	24,137	1.38	1.17
Employed	11,402	0.62	0.49	12,710	0.58	0.49	24,112	0.6	0.49
Self-employed in agriculture	7,353	0.35	0.48	7,749	0.35	0.48	15,102	0.35	0.48
Has a professional/technical/ /managerial job	7,353	0.11	0.31	7,749	0.08	0.28	15,102	0.09	0.29
Husband's education	11,236	8.64	4.38	12,471	8.85	4.18	23,707	8.75	4.28
Age gap between husband and wife	10,591	4.28	5.08	11,919	4.43	5.09	22,510	4.36	5.09
Wealth index	11,410	2.97	1.47	12,727	2.86	1.46	24,137	2.91	1.47
Participate decision about respondent's earning	4,500	0.97	0.18	4,739	0.96	0.2	9,239	0.96	0.19
Participate decision about respondent's health care	10,918	0.86	0.35	12,232	0.86	0.34	23,150	0.86	0.34
Participate decision about household purchase	10,890	0.83	0.37	12,187	0.83	0.38	23,077	0.83	0.37
Participate decision about visiting relatives	10,796	0.89	0.31	12,106	0.88	0.32	22,902	0.89	0.32
Husband is justified in beating wife if she goes out without telling him	11,223	0.24	0.43	12,474	0.25	0.43	23,697	0.25	0.43
Husband is justified in beating wife if she neglects children	11,196	0.25	0.43	12,488	0.26	0.44	23,684	0.26	0.44
Husband is justified in beating wife if she argues with him	11,176	0.07	0.26	12,435	0.08	0.27	23,611	0.08	0.27
Husband is justified in beating wife if she refuses to have sex with him	11,146	0.09	0.28	12,390	0.08	0.28	23,536	0.08	0.28
Husband is justified in beating wife if she burns the food	11,249	0.04	0.20	12,532	0.04	0.19	23,781	0.04	0.19
Smoking	11,410	0.03	0.18	12,724	0.03	0.16	24,134	0.03	0.17
Know modern contraceptive methods	11,410	0.98	0.12	12,727	0.98	0.13	24,137	0.98	0.13

(Table cont'd.)

Table 4.1 (cont'd.)

	Treatment group			Control group			All		
	N	Mean	S.D.	N	Mean	S.D.	N	Mean	S.D.
Currently using modern contraceptive methods	11,410	0.55	0.50	12,727	0.58	0.49	24,137	0.56	0.5
Reading newspaper	11,385	0.75	0.87	12,700	0.75	0.85	24,085	0.75	0.86
Listening to radio	11,388	1.07	1.07	12,705	1.07	1.07	24,093	1.07	1.07
Watching television	11,390	2.15	0.94	12,704	2.14	0.93	24,094	2.14	0.93
Panel B: Prenatal health facility use									
Receiving prenatal care from a skilled provider	4,387	0.87	0.34	6,375	0.88	0.33	10,762	0.88	0.33
Delivery assisted by a health provider	5,297	0.61	0.49	7,753	0.58	0.49	13,050	0.59	0.49
Number of months pregnant at time of first prenatal care visit	4,144	2.66	1.64	6,071	2.62	1.62	10,215	2.64	1.63
Delivered in a health facility	5,319	0.47	0.50	7,781	0.46	0.50	13,100	0.47	0.50
Panel C: Child mortality									
Neonatal mortality	33,098	0.03	0.17	32,159	0.03	0.17	65,257	0.03	0.17
Infant mortality	33,098	0.06	0.23	32,159	0.05	0.22	65,257	0.05	0.23
Under-five mortality	33,098	0.07	0.25	32,159	0.06	0.24	65,257	0.06	0.25

4.4. Empirical Strategy

I first consider the following equation to estimate the impact of female education on child mortality and outcomes related to mothers:

$$Y_i = \alpha_0 + \alpha_1 Schooling_i + \epsilon_i \quad (1)$$

where Y_i indicates the outcome of interest to woman *i*, and $Schooling_i$ represents years of completed schooling of the woman i as measured in the survey data. The coefficient α_1 represents the effect of female education on child mortality. ϵ_i is the error term representing unobservable factors affecting female educational attainment.

Simple OLS estimation of Eq. (1) will provide a biased coefficient estimate of α_1 if female schooling is not exogenously determined as there might be unmeasured individual unobserved factors that are correlated with child mortality and also affect female education. For example, parents would invest more in girls with high ability and those girls may have better health and

care more about children's health. Failing to take account of these factors in the estimation of Eq. (1) will lead to a biased estimate of the impact of maternal education on child mortality outcomes. To mitigate this issue, I take advantage of the exogenous variation in educational attainment generated by the change in the length of the school year in Indonesia in 1978.

One concern regarding the validity of the identification strategy is the manipulation of the running variable: the month-year of birth of women. There are two reasons for this concern should not be a serious issue. First, it is not likely that parents strategically plan the exact date of their child's birth. Second, the policy was announced at mid of 1978 and was implemented in haste, parents were not likely to postpone their children's entry to school when their children were already enrolled in school if they want to avoid the policy of school year extension. Since the year and month of birth is exogenous to students, the implementation of the school year extension policy is as good as random. In the absence of any manipulation, it is arguably expected that individuals who were born just before and after the cutoff should have similar pre-determined characteristics except for the exposure to the longer school year.

Most individuals who were born in 1972 or later were not affected by the longer school year policy because they had not been enrolled in primary school when the policy was implemented in 1978, and most individuals who were born in 1971 or earlier experienced the change in the policy because they were in school when the policy was implemented. However, early or delayed school entry may lead some children older than 7 not to be affected by the policy and some children younger than 7 to be affected by the policy in 1978. Therefore, considering the treatment status to the school year extension policy is not completely determined by their age in 1978, a fuzzy regression discontinuity design is appropriate for the study, which allows for

a jump in the probability of assignment to treatment at the cutoff. I implement the estimation using the following two equations:

$$Schooling_i = \gamma_0 + \gamma_1 T_i + g(x_i) + u_i \quad (2)$$

$$Y_i = \beta_0 + \beta_1 \widehat{Schooling}_i + f(x_i) + v_i \quad (3)$$

The term T_i is an indicator variable that equals to one if the individual *i* was born in 1971 or earlier and equals to zero otherwise. x_i is the running variable: the month-year of birth relative to December 1971. The control function, $f(x_i)$ and $g(x_i)$ are polynomial functions of the running variable on each side of the cutoff point. I use a quadratic polynomial function of the running variable in the baseline analysis and exploit a linear and cubic function in the robustness check. ϵ_i and v_i are unobserved terms for the first and second stage equations, respectively. The regression controls for region fixed effects, survey fixed effects, and women's month-of-birth fixed effects. The regression also includes children's predetermined characteristics, such as gender and birth order.

To study the effect of school year extension policy on child mortality (reduced form), I adopt a similar specification by replacing the outcome variable with child mortality measures in Eq. (2), which is given by

$$Y_i = \lambda_0 + \lambda_1 T_i + h(x_i) + \eta_i \quad (4)$$

where $h(x_i)$ is a polynomial function of x_i, η_i is the unobserved term, and all other variables are as previously defined. In this specification, λ_1 can be interpreted as the effect of the policy of extending the school year.

In the main analysis, I adopt a bandwidth of 40 months near the cutoff, which is obtained from the first-stage results for years of school by using the MSE-optimal bandwidth selection

method suggested by Calonico et al. (2017). In the section of robustness check, I estimate the model with narrower and wider bandwidths to test whether the results are sensitive to the bandwidth choice. Furthermore, I present results employing linear and cubic polynomial function of the running variable in regressions. Standard errors are clustered at the month-year of birth level (Lee and Card, 2008).

4.5. Results

4.5.1. First Stage Results: Impacts of The Policy Change on Maternal Education

Before showing the results, I first present a validity check for the RD design. The identification strategy relies on the assumption that affected cohorts are comparable to unaffected cohorts around the discontinuity threshold. The observed characteristics of individuals should be similar around the cutoff. However, a drawback of the DHS is that it does not include a rich set of information on individual's predetermined observable characteristics. To test the validity of the assumption, I compare the cohort size on each side around the cutoff following Clark and Royer (2013). Specifically, I examine whether the density of the running variable: the month-year of birth, is continuous at the discontinuity. As shown in Figure C.1, there is no evidence of heaping around the cutoff.

I begin the analysis of the effect of the policy by graphically showing the relationship between individuals' month-year of birth and years of completed education in Figure 4.1. The x-axis shows the normalized running variable (month-year of birth relative to December 1971) so that December 1971 is time zero. The dots in the figure represent years of education by month-year of birth cohort. The solid line represents fitted regression lines from the quadratic polynomial specification. Women immediately on the right-hand side of the discontinuity are

the treatment group and women on the left-hand side are the control group. The graph indicates evidence of a notable discontinuity in women' years of education at the cutoff. As shown in Figures 4.2 and 4.3, there is also a discontinuity in the completion of primary school and the completion of some secondary school at the cutoff. It is worth noting that the increase of educational attainment does not extend over a large number of birth months, which implies that the school year extension policy impacts women on the right-hand side near the cutoff more significantly compared to women far away from the cutoff.

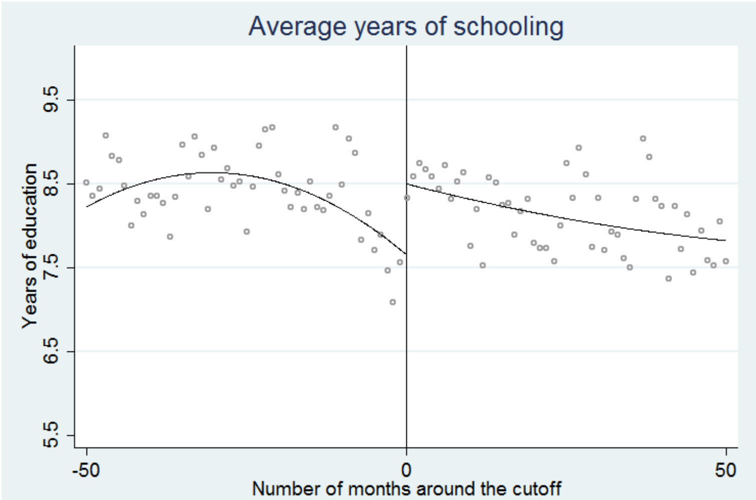

Figure 4.1. Impacts of the Policy on Female Years of Education

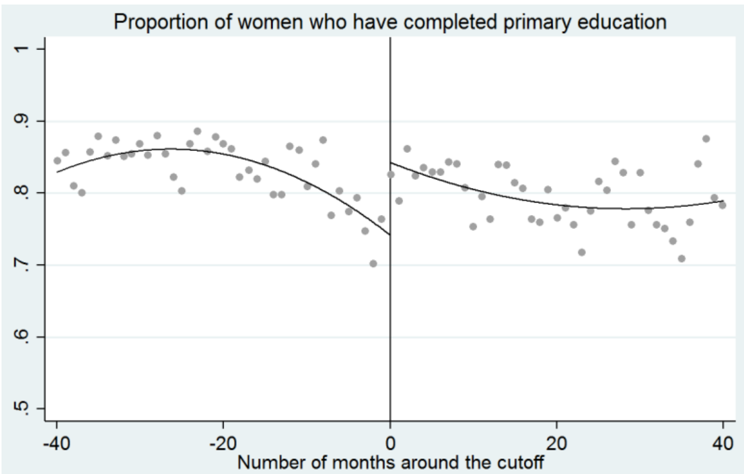

Figure 4.2. Impacts of the Policy on Completion of Primary Education

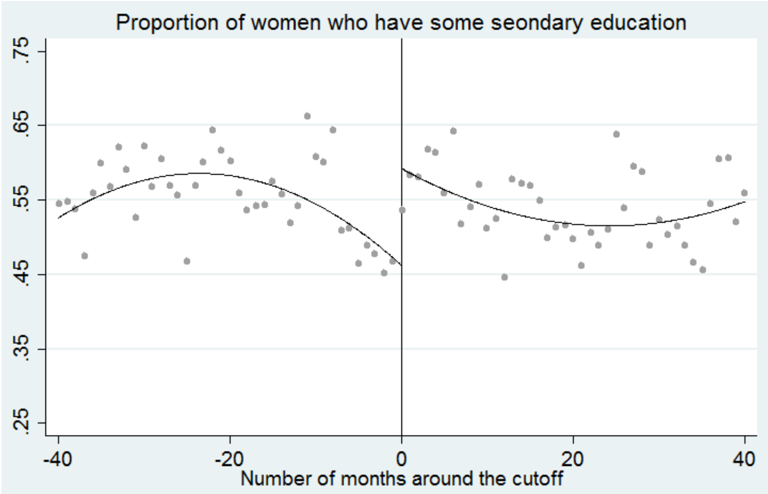

Figure 4.3. Impacts of the Policy on Completion of Some Secondary Education

Table 4.2 presents the first stage regression results of the education policy on women's educational attainment: years of education, primary school completion, and some secondary school completion based on Eq. (2). The latter two measures are binary variables. The coefficient estimates are based on two different specifications: columns (1), (3), and (5) present estimates in the absence of any controls and columns (2), (4), and (6) display the estimates with survey dummies, birth of month fixed effects, and region fixed effects. The estimated coefficients are all positive and statistically significant. The discontinuity in education is associated with an increase in education of 0.82 years (9.8 percent increase), after controlling for other variables. In addition, at the discontinuity, the policy increases the women's probability of completing primary schooling by 6.6 percentage points (8 percent increase), and increases the probability of completing some secondary education by 8.2 percentage points (14.9 percent increase). The results indicate that the education policy not only affects women's primary school completion but also has an impact on women's educational attainment at a higher level.

