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Women's education, marriage, and fertility outcomes: Evidence from Thailand's compulsory schooling law

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ABSTRACT

Increased education affects market and non-market outcomes. This paper investigates the causal impact of the extension of compulsory education from 6 to 9 years on females' education, marriage, and fertility outcomes in Thailand. Using data from the Multiple Indicator Cluster Survey (MICS) and a donut-hole Regression Discontinuity (RD) design, we show that the new law increases lower secondary school completion in girls, leading to decreased probabilities of giving birth in the school-age years (14–17 years). The policy primarily affects the marginal child leading to the postponement of the timing of their fertility to after-school years. We also document heterogeneity and show that the fertility effects are stronger for Muslim women. The policy leads to a consistent drop in the probability of marriage and cumulative births for Muslim women, which sustain beyond the completion of schooling years. The results hold with alternative empirical model specifications and falsification tests.

1. Introduction

Education, especially female education, has been regarded as the key to positive ripple effects on economic and social outcomes. Education has strong effects on economic growth and income inequality (Mankiw et al., 1992), worker productivity and earnings in the labor market (Card, 1999), and spillover effects on another workers' productivity (Acemoglu & Angrist, 2000; Iranzo & Peri, 2009). Education affects non-market outcomes like individual's health, fertility, children's long-term economic outcomes, and child quality among other outcomes (Grossman, 2006). Given the widespread impact that education has on the economy and individual well-being, many developing countries have invested in school construction programs, conditional cash transfers, and compulsory schooling laws to give their population greater access and push to achieve more education (Burde & Linden, 2013; Cui et al., 2019; Schultz, 2004).

Female education, especially, is associated with lower fertility and lower child mortality, increasing women's labor supply, increasing bargaining power in the households, changing marriage market returns, and increases in children's schooling (Attanasio & Kaufmann, 2017; Black et al., 2008; Keats, 2018; Lam & Duryea, 1999; McCrary & Royer, 2011; Osili & Long, 2008). This paper contributes to this vast literature and studies the causal impact of a compulsory schooling law on female education and the corresponding changes in marriage and fertility outcomes in Thailand. Since the relationship between schooling, fertility, and marriage varies by women's educational level and stages of country's development, the question warrants an empirical investigation in different developing country settings (Kim, 2016).

In Thailand, the Compulsory Education Act B.E. 2545 (2002) was announced on December 31, 2002 and increased the compulsory schooling until the children were 16 years of age or would have completed 9 years of education vis-à-vis the 6 years of compulsory primary education that previously existed. We use the timing of the policy implementation and the date of birth of children to determine the affected cohorts by the policy and the consequences of increasing schooling for the affected cohorts. Using the 14-province dataset in the Thai 2015-2016 Multiple Indicator Cluster Survey (MICS) and a donut-hole Regression Discontinuity (RD) design, we show that the law increases female secondary education completion between 6.9 and 14.9 percentage points while having no such impact on male secondary education. For females, these effects persist beyond the lower secondary school. Looking at the cumulative effect on females completing at least

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grade 9, we see that the policy is able to push females to complete at least lower secondary school by between 7.5 and 8.9 percentage points. We also find evidence of increased vocational education for females in higher secondary, leading us to believe that the policy was successful in retaining the marginal female student to complete her schooling. This is in line with similar outcomes observed as a result of the compulsory schooling reform in China. Fang, Eggleston, Rizzo, Rozelle, & Zeckhauser (2012) find that the compulsory schooling reform in China had much stronger effects on young women than men, as young women were more likely to be on the margin of being affected.