Table 4.2. Impacts of School Year Extension Policy on Female Education

	Years of schooling		Complete primary schooling		Complete some secondary schooling	
	(1)	(2)	(3)	(4)	(5)	(6)
Policy	1.244***	0.818***	0.101***	0.066***	0.130***	0.082***
	(0.193)	(0.151)	(0.019)	(0.017)	(0.025)	(0.019)
Month of birth FE	N	Y	N	Y	N	Y
Survey FE	N	Y	N	Y	N	Y
Region FE	N	Y	N	Y	N	Y
Observations	24,119	24,119	24,119	24,119	24,119	24,119
Mean of dep. var.	8.35	8.35	0.83	0.83	0.55	0.55

Notes: Standard errors are clustered at the month-year of birth level and reported in parenthesis. Controls in each specification include region fixed effects, birth month fixed effects, year of survey fixed effects.
* p<0.1, ** p<0.05, *** p<0.01

In general, the first stage results indicate that the school year extension policy leads to a substantial increase in female educational attainment. The result is comparable with the estimates from previous studies using the same identification strategy in Indonesia. For example, Prinduri (2014) finds that the 1978 school year extension policy raises female educational attainment by 0.67-0.87 years.

4.5.2. The Impact of Maternal Education on Child Mortality

Having shown that the longer school year policy in 1978 has a significant effect on female education, I turn to the discussion of the causal effects of female education on child mortality outcomes. Figure 4.4 plots the mortality of children born to all women in the study sample. Panel (a) depicts that there is a downward jump in neonatal mortality at the cutoff, which appears to overlap with the timing of the increase in female education. There is also a similar pattern in panels (b) and (c) for infant mortality and under-five mortality, while the magnitude of the jump at the cutoff is relatively small.

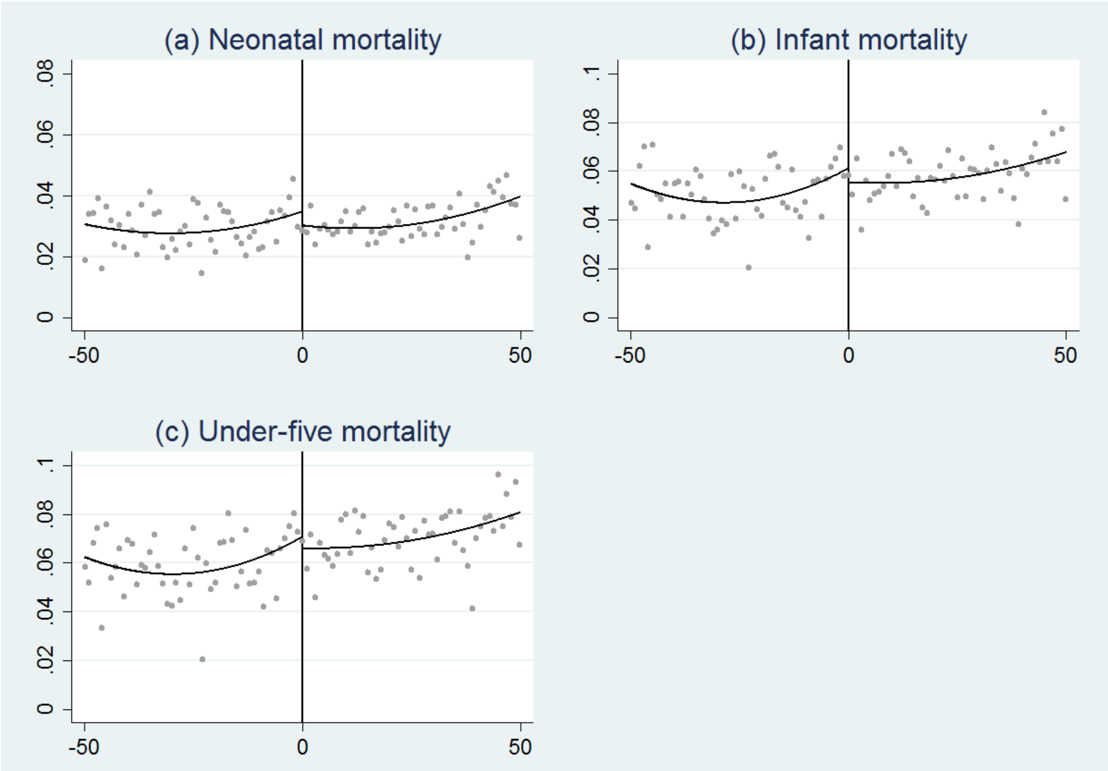

Figure 4.4. Impacts of the Policy on Child Mortality

Table 4.3 presents the main results. To facilitate the comparison, I report the OLS estimation results for Eq. (1) in columns (1), (4), and (7). OLS estimates show that there are negative significant associations between maternal education and indicators for child mortality. Specifically, one additional year of maternal education is associated with a 0.2, 0.5, and 0.7 percentage points decrease in neonatal, infant, and under-five mortality, separately. Columns (3), (6), and (9) show that one additional year of education decreases the neonatal, infant and under-five mortality by 0.8, 0.8, and 0.7 percentage points. The significant effect of the policy is also confirmed by the F-tests of the instrument which is bigger than the threshold of 10. However, the estimate of under-five mortality is not statistically significant. Columns (2), (5), and (7) show that the reduced form estimates for Eq. (4) align with the 2SLS estimates as well as the graph presentation in Figure 4.4, which leads to the same conclusion that increased

schooling resulting from the education policy lowered child mortality, especially for neonatal mortality.

Table 4.3. Impacts of Female Education on Child Mortality

	Neonatal mortality			Infant mortality			Under-five mortality		
	OLS	RF	2SLS	OLS	RF	2SLS	OLS	RF	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Education	-0.002***		-0.008**	-0.005***		-0.008*	-0.007***		-0.007
	(0.000)		(0.003)	(0.000)		(0.004)	(0.000)		(0.005)
Policy		-0.007**			-0.007*			-0.006	
		(0.003)			(0.004)			(0.004)	
Observations	65,208	65,257	65,208	65,208	65,257	65,208	65,208	65,257	65,208
F-test 1st stage			22.49			22.49			22.49
Mean of dep. var.	0.03	0.03	0.03	0.05	0.05	0.05	0.06	0.06	0.06

Notes: Standard errors are clustered at the month-year of birth level and reported in parentheses. Controls in each specification include quadratic function of running variable on either side of the cutoff point, child birth order, child gender, region fixed effects, birth month fixed effects, year of survey fixed effects.
* $p<0.1$, ** $p<0.05$, *** $p<0.01$

The paper conducts a number of robustness checks to examine the sensitiveness of the RD estimates. These results are presented in Table C.1. I first try a narrower bandwidth of 28 and a wider bandwidth of 52. The results in panels A and B show that a one additional year of increase in female education significantly reduces the risk of neonatal death by 0.8 and 0.6 percentage points. Both of these specifications suggest that maternal education does not significantly affect infant mortality and under-five mortality, which is consistent with the baseline results.

Second, I test whether the baseline results are sensitive to the function form of the running variable. Note the trade-off between the order of the polynomial and the bandwidth (Lee and Lemieux, 2010), I present results using linear and cubic function of running variable with the

corresponding optimal-MSE bandwidths in Panels C and D. The magnitude of the estimates is also similar to that of the baseline estimates in Table 4.3.

Taken together, the above results confirm the robustness of the effect of female education on neonatal mortality. While there are negative correlations between female education and infant mortality and under-five mortality, the statistical significance is not consistent in different specifications.

4.5.3. Placebo test

To eliminate the concern that the education reform variable picks up some unspecified time trends, I run a number of placebo tests to validate the effect of female education is not spurious. Specifically, I assign pseudo policies at each side of the time of real policy and estimate the reduced form by Eq. (4). I move the actual policy forward and backward 25 months around the cutoff so that I test 50 placebo reforms in total. Those placebo reforms should have non-significant effects on child mortality. Keeping using a 40-months bandwidth ensures comparability with the baseline estimates. Each panel of Figure 4.5 plots the distribution of placebo estimates along with the baseline reduced form estimates for each child mortality measures. Figure 4.5 also reports the p-value of each placebo test which is defined as the proportion of placebo estimates with an estimated effect that is negative and greater than the magnitude of the baseline estimates. As shown in panel (a) of Figure 4.5, the actual coefficient of school year extension policy for neonatal mortality lies at the far left tail of the placebo estimates distribution. The p-value of placebo neonatal estimates is 0.02, which indicates that the findings for effects of the policy on neonatal mortality do not simply reflect pre-trends or other proximate social or institutional change and it is not likely that the estimates that findings

in the baseline analysis are merely due to randomness. Panels (b) and (c) of Figure 4.5 present that the p-value of the placebo estimates of infant mortality and under-five mortality is 0.20 and 0.24, which corroborates the baseline findings that the estimated treatment effects on infant mortality and under-five mortality might be less robust.

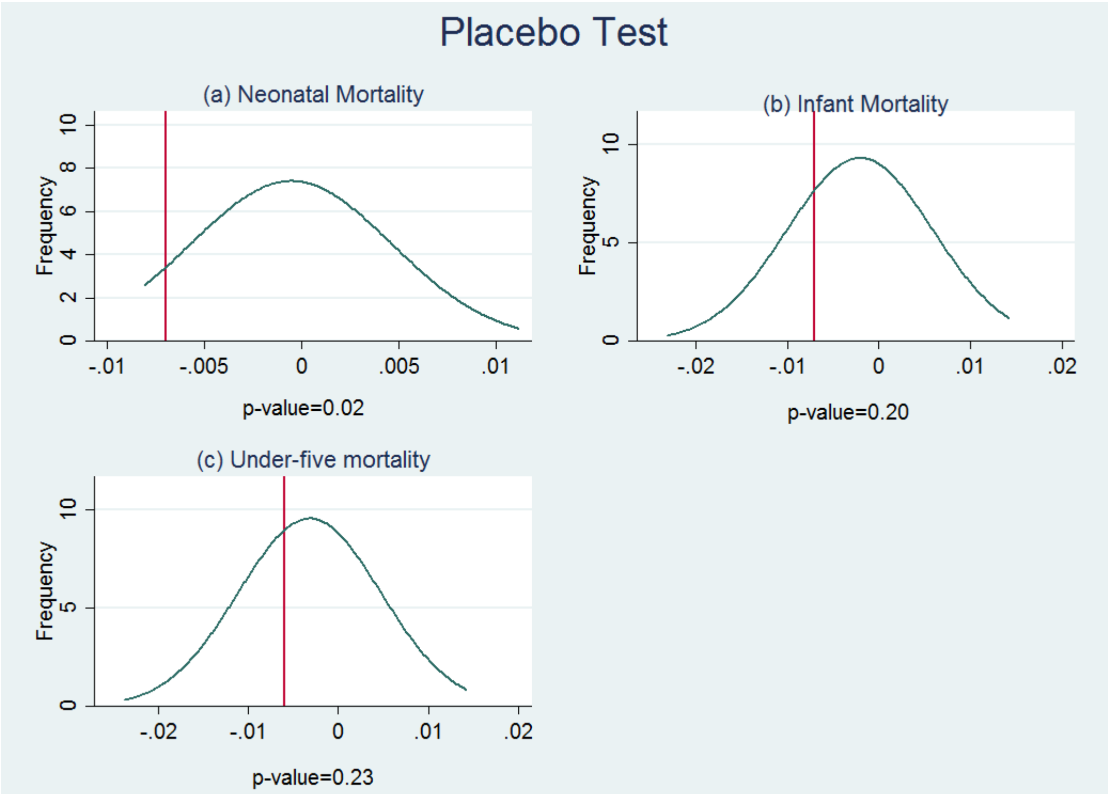

Figure 4.5. Placebo Test

4.6. Mechanisms

This section aims to shed some light on the mechanisms through which increased maternal education might affect child mortality. The results are presented in Tables 4.4-4.12. I also report the reduced form estimates for comparison purpose. The graphs of those intermediate outcome variables are shown in Figures 4.6-4.14.

4.6.1. Fertility Behaviors

I start by examining whether maternal education affects women's fertility behaviors and fertility preferences. Highly educated women may prefer fewer but healthier children, so they would give fewer births to improve the quality of their offspring (Becker and Lewis, 1973). Table 4.4 reports the effect of maternal education on women's fertility behaviors. Columns (1) and (2) show that the coefficients of women's number of children ever born and the ideal number of children have negative signs, as expected, but the magnitude of the estimates is relatively small and not statistically different from zero. Contrary to the theory indication, I do not find that female education has a significant impact on women's fertility preferences, nor do I find that female education decreases women's total fertility.

Table 4.4. Impacts of Female Education on Fertility Behaviors

	Number of Children Ever Born	Ideal Number of Children	Age at First Birth	Age at First Marriage	Number of Children born before 25
	(1)	(2)	(3)	(4)	(5)
Education	-0.027	-0.086	0.528***	0.760***	-0.101**
	(0.079)	(0.060)	(0.188)	(0.205)	(0.049)
Observations	24,119	24,119	22,899	23,861	24,119
F-test 1st stage	29.23	29.23	25.85	30.80	29.23
Mean of dep. var.	2.53	3.26	21.52	20.26	1.36

Notes: Standard errors are clustered at the month-year of birth level and reported in parentheses. Controls in each specification include quadratic function of running variable on either side of the cutoff point, region fixed effects, birth month fixed effects, year of survey fixed effects.
* p<0.1, ** p<0.05, *** p<0.01

Although female education does not affect women's total number of children, the results in columns (3) and (4) demonstrate that increased maternal education delays women's age at first

Figure 4.6. Fertility Behaviors

marriage and age at first birth. An additional year of education increases women's age at first marriage and age at first birth by 0.5 and 0.8 years, separately. Increased female education also has a negative impact on the women's number of children born before 25 years old. Column (5) shows that one more year of female education decreases women's total number of children born before 25 by 0.1. The findings indicate that highly educated women appear to catch up to less-educated peers even if increased education postpones the timing of child-bearing. The delay of the timing of first marriage and first birth could be partly explained by the "incarceration effect" of education because marriage and schooling are not compatible events in general (Black et al., 2008; Kirdar et al., 2018).

Childbearing at a young age has been linked to deleterious impacts on both mothers and children (Schultz, 2007). Previous literature has shown that children who were born to teenage

mothers suffer a higher rate of death compared to children born to mothers older than 18 (Hobcraft et al. 1993). To further examine the impact of increased education on teenage marriage and teenage birth, I investigate whether maternal education affects the probability of getting married and giving birth by age 15 to 18. The results in Tables 4.5 and 4.6 suggest that maternal education decreases the probability of teenage marriage and birth by age 15-18 significantly. Each additional year of female education is estimated to decrease the probability of having first birth by age 15-18 from 93% to 26% and to decrease the likelihood of being married by age 15-18 from 56% to 19% at each age.

Table 4.5. Impacts of Female Education on Teenage Marriage

	Married before 15	Married before 16	Married before 17	Married before 18
	(1)	(2)	(3)	(4)
Education	-0.082***	-0.076***	-0.078***	-0.071***
	(0.017)	(0.017)	(0.020)	(0.023)
Observation	23,861	23,861	23,861	23,861
F-test 1st stage	30.80	30.80	30.80	30.80
Mean of dep. var.	0.14	0.20	0.29	0.38

Notes: Standard errors are clustered at the month-year of birth level and reported in parentheses. Controls in each specification include quadratic function of running variable on either side of the cutoff point, region fixed effects, birth month fixed effects, year of survey fixed effects.
* p<0.1, ** p<0.05, *** p<0.01

Taken together, the estimates suggest large and significant impacts of maternal education on delaying fertility behaviors, which could partially explain the negative effect of female education on child mortality. The result is consistent with the finding by Breierova and Duflo (2004) who makes use of the primary school construction in Indonesia as an instrument and finds that increased female education has no effects on women’s total number of children, but leads to a lower number of early births, and lower child mortality.

Table 4.6. Impacts of Female Education on Teenage Birth

	Give birth before 15	Give birth before 16	Give birth before 17	Give birth before 18
	(1)	(2)	(3)	(4)
Education	-0.056***	-0.054***	-0.070***	-0.065***
	(0.012)	(0.015)	(0.016)	(0.018)
Observation	22,899	22,899	22,899	22,899
F-test 1st stage	25.85	25.85	25.85	25.85
Mean of dep. var.	0.06	0.10	0.17	0.25

Notes: Standard errors are clustered at the month-year of birth level and reported in parentheses. Controls in each specification include quadratic function of running variable on either side of the cutoff point, region fixed effects, birth month fixed effects, year of survey fixed effects.
* p<0.1, ** p<0.05, *** p<0.01

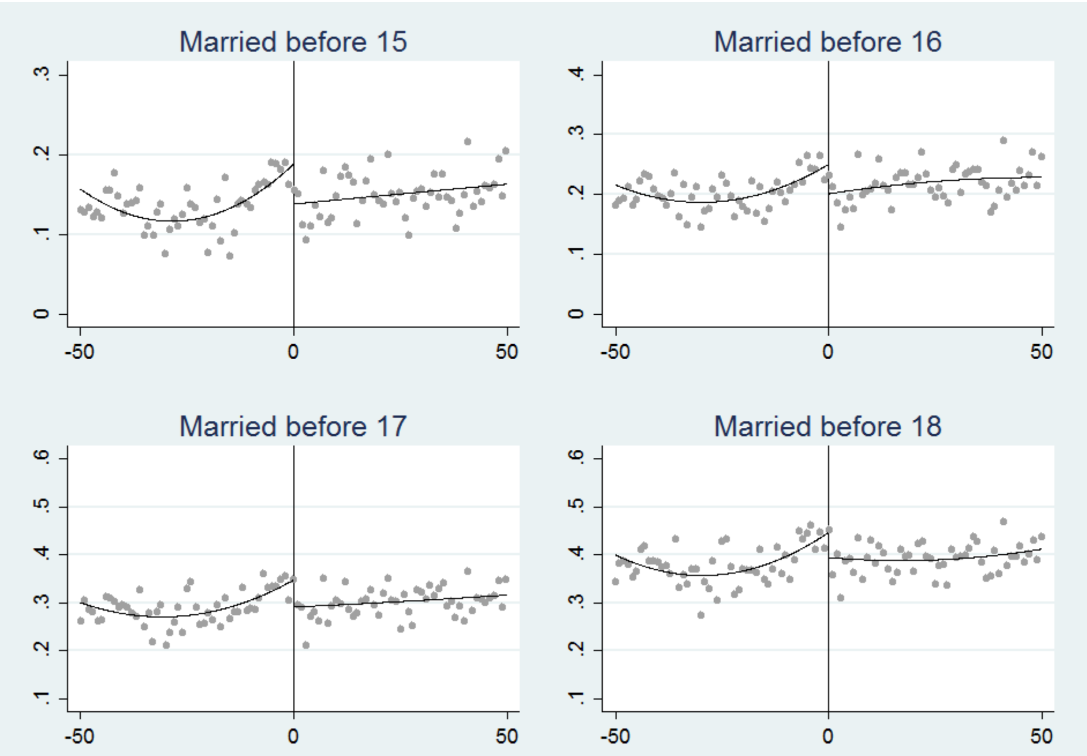

Figure 4.7. Teenage Marriage

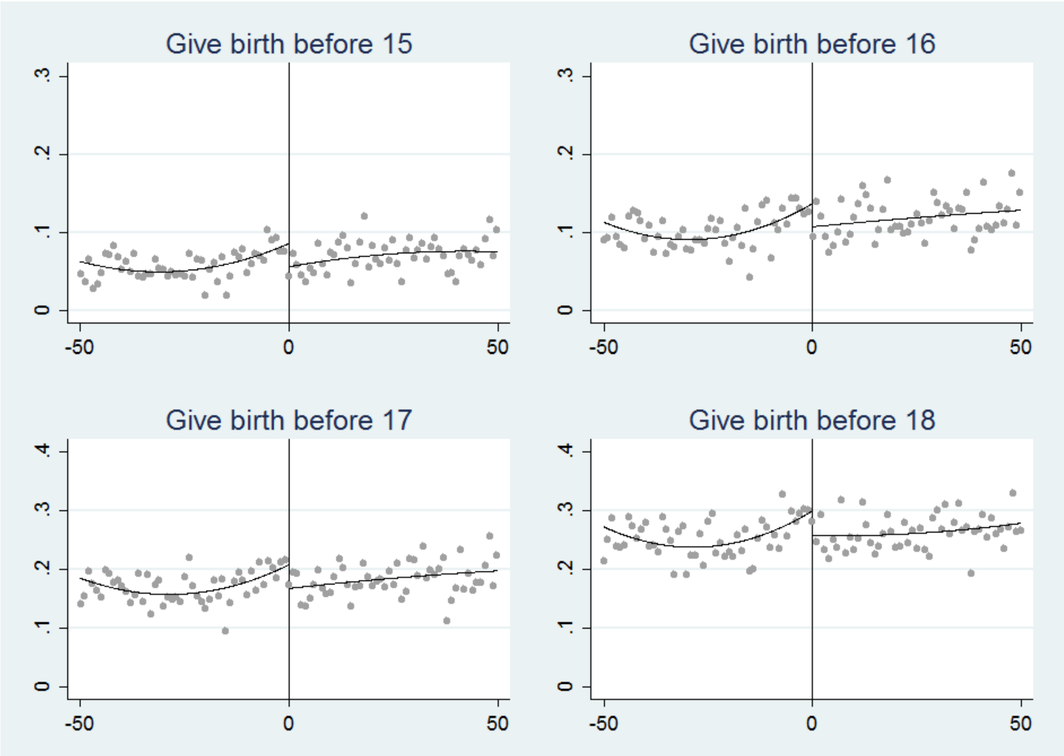

Figure 4.8. Teenage Birth

4.6.2. Spouse Characteristics and Employment Opportunities

I next proceed to examine whether increased female education affects the characteristics of their husbands, women's labor market opportunities, and family wealth status. Columns (1) and (2) in Table 4.7 show that one additional year of female education increases husband's education by about 1 year and decreases the age difference between husband and wife by 0.45 years. Given the average age gap in the study sample is 4.28 years, increased female education reduces the age gap by 10.5 percent.