We focus on the consequences of the changing compulsory schooling law on marriage and fertility outcomes. Silles (2011) shows that compulsory schooling laws in Great Britain and Northern Ireland reduced the incidence of teenage childbearing. Similarly, Black et al. (2008) show that educational reforms have spillover effects in decreasing teenage fertility in the US and Norway. Kan and Lee (2018) use Taiwan's 1968 compulsory education law to find no effect on fertility for women using the exact date of birth exposure. In a developing country context, Osili and Long (2008) find that increasing female education by one year reduces early fertility by 0.26 births in Nigeria. Female education may also delay marriages in some cases. Keats (2018) finds evidence of marriage delay due to free primary education in Uganda while Erten and Keskin ((2018)) find no evidence of the impact of schooling reform on marriage in Turkey. Ali and Gurmu (2018) document a significant decrease in the number of children ever born to women in Egypt with the reduction in fertility coming from delaying maternal age rather than changing women's fertility preferences. The impact of education on marriage and fertility can materialize through changing labor market returns, improving marriage market returns due to positive assortative mating, impacting opportunity costs of having children, changing health behaviors, changing cultural norms, and changing bargaining power within the household to affect women's empowerment. The evidence also points towards the possibility of no change in total fertility if women just change the timing of birth from earlier to later years. Given the varied effects and pathways in changing marriage and fertility across different countries, we analyze the impact of the compulsory schooling law on these outcomes in Thailand.

Using interview data on women aged 15 to 49 years from the fourteen province 2015-16 Multiple Indicator Cluster Survey (MICS), we construct a women-age panel format using information on marriage and birth timings for the 14 provinces in Thailand. This panel data format allows us to compare marriage and fertility outcomes between the treated and control groups at each particular age up to 20 years old. We employ a donut-hole Regression Discontinuity (RD) design in which individuals born within 12 months of the birth threshold are excluded to derive a causal impact of the policy. Our study finds a significant impact of the compulsory schooling law on decreasing the probability of females ever giving birth in Thailand in the school-age years (14–17 years). If higher-educated women delay marriage or cohabitation due to opportunities in the labor market, then education can reduce their fertility by effectively lowering their reproductive lifespan. Analyzing the fertility patterns, we find that girls delay their fertility to after school years. Correspondingly, we also document an increase in the probability of marriage after the age of 17 years. Therefore, the policy is effective in delaying fertility for women from school to after-school years and displays a direct effect of school attendance, rather than changing fertility preferences. These effects are consistent with what is found in terms of educational impact on early fertility (Duflo et al., 2015; Keats, 2018) and postponement of fertility to later years in many developing countries (Breierova & Duflo, 2004; Kırdar et al., 2018; Osili & Long, 2008). We also notice an increase in vocational education for females after the enactment of compulsory schooling. Given these findings, this policy primarily affects the education of young women at risk of dropping out of school similar to the findings for school entry policies within the US (McCrary & Royer, 2011).

There is heterogeneity in effect of the compulsory education law in Thailand. It affects the educational attainment of young women in rural areas more than in the urban areas. This is anticipated as rural women may have lower levels of education to begin with and consistent with effects seen in other countries like Turkey and China (Erten & Keskin, 2018; Fang et al., 2012). However, these increases in education do not translate into greater decreases in fertility and marriage for rural women vis-à-vis urban women. Pattaravanich et al. (2005) document the rural-urban differences as well as Muslims and non-Muslims in Thailand, and even though the differences have narrowed over time, rural population and Muslims continue to be disadvantaged, especially in the South. Along these lines, this paper also finds evidence of differential impact by religion. The policy leads to a consistent drop in the probability of marriage and cumulative births for Muslim women, which sustain beyond the completion of schooling years. The cumulative births for Muslim women decrease by about 6 percentage points, which sustains until the age of 17 years, and we do not find an increase in births for Muslim women after school-years. Therefore, we observe a more prolonged effect of the increase in education for Muslim girls, leading to changing preferences regarding marriage and fertility over their lifespan, consistent with the female empowerment effects of increased education for girls (Doepke & Tertilt, 2018).

Heath and Jayachandran (2017) argue that the effects over the life course, such as total births, or marital outcomes that depend on general equilibrium adjustments, including assortative mating and marriage markets, are less clearly established and are context-dependent. Using an exogenous variation in education, we are able to develop a causal estimate of the compulsory schooling law on marriage and fertility outcomes in Thailand and add to the rich literature on the impact of compulsory schooling laws on human capital formation and the impact on marriage and fertility. In the context of Thailand, Paweenawat and Liao (2019) use the 1977 Thai schooling education reform that impacted primary education and pseudo panel approaches to study the effect of education on marriage market and fertility. To our knowledge, ours is the first study to use the 2003 extension of compulsory schooling law affecting the lower secondary education to study the causal impact of compulsory schooling on women outcomes in these 14 provinces of Thailand. This direct impact of the reform on secondary students brings a contemporaneous change in teen fertility, distinct from the earlier reform that only affected primary education. Moreover, our dataset allows us to create a woman-age panel to explore fertility dynamics and we document the impact on fertility timing of women affected by the new education law. This dynamic has not been analyzed in the context of Thailand's compulsory schooling law.[1]