Columns (3), (4), and (5) in Table 4.7 report the effects of female education on women's probability of being employed, whether the woman has professional/technical/managerial jobs, and whether the woman is self-employed in the agriculture sector. The results suggest that increased female education does not affect the women's labor force participation, but it

increases the probability of having professional/technical/managerial jobs for women by 3.9 percentage points and decreases the probability of being self-employed in the agriculture sector by 4.1 percentage points. In addition, as shown in column (6), increased female education also increases women's composite wealth index of household assets.[5]

A potential problem with the estimation strategy is that the change in the length of the school year may affect both women and men simultaneously. Since husbands are, on average, older than wives by 4.3 years, it is not likely that the husband's education is directly affected by the education policy.

Table 4.7. Impacts of Female Education on Characteristics of Husband and Women's Labor Force Participation

	Husband's Years of Education	Age Difference between Husband and wife	Employed	Professiona l/technical/ managerial job	Self-employed in agriculture sector	Wealth Index
	(1)	(2)	(3)	(4)	(5)	(6)
Education	0.979***	-0.448*	-0.014	0.039***	-0.041**	0.283***
	(0.116)	(0.244)	(0.020)	(0.015)	(0.020)	(0.047)
Observation	23,690	22,494	24,094	15,091	15,091	24,119
F-test 1st stage	31.43	30.50	28.97	16.22	16.22	29.23
Mean of dep. var.	8.85	4.43	0.58	0.08	0.35	2.86

Notes: Standard errors are clustered at the month-year of birth level and reported in parentheses. Controls in each specification include quadratic function of running variable on either side of the cutoff point, region fixed effects, birth month fixed effects, year of survey fixed effects.
* p<0.1, ** p<0.05, *** p<0.01

[5] Wealth index measures the ownership of television, radios, and refrigerators, type of flooring, and water supply, and so forth. DHS separates all interviewed households into five wealth quintiles so that the wealth index varies from 1 (poorest) to 5 (richest). For the details of the wealth index, see https://dhsprogram.com/topics/wealth-index/

Figure 4.9. Women Employment Status, Husband's Characteristics, and Wealth Index

4.6.3. Women Empowerment

Then, I investigate if the increase in female education induced by the longer school year policy changes women's decision power in household and attitudes toward domestic violence. Table 4.8 presents the 2SLS estimates of female education on women's decision autonomy in the household. The results show that the coefficients are all positive but only one out of four items are statistically significant. The composite index of women's autonomy is statistically significant at the 10% level.

Table 4.8 Impacts of Female Education on Household Decision Making

	Decision on Spending	Decision on Health Care	Decision on Large Purchase	Decision on Relatives Visiting	Women Decision Index
	(1)	(2)	(3)	(4)	(5)
Education	0.012	0.034*	0.004	0.019	0.064*
	(0.011)	(0.019)	(0.017)	(0.013)	(0.035)
Observation	9,236	23,150	23,062	22,887	23,229
F-test 1st stage	17.33	30.05	31.69	33.44	30.50
Mean of dep. var.	0.96	0.86	0.83	0.88	-

Notes: Standard errors are clustered at the month-year of birth level and reported in parentheses. Controls in each specification include quadratic function of running variable on either side of the cutoff point, region fixed effects, birth month fixed effects, year of survey fixed effects.
* p<0.1, ** p<0.05, *** p<0.01

Table 4.9 Impacts of Female Education on Women's Attitude towards Domestic Violence

	Beating wife is justified if she goes out without telling him	Beating wife is justified if she neglects the children	Beating wife is justified if she argues with him.	Beating wife is justified if she refuses to have sex with him	Beating wife is justified if she burns the food	Wife Beating Index
	(1)	(2)	(3)	(4)	(5)	(6)
Education	-0.019	-0.025	0.005	0.008	-0.001	-0.019
	(0.020)	(0.018)	(0.010)	(0.011)	(0.007)	(0.026)
Observation	23,682	23,668	23,595	23,521	23,765	23,938
F-test 1st stage	33.77	35.95	35.70	32.68	35.86	32.86
Mean of dep. var.	0.25	0.26	0.08	0.08	0.04	-

Notes: Standard errors are clustered at the month-year of birth level and reported in parentheses. Controls in each specification include quadratic function of running variable on either side of the cutoff point, region fixed effects, birth month fixed effects, year of survey fixed effects.
* p<0.1, ** p<0.05, *** p<0.01

Figure 4.10 Women Decision Making Power

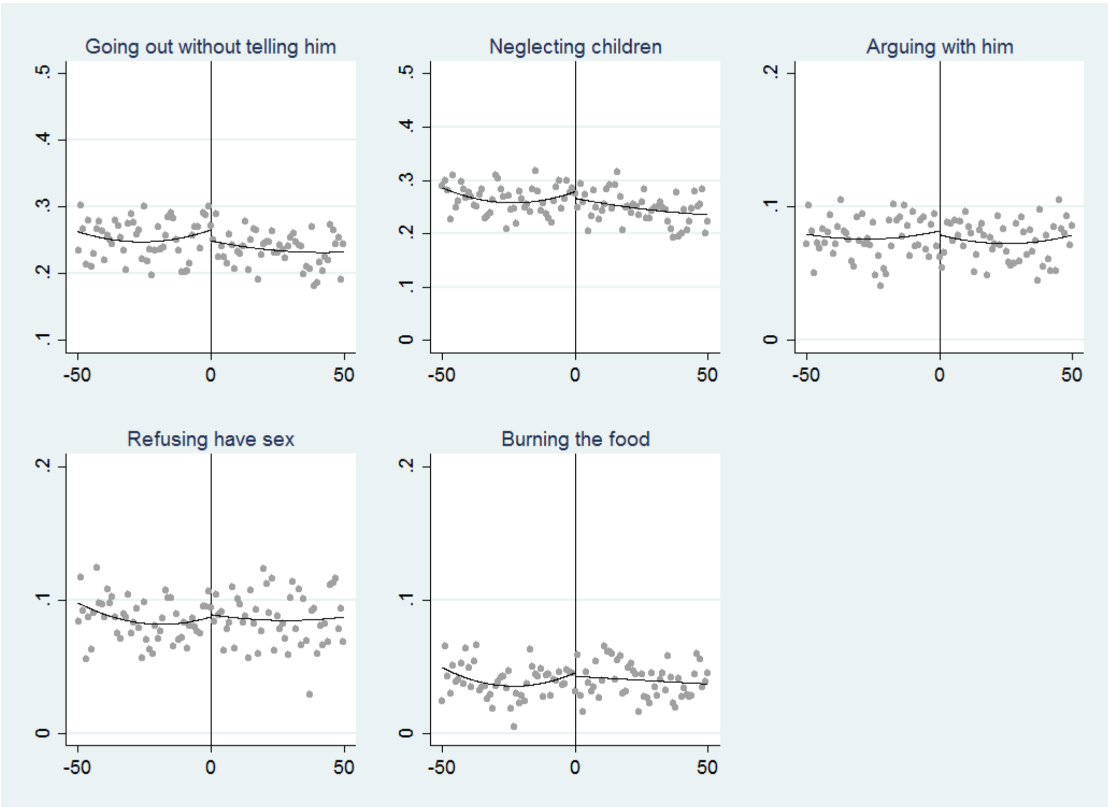

Figure 4.11 Women's Attitudes towards Beating Wife

Estimates of the causal effect of female education on women's attitudes toward domestic violence are reported in Table 4.9. These estimates have mixed sign and are all statistically insignificant, indicating that the decline in child mortality observed in Table 4.3 is not likely to be on account of a change in women empowerment in household.

4.6.4. Health Awareness and Prenatal Health Care Use

Highly educated women might be likely to increase the health inputs and keep their children healthy more effectively (Rosenzweig and Schultz 1989). To test this potential channel, I investigate the effect of education on female's health-seeking behaviors. Columns (1) to (3) of Table 4.10 show that increased female education decreases the women's probability of smoking by 1.7 percentage points and increases women's awareness of modern contraceptive methods by 1.9 percentage points although it does not seem to affect the use of modern contraceptive methods.[6]

Table 4.10. Impacts of Female Education on Health Awareness

	Do you smoke?	Do you know modern contraceptive method?	Currently using modern contraceptive method
	(1)	(2)	(3)
Education	-0.017**	0.019***	0.010
	(0.009)	(0.005)	(0.020)
Observations	24,116	24,119	24,119
F-test 1st stage	29.46	29.23	29.23
Mean of dep. var.	0.03	0.98	0.58

Notes: Standard errors are clustered at the month-year of birth level and reported in parentheses. Controls in each specification include quadratic function of running variable on either side of the cutoff point, region fixed effects, birth month fixed effects, year of survey fixed effects.
* p<0.1, ** p<0.05, *** p<0.01

[6] Modern contraceptive methods include female sterilization, male sterilization, pill, intrauterine device (IUD), injectables, implants, male condom, diaphragm, lactational amenorrhea method (LAM), and emergency contraception.

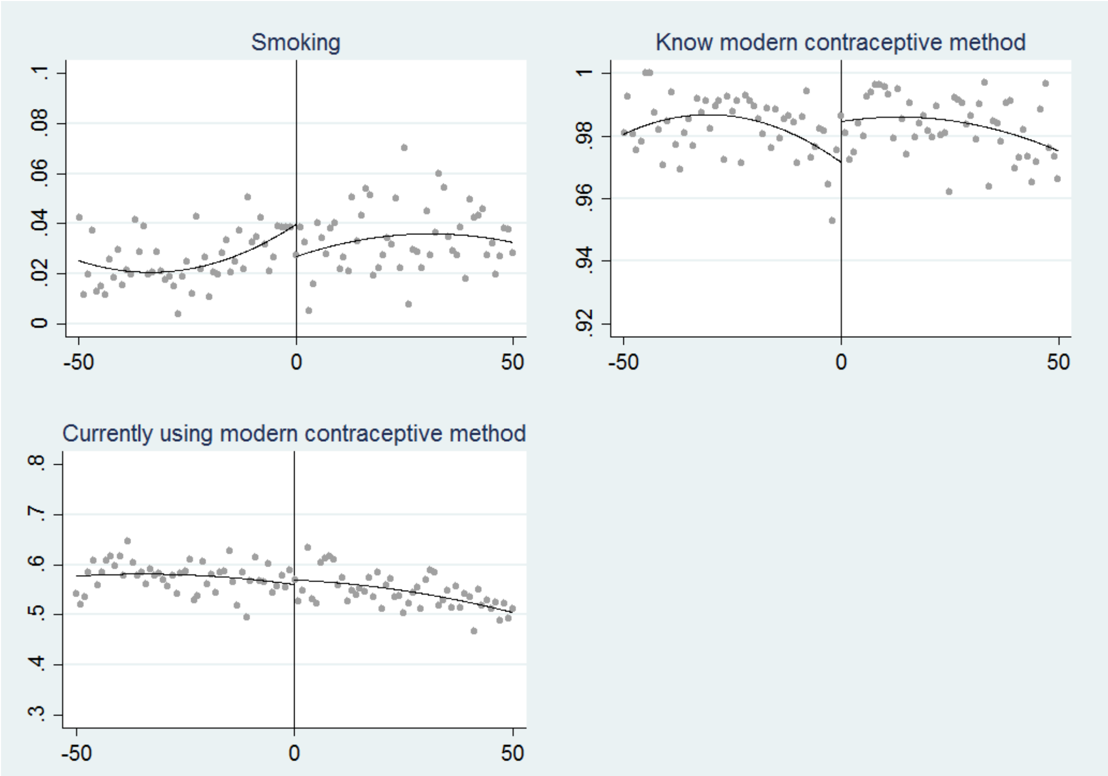

Figure 4.12 Health Behaviors

Columns (1) and (3) of Table 4.11 show that increased female education increases the likelihood of receiving antenatal care from a skilled provider and be assisted by skilled providers at childbirth.[7] Furthermore, column (2) indicates that female education also leads women to show up early for their first antenatal care visit. Each additional year of education decreases the duration of pregnancy at the time of first antenatal care visit by 0.3 months, which corresponds to about a 10 percent reduction. Column (4) shows that higher female education has positive impacts on the likelihood of a woman delivering her children at a health facility, such as hospital and clinic rather than at home. An additional year of schooling increases the likelihood of delivering at a health facility by 9.3 percentage points. This is a large effect considering that only 40% of women on average delivered in a health facility. The F-test for

[7] Skilled provider includes doctor, obstetrician, nurse, midwife, and village midwife.

Tables 4.11 Impacts of Female Education on Health Awareness and Prenatal Health Care Use

	Receiving Antenatal Care from a Skilled Provider	Number of Months Pregnant at the time of first prenatal care visit	Birth Assisted by Skilled Providers	Delivered in a Health Facility
	(1)	(2)	(3)	(4)
Education	0.045**	-0.289**	0.066***	0.093***
	(0.021)	(0.118)	(0.025)	(0.032)
Observations	10,753	10,207	13,040	13,090
F-test 1st stage	6.869	7.826	9.100	9.025
Mean of dep. var.	0.88	2.62	0.58	0.46

Notes: Standard errors are clustered at the month-year of birth level and reported in parentheses. Controls in each specification include quadratic function of running variable on either side of the cutoff point, region fixed effects, birth month fixed effects, year of survey fixed effects.
* p<0.1, ** p<0.05, *** p<0.01

the first stage in columns (1)-(4) varies from 6.87 to 9.1 which are smaller than the threshold of 10, but all of them exceed the value of 5, a less strict rule of thumb proposed by Cameron and Trivedi (2005). A limitation of the above analysis is that the data on prenatal health care inputs are limited to women who have children younger than 5 years old prior to the survey. Therefore, the results should be explained as suggestive evidence.

4.6.5. Media Usage

Mass media could be critical credible sources of health information for women by providing information relevant to children's health, such as information about the importance of vaccination, micronutrients, or preventive measures against diarrhea. In the DHS, the extent to which exposure to media is assessed by asking a respondent how often she reads newspapers, listens to the radio and watches television, with three response options: not at all, less than once a week, at least once a week, and almost every day. The above options are coded from 0 to 3.

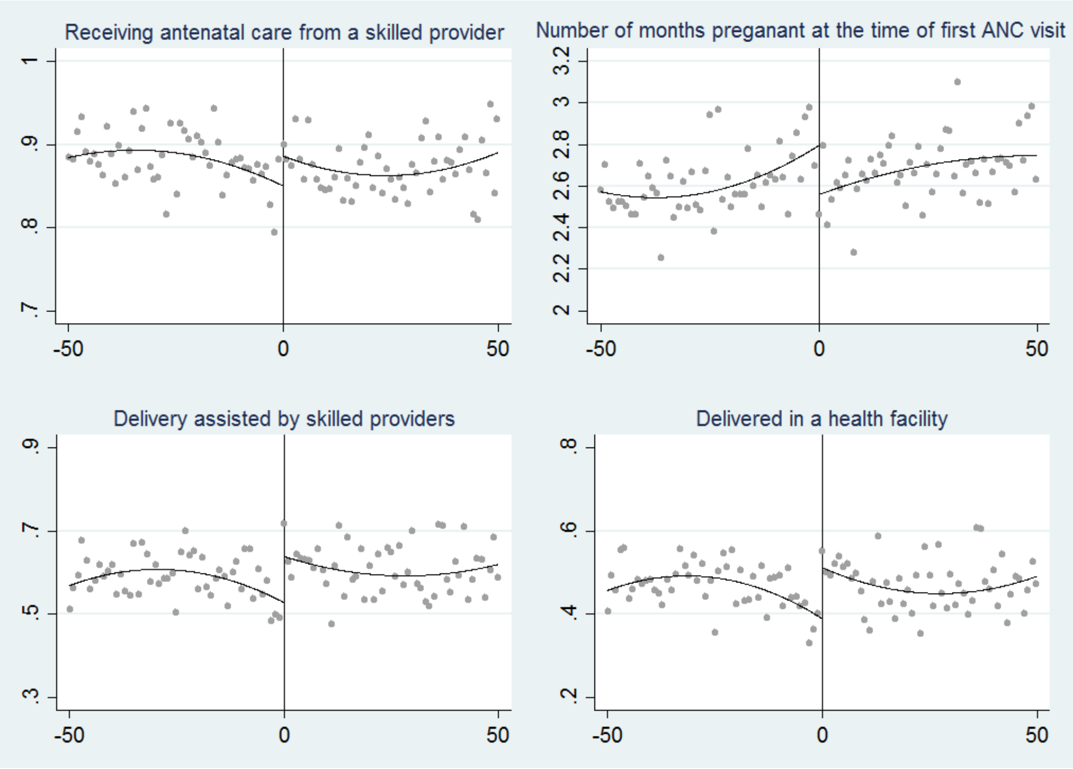

Figure 4.13. Prenatal Health Care Use

To facilitate the interpretation, I standardize each item to a mean of zero and a standard deviation of 1. I also generate an index by averaging these three standardized items. Summary statistics in Table 4.1 show that television is the most popular mass media for women in Indonesia, while exposure to radio and print media is relatively lower. The results in Table 4.12 suggest that the estimates of the coefficients are all statistically significant, which indicates that women with higher educational attainment are more likely to increase the use of mass media compared to women with less schooling.

4.7. Conclusions

The paper provides evidence on the causal intergenerational effects of maternal education on child mortality and tentatively explores pathways through which maternal education operates using data from three waves of the Indonesia Demographic and Health Survey. Despite the

Table 4.12. Impacts of Female Education on Mass Media Use

	How often do you read Newspaper	How often do you listen Radio	How often do you watch TV	Media Usage Index
	(1)	(2)	(3)	(4)
Education	0.151***	0.076*	0.099***	0.119***
	(0.049)	(0.046)	(0.036)	(0.037)
Observation	24,067	24,075	24,076	24,100
F-test 1st stage	30.01	30.02	30.07	29.68
Mean of dep. var.	0.75	1.07	2.14	-

Notes: Standard errors are clustered at the month-year of birth level and reported in parentheses. Controls in each specification include quadratic function of running variable on either side of the cutoff point, region fixed effects, birth month fixed effects, year of survey fixed effects.
* p<0.1, ** p<0.05, *** p<0.01

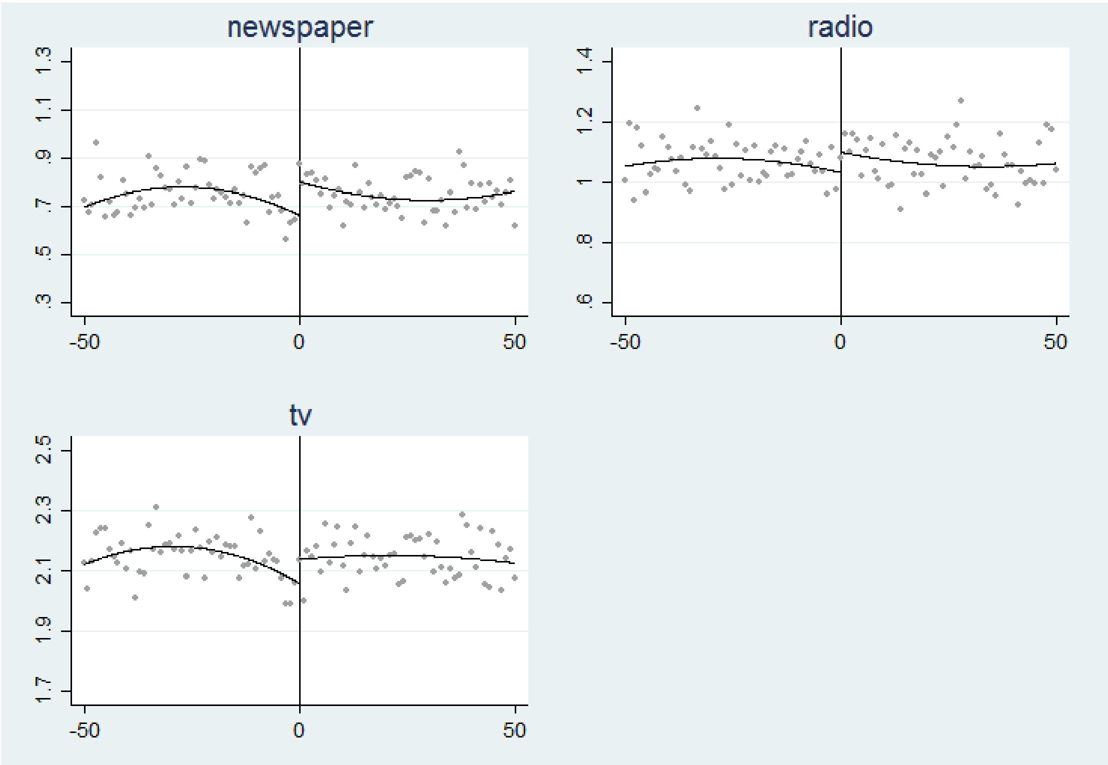

Figure 4.14. Mass Media Use

potential importance of maternal education, studies on the intergenerational effect of maternal education are sparse, especially in the context of developing countries. The current study makes use of the nationally implemented education reform in Indonesia in 1978 as an exogenous source of change in individual's educational attainment to circumvent the endogeneity problem

of education. The analysis shows that women who were affected by the longer school year have competed, on average, additional 0.82 years of schooling as compared to women who were not exposed to the policy. Using the education reform as an exogenous instrument for educational attainment, the paper finds that increased maternal education decreases neonatal mortality by 0.8 percentage points, which transmit to a 26% reduction. The results remain robust to a number of placebo and sensitivity tests. However, I do not find significant and robust effects of female education on infant mortality and under-five mortality.

The mechanism analysis shows that the decrease in child mortality is not likely driven by the quality-quantity tradeoff suggested by Becker and Lewis (1973) since the increase in female education did not statistically significant affect women's overall fertility and the ideal number of children. Additionally, I find that the increase in female education postpones women's age at first marriage and age at first birth, and decreases the likelihood of teenage marriage and teenage birth. Moreover, I provide evidence that female education does not significantly affect women's labor force participation and modern contraceptive method use, although I find that increased female education affects women's occupation and awareness of modern contraceptive methods. The analysis also establishes that higher education leads female to engage in health-seeking behaviors, such as decreasing smoking rates and increasing the prenatal health care use, while there is little evidence that female education increases women's autonomy in terms of household decision making and attitudes against domestic violence.

In summary, the study adds new evidence on the intergenerational effects of female education. The findings about the protective effects of maternal education on child mortality have important policy implications for public policies in Indonesia and other middle-income

developing countries. Policies that aim to promote female schooling might be an effective approach to improve child health outcomes and to reduce inequality transmitted across generations.

CHAPTER 5. CONCLUSION

5.1. Classroom Composition and Student Academic Achievement: The Impact of Peers' Parental Education and Peers' Gender

In this paper, I use the China Education Panel Survey (CEPS) 2013-2014, a national level representative survey of middle school students and exploit the randomized student assignments to examine the existence and potential pathways of the impact of peer parental education and peer female proportion on educational outcomes in Chinese middle schools.

The results suggest that having peers with high-educated parents positively affects both male and female students' scholastic performance as measured by test scores. One additional year of average peer parental education increases students' test scores by 0.152 standard deviations for male students and 0.138 standard deviations for female students. In addition, having a higher proportion of female peers in the classroom improves male students' test scores. Specifically, a 10 percentage point increase in female student proportion in the classroom raises male students' test score by 0.141 standard deviations. The paper also finds that peer effects work in a heterogeneous pattern: students who have a higher parental education benefit more from peer groups characterized by higher levels of parental education compared to students with medium- or less-educated parents. The estimates from using data of schools where students are not randomly assigned to classrooms indicate that neglecting the nonrandom student assignment within schools would induce severe upward bias in estimated peer effects. An exploration of the potential mechanisms shows that higher peer parental education may improve students' academic outcomes through academic quality of peers, students' perception of the classroom atmosphere, and behaviors of students' classroom friends, but these channels are heterogeneous across genders. There is no evidence that the parental education and the

proportion of female students at the classroom level influence teachers' weekly working hours and pedagogical methods.

5.2. The Impact of Female Education on Fertility: Evidence from Malawi Universal Primary Education Program

Chapter 3 uses an exogenous variation in education caused by the Universal Primary Education (UPE) policy in Malawi to examine the extent to which female education impacts fertility. The estimation strategy identifies the local average treatment effect of female education on fertility, which is the effect of schooling for those women who changed their education decisions due to the UPE policy.

The results show that the UPE policy positively affects rural women's educational attainment and that increased female education has a negative impact on fertility. Specifically, the education policy increases rural women's schooling attainment by 0.42 years. An additional year of female schooling decreases women's number of children ever born and number of living children by 0.39 and 0.34, respectively. A variety of specification and placebo tests reveal the robustness of the findings. The results are consistent with the causal findings of previous studies using data from sub-Saharan countries (Grepin and Bharadwaj, 2015; Keats, 2018; Osili and Long, 2008). Compared to previous studies, the current paper additionally finds that higher female education appears to lower women's completed number of births at their mid-30s. The results of the paper suggest that promoting female education might be an effective policy of decreasing fertility in Malawi.

Additionally, using the rich set of information in the DHS, I investigate possible mechanisms by which education might affect fertility. The results indicate that increased female education

decreases women's desired number of children, postpones their marriage age and maternal age. However, there is no evidence that female education influences women's labor market participation and occupation, which suggests that education plays a limited role in reducing fertility through the channel of increasing opportunity cost of women's time in an agriculture-based economy.

5.3. The Effect of Maternal Education on Child Mortality in Indonesia

The paper provides evidence on the causal intergenerational effects of maternal education on child mortality and tentatively explores pathways through which maternal education operates using data from three waves of the Indonesia Demographic and Health Survey. The current study makes use of the nationally implemented education reform in Indonesia in 1978 as an exogenous source of change in individual's educational attainment to circumvent the endogeneity problem of education.

The paper shows that women who were affected by the longer school year have competed, on average, additional 0.82 years of schooling as compared to women who were not exposed to the policy. Using the education reform as an exogenous instrument for educational attainment, the paper finds that increased maternal education decreases neonatal mortality by 0.8 percentage points, which transmit to a 26% reduction. However, I do not find significant and robust effects of female education on infant mortality and under-five mortality. The results remain robust to a number of placebo and sensitivity tests.

The mechanism analysis shows that the increase in female education postpones women's age at first marriage and age at first birth, and decreases the likelihood of teenage marriage and teenage birth. Moreover, I provide evidence that female education does not significantly affect

women's labor force participation and modern contraceptive method use, although I find that increased female education affects women's occupation and awareness of modern contraceptive methods. The analysis also establishes that higher education leads female to engage in health-seeking behaviors, such as decreasing smoking rates and increasing the prenatal health care use, while there is little evidence that female education increases women's autonomy in terms of household decision making and attitudes against domestic violence.

APPENDIX A. SUPPLEMENTARY FIGURES AND TABLES FOR CHAPTER 2

Table A.1. Summary Statistics of Teachers and Intermediate Outcomes

	Mean	Standard deviation	Number of observations
Panel A: Teacher characteristics			
Female	0.79	0.41	593
Years of experience	15.96	9.19	593
Years of education	16.01	0.69	593
Panel B: Classroom environment			
My class is in good atmosphere (1=strongly disagree, 4=strongly agree)	3.26	0.85	7,442
Most of my classmates are friendly to me. (1=strongly disagree, 4=strongly agree)	3.33	0.80	7,440
I feel close to people in school (1=strongly disagree, 4=strongly agree)	3.06	0.91	7,389
Panel C: Time allocation			
Studying (hours/per week)	14.22	9.51	7,591
Entertainment (hours/per week)	13.97	18.68	7,229
Panel D: Habitual tardiness and absenteeism			
I am always late for class (1=strongly disagree, 4=strongly agree)	1.22	0.59	7,566
I always skip class (1=strongly disagree, 4=strongly agree)	1.08	0.40	7,562
Panel E: Friends' behaviors			
Do well in academic performance (1=None of them, 3=Most of them)	2.38	0.59	7,484
Expect to go to college (1=None of them, 3=Most of them)	2.65	0.57	7,452
Skipping class (1=None of them, 3=Most of them)	1.09	0.32	7,456
Punished for violating school rules (1=None of them, 3=Most of them)	1.11	0.36	7,474
Fight with others (1=None of them, 3=Most of them	1.12	0.36	7,479
Panel F: Teacher's teaching efforts and teaching methods			
Total working hours last week	45.95	22.63	585
I often discuss the subject with my colleagues (1=No, 2=Yes)	1.01	0.08	593
How often do you apply "lectures" in teaching? (1=Never, 5=Always)	3.94	0.67	591
How often do you apply "group discussion" in teaching? (1=Never, 5=Always)	3.47	0.88	595
How often do you apply "interaction with students" in teaching? (1=Never, 5=Always)	3.91	0.75	595

Table A.2. Random Assignment Test of Teacher Characteristics

	Peer Female Proportion	Peer Parental Education
Panel A: Chinese teacher		
Female	-0.032	0.014
	(0.394)	(0.054)
Years of education	-1.031	-0.071
	(0.919)	(0.085)
Years of experience	6.210	1.111
	(12.353)	(1.384)
Panel B: Math teacher		
Female	-0.673	0.043
	(0.490)	(0.079)
Years of education	-0.558	-0.118
	(0.884)	(0.072)
Years of experience	2.009	0.966
	(10.729)	(1.354)
Panel C: English teacher		
Female	-0.008	0.051
	(0.426)	(0.040)
Years of education	0.420	-0.117
	(0.704)	(0.078)
Years of experience	5.000	0.973
	(8.354)	(1.267)

Notes: All specifications include school-by-grade fixed effects. Standard errors are clustered at the school-by-grade level and reported in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table A.3. Decomposition of Variance in the Proportion of Female Students and Parental Education

	Female proportion	Parental education
Within grade-by-school	0.002	0.183
Between grade-by-school	0.006	4.126
Total	0.008	4.309
(%) of Within grade-by-school variation in total variation	25.00%	4.25%

Table A.4. Estimates of Peer Effects on Test Scores Using Nonrandomized Sample

	All (1)	Male (2)	Female (3)
Peer Parental Education	0.231***	0.257***	0.207***
	(0.049)	(0.056)	(0.049)
Peer Female Proportion	2.790***	2.837***	2.731***
	(0.391)	(0.476)	(0.416)
Observations	9,146	4,651	4,495
R-squared	0.103	0.079	0.097

Notes: Test scores are standardized by grade and school. Student control variables include student's age, gender, number of siblings, parental education, and dummy variables indicating race, hukou status, whether attended kindergarten, whether repeated a grade in primary school. Teacher control variables include teacher's gender, years of experience, and years of education. Peer parental education and peer female proportion are measured at classroom level. All specifications include school-by-grade fixed effects. Standard errors are clustered at the school-by-grade level and reported in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table A.5. Estimates of Peer effects on Students' Academic Achievement Using Subsamples of Grade 7 and Grade 9

	Grade 7		Grade 9	
	Male (1)	Female (2)	Male (3)	Female (4)
Peer Parental Education	0.206***	0.105**	0.094*	0.154***
	(0.073)	(0.051)	(0.051)	(0.044)
Peer Female Proportion	1.349	0.271	1.522***	0.002
	(0.894)	(0.666)	(0.412)	(0.498)
Observations	2,155	2,008	1,725	1,759
R-squared	0.068	0.078	0.068	0.085

Notes: Test scores are standardized by grade and school. Student control variables include student's age, gender, number of siblings, parental education, and dummy variables indicating race, hukou status, whether attended kindergarten, whether repeated a grade in primary school. Teacher control variables include teacher's gender, years of experience, and years of education. Peer parental education and peer female proportion are measured at classroom level. All specifications include school-by-grade fixed effects. Standard errors are clustered at the school-by-grade level and reported in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

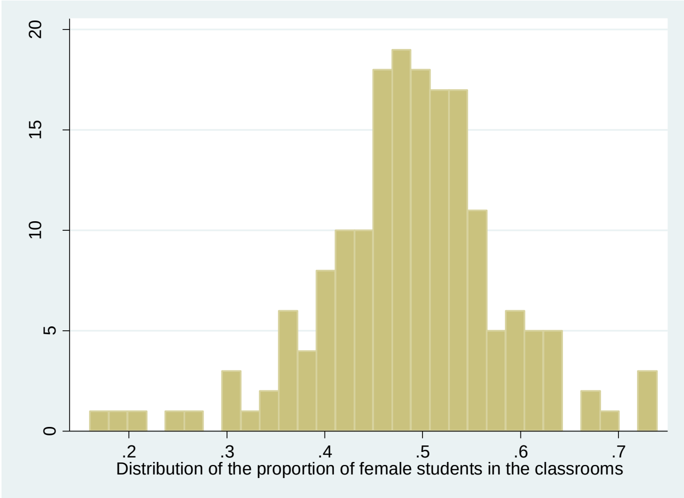

Figure A.1. Distribution of the proportion of female students in the classrooms

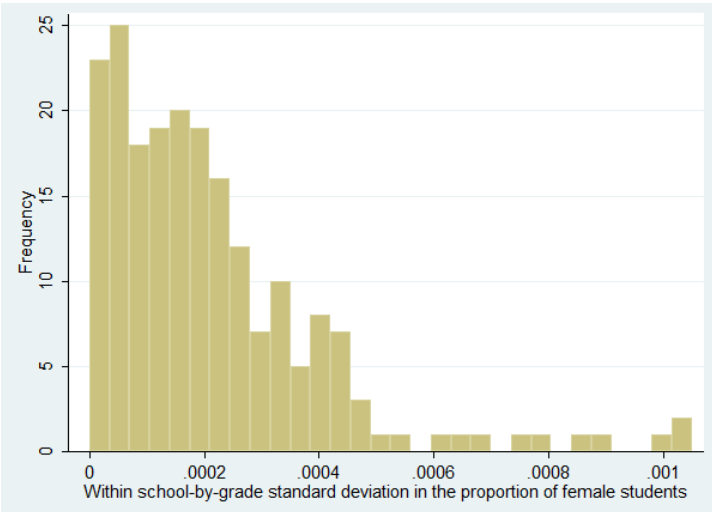

Figure A.2. Distribution of within grade-by-school standard deviation in the proportion of female students

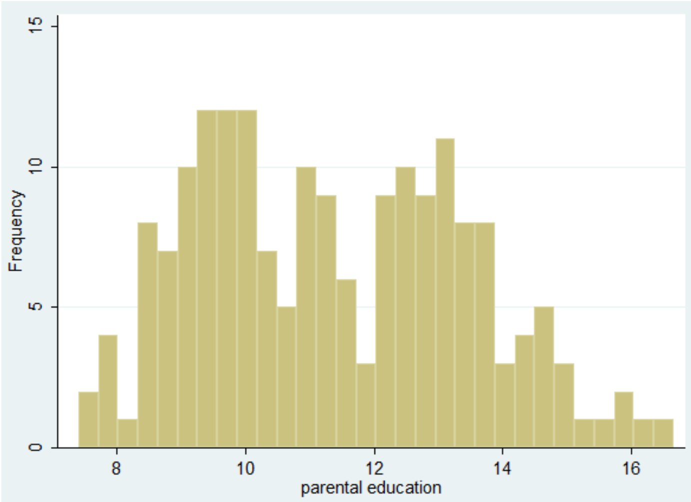

Figure A.3. Distribution of the average parental education in the classrooms

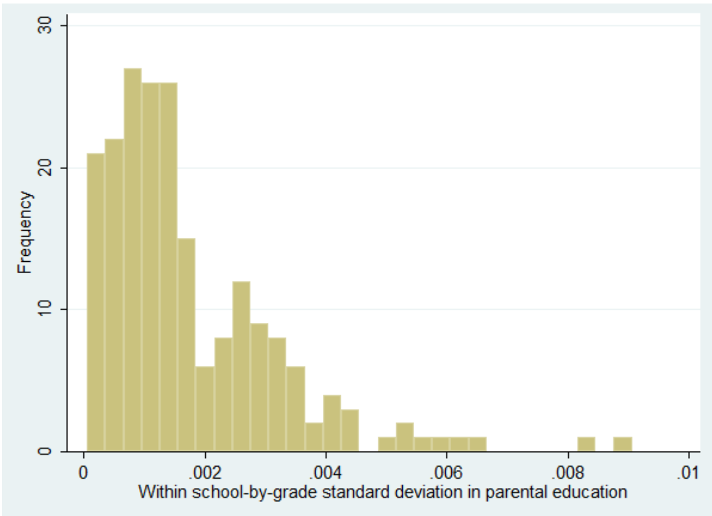

Figure A.4. Distribution of within grade-by-school standard deviation in parental education

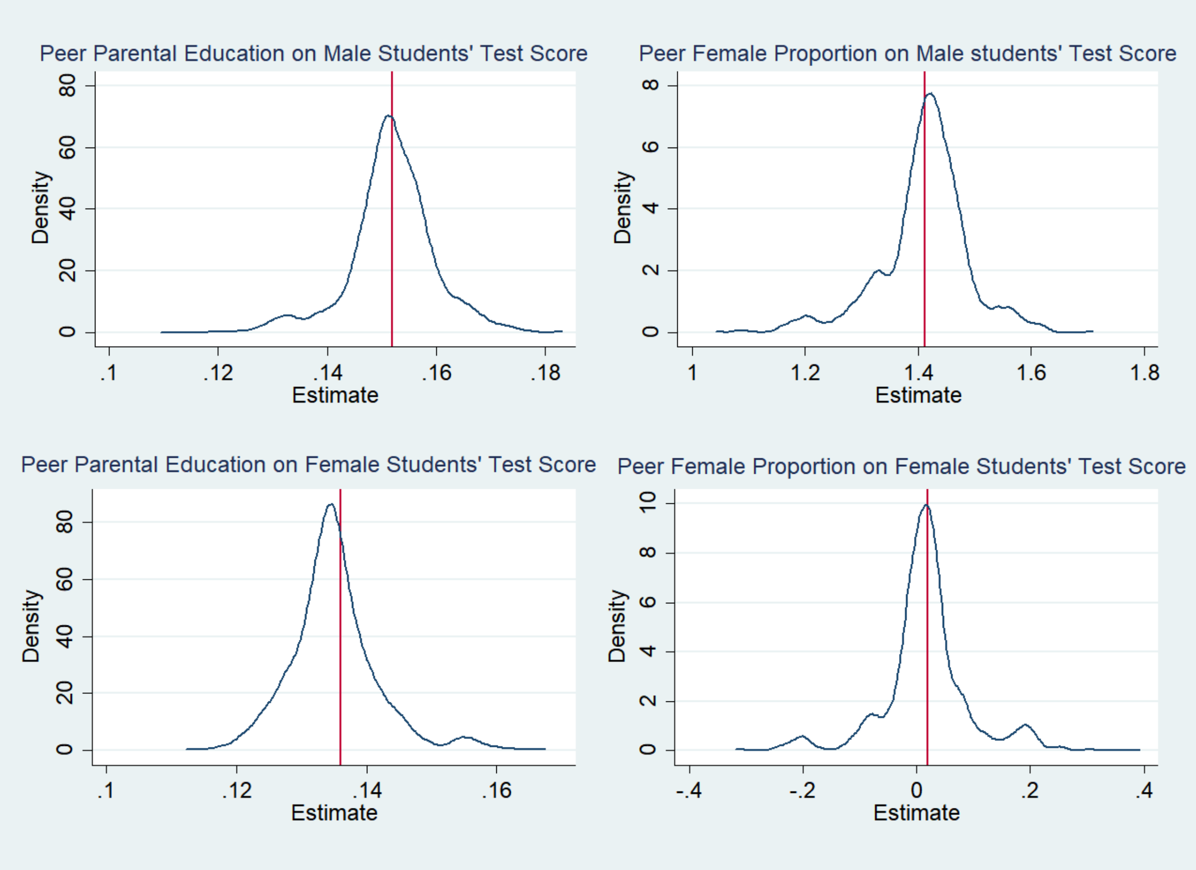

Figure A.5. Distribution of Estimates on Students' Test Score

APPENDIX B. SUPPLEMENTARY FIGURES AND TABLES FOR CHAPTER 3

Table B.1. Summary Statistics (PHC)

	All	Treated group (Ages 10-15 in 1994)	Control group (Ages 17-22 in 1994)	Difference (2)-(3)
	(1)	(2)	(3)	(4)
Predetermined Variable				
Age	28.94	26.11	33.21	-7.10***
	(3.89)	(1.72)	(1.77)	(0.01)
Christian (=1 if religion is Christian, =0 otherwise)	0.84	0.83	0.84	-0.01***
	(0.37)	(0.37)	(0.36)	(0.00)
Currently Married (=1 if currently married, =0 otherwise)	0.88	0.89	0.85	0.04***
	(0.33)	(0.31)	(0.35)	(0.00)
Chewa (=1 if ethnicity is Chewa, =0 otherwise)	0.33	0.32	0.34	-0.02***
	(0.47)	(0.47)	(0.47)	(0.00)
Rural (=1 if living in rural areas, =0 otherwise)	0.84	0.83	0.85	-0.02***
	(0.37)	(0.37)	(0.35)	(0.00)
Years of Education	4.88	5.32	4.23	1.09***
	(4.03)	(3.98)	(4.02)	(0.03)
Outcome variable				
Number of children ever born	3.51	3.80	4.59	-1.78***
	(1.98)	(1.46)	(2.07)	(0.01)
Number of total living children	3.12	2.56	3.96	-1.39***
	(1.76)	(1.22)	(1.68)	(0.01)
Number of Observations	106,297	63,913	42,384	

Notes: Standard errors reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table B.2. Balance Test

	Christian	Chewa	Currently married
	(1)	(2)	(3)
Panel A: DHS			
Policy	0.016	0.017*	0.001
	(0.017)	(0.010)	(0.013)
District FE	Yes	Yes	Yes
District Trends	Yes	Yes	Yes
Observations	16,367	16,367	16,367
Mean of dep. var	0.61	0.31	0.77
Panel B: PHC			
Policy	-0.008	-0.004	0.009*
	(0.005)	(0.004)	(0.005)
District FE	Yes	Yes	Yes
District Trends	Yes	Yes	Yes
Observations	88,699	88,699	88,699
Mean of dep. var	0.84	0.33	0.88

Notes: Standard errors reported in parentheses. Each specification controls for district fixed effects, district specific time trends. Specifications in Panel A additionally control for year of survey fixed effects. * p<0.1, ** p<0.05, *** p<0.01

Table B.3. First Stage Results: The Impact of The UPE Policy on Female Schooling Attainment Using Data of PHC

	All	Rural	Urban	All	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Reform 1994	0.292***	0.328***	0.117	0.310***	0.331***	0.158
	(0.075)	(0.073)	(0.150)	(0.065)	(0.068)	(0.132)
Controls	No	No	No	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
District Trends	Yes	Yes	Yes	Yes	Yes	Yes
First Stage F-stat	15.18	20.39	0.61	23.05	24.04	1.43
Observations	106,297	88,699	16,910	106,297	88,699	16,910
Mean of dep. var	4.88	4.26	7.96	4.88	4.26	7.96

Notes: Standard errors are clustered at the district-by-birth cohort level and reported in parentheses. Individual controls include religion, ethnicity, and marital status. Each specification controls for district fixed effects, district specific time trends, and year of survey fixed effects. * p<0.1, ** p<0.05, *** p<0.01

Table B.4. The Impacts of UPE Policy on Rural Women's Propensity of Completing Primary School and Some Secondary School

	DHS		PHC	
	Primary school completion	Some secondary school completion	Primary school completion	Some secondary school completion
	(1)	(2)	(3)	(4)
Reform 1994	0.040***	0.038***	0.041***	0.030***
	(0.011)	(0.012)	(0.007)	(0.005)
District FE	Yes	Yes	Yes	Yes
District Trends	Yes	Yes	Yes	Yes
First Stage F-stat	9.70	9.02	31.07	32.29
Observations	16,367	16,367	88,518	88,518
R-squared	0.092	0.075	0.064	0.032
Mean of dep.var	0.15	0.08	0.22	0.12

Notes: Standard errors are clustered at the district-by-birth cohort level and reported in parentheses. Individual controls include religion, ethnicity, and marital status. Each specification controls for district fixed effects, district specific time trends, and year of survey fixed effects. * p<0.1, ** p<0.05, *** p<0.01

Table B.5. First Stage Results: The Impact of the UPE policy on Male Schooling Attainment

	DHS	PHC
Reform 1994	0.124	-0.007
	(0.139)	(0.066)
District FE	Yes	Yes
District Trends	Yes	Yes
First Stage F-stat	0.80	0.01
Observations	5,003	90,897
R-squared	0.159	0.161

Notes: Standard errors are clustered at the district-by-birth cohort level and reported in parentheses. Individual controls include religion, ethnicity, and marital status. Each specification controls for district fixed effects, district specific time trends, and year of survey fixed effects. * p<0.1, ** p<0.05, *** p<0.01

Table B.6. The Impacts of Female Education on Fertility Using Data of PHC

	Number of Children Ever Born			Number of Living Children		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	Reduced Form	2SLS	OLS	Reduced Form	2SLS
Education	-0.101***		-0.497***	-0.063***		-0.342***
	(0.002)		(0.120)	(0.001)		(0.097)
Reform 1994		-0.163***			-0.114***	
		(0.029)			(0.026)	
District FE	Yes	Yes	Yes	Yes	Yes	Yes
District Trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	88,699	88,699	88,699	88,699	88,699	88,699
First Stage F-stat			24.04			24.04
Mean of dep. var	4.76	4.76	4.76	4.06	4.06	4.06

Notes: Standard errors are clustered at the district-by-birth cohort level and reported in parentheses. Individual controls include religion, ethnicity, and marital status. Each specification controls for district fixed effects, district specific time trends, and year of survey fixed effects. * p<0.1, ** p<0.05, *** p<0.01

Table B.7. The Impact of Female Education on Fertility Using Various Specifications (PHC)

	Baseline	Include cohorts aged 16	Exclude cohorts aged 15 and 16	Quadratic function	Bandwidth= 8	Bandwidth= 7	Bandwidth= 5	Bandwidth= 4	Bandwidth= 3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Number of children ever born									
Education	-0.497***	-0.407***	-0.591***	-0.341**	-0.389***	-0.365***	-0.569***	-0.378***	-0.360***
	(0.120)	(0.073)	(0.190)	(0.163)	(0.071)	(0.083)	(0.170)	(0.113)	(0.125)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	88,699	91,779	91,038	88,699	116,738	101,420	73,568	56,950	42,486
First Stage F-stat	24.04	53.07	11.67	8.141	58.91	37.40	14.14	17.47	15.30
Panel B: Number of living children									
Education	-0.342***	-0.264***	-0.446***	-0.125	-0.325***	-0.281***	-0.317***	-0.245***	-0.203**
	(0.097)	(0.053)	(0.168)	(0.102)	(0.065)	(0.073)	(0.115)	(0.089)	(0.094)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	88,699	91,779	91,038	88,699	111,425	101,420	73,568	56,950	42,486
First Stage F-stat	24.04	53.07	11.67	8.141	56.15	37.40	14.14	17.47	15.30

Notes: Standard errors are clustered at the district-by-birth cohort level and reported in parentheses. Individual controls include religion, ethnicity, and marital status. Each specification controls for district fixed effects, district specific time trends, and year of survey fixed effects. * p<0.1, ** p<0.05, *** p<0.01

Table B.8. The Impact of The UPE Policy on Fertility Using DID Approach

	Number of children ever born	Number of living children
Rural × Cohort	-0.300***	-0.150**
	(0.067)	(0.063)
Rural	1.070***	0.751***
	(0.058)	(0.055)
Cohort	0.157**	0.046
	(0.077)	(0.070)
Number of Observations	19,325	19,325

Notes: Standard errors are clustered at the district-by-birth cohort level and reported in parentheses. The regression controls for age, religion, ethnicity, marital status and district fixed effects, district specific time trends, and year of survey fixed effects. * p<0.1, ** p<0.05, *** p<0.01

Table B.9. Placebo Tests (PHC)

	2 years back	3 years back	4 years back	2 years forward	3 years forward	4 years forward
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Number of children ever born						
Placebo reform	0.024	0.053	0.059	-0.002	0.017	0.041**
	(0.032)	(0.035)	(0.036)	(0.022)	(0.019)	(0.018)
Observations	77,960	74,063	69,971	94,561	95,687	96,889
Panel B: Number of children living children						
Placebo reform	0.013	0.013	-0.016	-0.029	-0.016	0.002
	(0.027)	(0.030)	(0.029)	(0.020)	(0.018)	(0.017)
Observations	77,960	74,063	69,971	94,561	95,687	96,889

Notes: Standard errors are clustered at the district-by-birth cohort level and reported in parentheses. Individual controls include religion, ethnicity, and marital status. Each specification controls for district fixed effects, district specific time trends, and year of survey fixed effects. * p<0.1, ** p<0.05, *** p<0.01

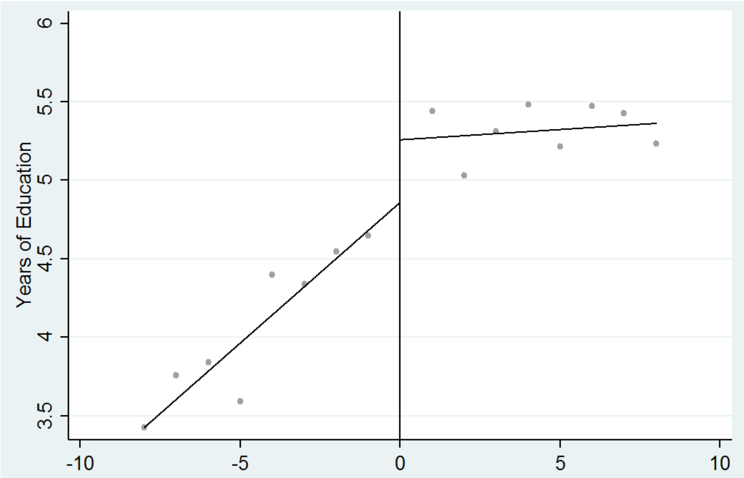

Figure B.1. Female years of education-PHC

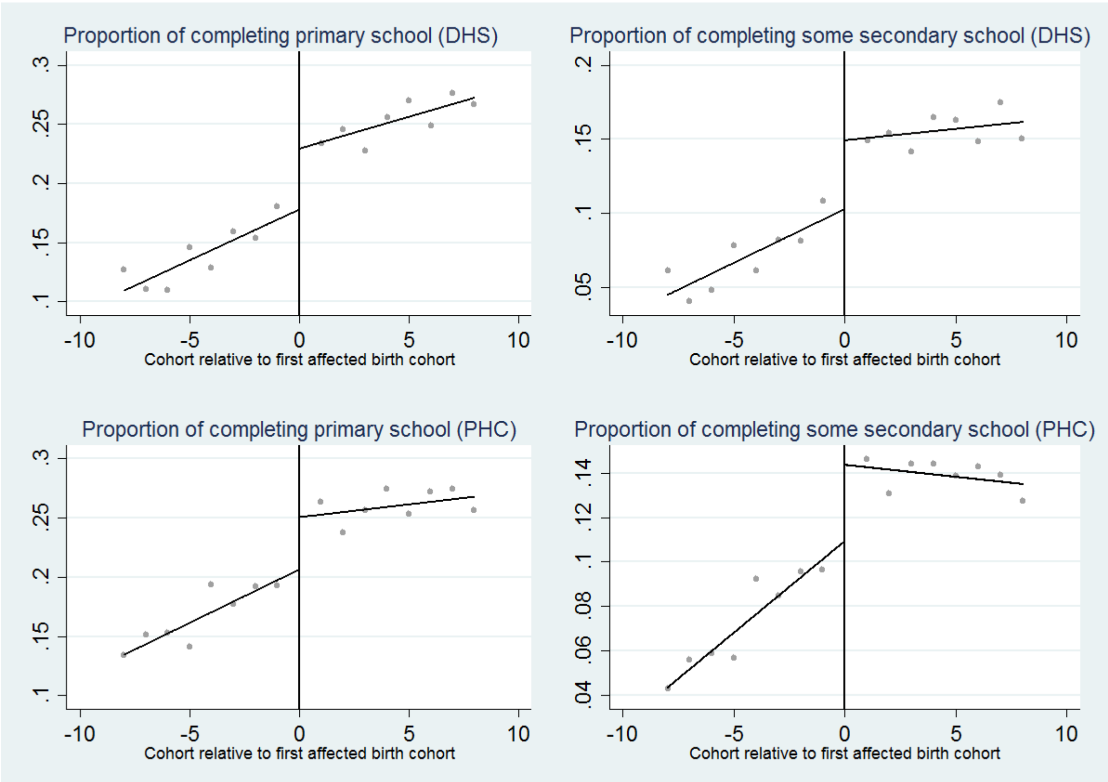

Figure B.2. Proportion of completing primary school and some secondary school

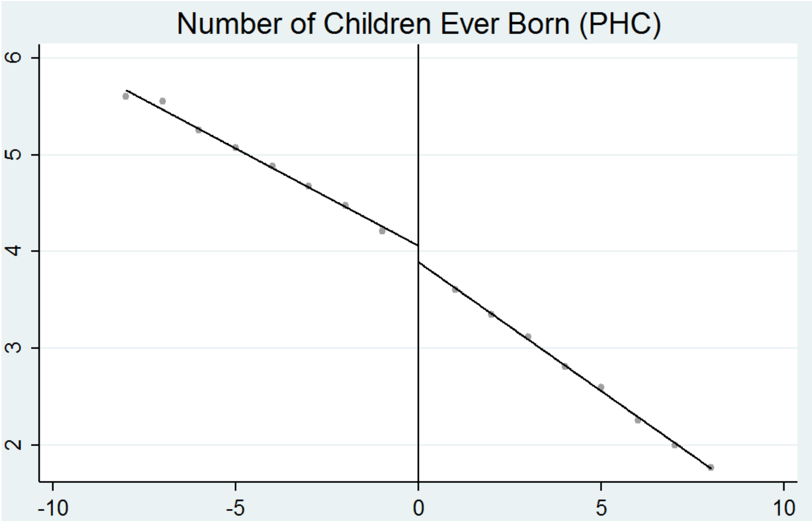

Figure B.3. Number of Children Ever Born (PHC)

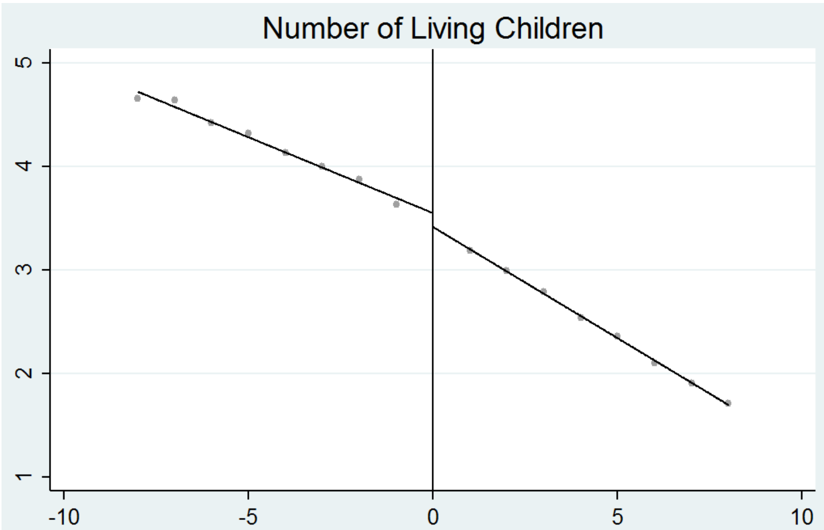

Figure B.4. Number of Living Children (PHC)

APPENDIX C. SUPPLEMENTARY FIGURES AND TABLES FOR CHAPTER 4

Table C.1. Impacts of Female Education on Child Mortality--Robustness Check

	Neonatal mortality		Infant mortality		Under-five mortality	
	2SLS	RF	2SLS	RF	2SLS	RF
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A Bandwidth=28 (Quadratic Polynomial)						
Education	-0.008**		-0.001		-0.000	
	(0.003)		(0.006)		(0.007)	
Policy		-0.008**		-0.001		-0.000
		(0.003)		(0.006)		(0.006)
Observation	46,843	46,843	46,843	46,843	46,843	46,843
F-test 1st stage	20		20		20	
Panel B Bandwidth=52 (Quadratic Polynomial)						
Education	-0.010*		-0.011		-0.008	
	(0.005)		(0.007)		(0.007)	
Policy		-0.006*		-0.006		-0.005
		(0.003)		(0.004)		(0.004)
Observation	83,725	83,725	83,725	83,725	83,725	83,725
F-test 1st stage	12.84		12.84		12.84	
Panel C Bandwidth=29 (Linear Polynomial)						
Education	-0.009**		-0.008		-0.004	
	(0.004)		(0.005)		(0.006)	
Policy		-0.005**		-0.004		-0.002
		(0.002)		(0.003)		(0.003)
Observation	48,826	48,826	48,826	48,826	48,826	48,826
F-test 1st stage	14.93		14.93		14.93	
Panel D Bandwidth=60 (Cubic Polynomial)						
Education	-0.007*		-0.009*		-0.008	
	(0.004)		(0.005)		(0.005)	
Policy		-0.007*		-0.009*		-0.008
		(0.004)		(0.005)		(0.005)
Observation	96,986	96,986	96,986	96,986	96,986	96,986
F-test 1st stage	23.21		23.21		23.21	

Notes: Standard errors are clustered at the month-year of birth level and reported in parenthesis. Controls in each specification include child birth order, child gender, region fixed effects, birth month fixed effects, year of survey fixed effects.
* $p<0.1$, ** $p<0.05$, *** $p<0.01$

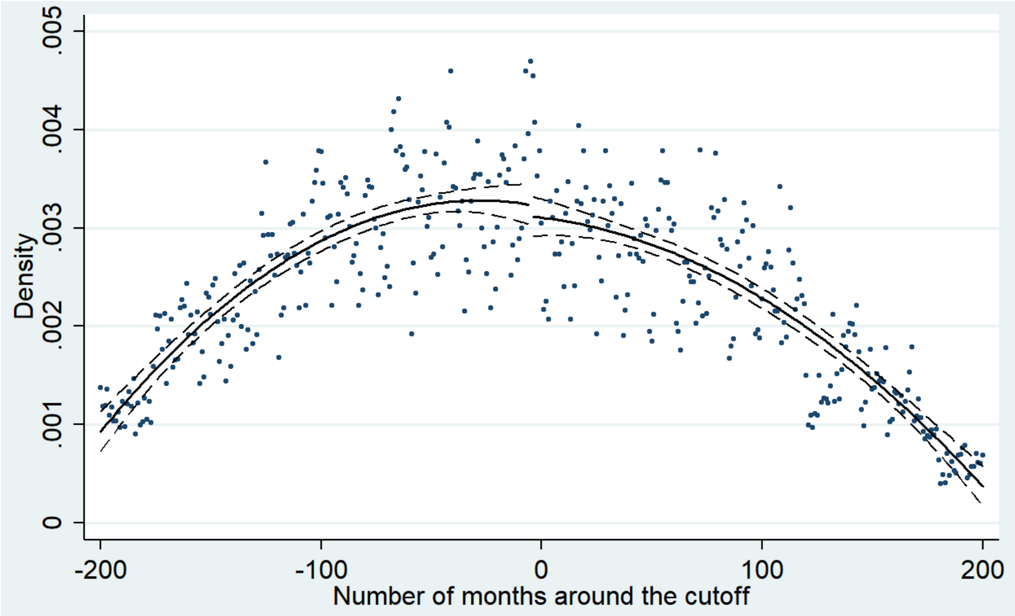

Figure C.1. RD Verification Test

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