2. Background of the Thai education system and compulsory education laws

The current education system in Thailand consists of 3 years of pre-primary education, 6 years of primary education (Grades 1–6), 3 years of lower secondary education (Grades 7–9), 3 years of higher secondary education (Grades 10–12), and at least 2–4 years of tertiary education.[2] Most children start pre-primary education when they are around 3 years old and complete higher secondary education or high school when they are around 18 years old. Around 82% of students were in public schools

[1] It should be noted that the results are representative at the province level but not of Thai population in general as the survey data covers only the 14 vulnerable provinces and not all the 76 provinces in Thailand.

[2] The tertiary degrees range from 2-year post-secondary or college diplomas, bachelor's degrees, master's degrees, and Ph.D. degrees. The 2-year diplomas are mostly vocational degrees and diplomas for students who fail to complete all requirements of the bachelor's degrees. The bachelor's degrees in most disciplines take four years, with exceptions of 5-to-6-year programs in medicine, pharmacy, dentistry, and veterinary. Most Master's degrees take 1 to 2 years, while Ph.D. programs take at least 5 years.

in 2006 (Brief Education Statistics 2006, Ministry of Education). Most public schools, especially those in provincial areas, were traditionally divided into elementary and secondary schools. Elementary schools have been available in villages for decades, but secondary schools were mostly in district municipal areas up until the recent educational reform in the early 2000′s (Thongthew, 1999). Currently, lower secondary education is available, in most cases, at a subdistrict level, and higher secondary education is available in district municipal areas.[3] Vocational track is also available starting in higher secondary education with a 2-year college diploma as the highest vocational degree. Vocational schools are scattered throughout the country, but most of them are located in large towns and cities. Students from rural areas usually have to move into towns and cities to continue their study.

The first compulsory education law in Thailand was enacted in 1921 (Sangnapaboworn, 2007). It required children aged 7 years and above to attend school until they turn 14 years of age or have completed 3 years of primary education. The compulsory education was then increased to 4 and 7 years in 1936 and 1960, respectively. However, the compulsory education law was not heavily enforced during this period due to both the low demand and the low supply of schooling, especially in rural areas. The major education reform in 1977 then restructured the Thai education system to the current system and changed the compulsory schooling to 6 years of primary education. This reform also brought about a strict enforcement of the compulsory schooling law and resulted in a significant increase in the number of students in Grades 5-6 (Chankrajang & Muttarak, 2017).

The most recent change in compulsory education extended the compulsory schooling to 9 years, or lower secondary education, starting in 2003. The Compulsory Education Act B.E. 2545 (2002), announced on December 31, 2002, states that children who are turning seven years of age must enroll in school and must stay in school until they are turning 16 years of age or have completed 9 years of education [excluding the pre-primary education] (Royal Thai Government Gazette, 119 (128A), 2002).

These recent major education reforms in Thailand occurred as a result of political movements. The reform in 1977 took place as democracy was restored after decades of military dictatorship between 1948 and 1973. The Compulsory Education Act of 2002 was a part of several other reform measures following the enactment of the 1997 Constitution of the Kingdom of Thailand (Sangnapaboworn, 2007), the constitution that was drafted during another democracy restoration after the abolishment of another military regime in 1992.

In this paper, we focus on the 2003 extension of compulsory schooling to 9 years or lower secondary education. Following the announcement of the Compulsory Education Act of 2002, the enrollment in lower secondary education steadily increased in 2003-2004 (see Appendix Fig. A1). Prior to the policy, the gap between Grade 6 and 7 enrollments averaged to 11.8 percentage points between 1999 and 2002, and this gap closed to around 3 percentage points in 2004–5. By 2005, all of Grade-7-age population were in the formal education system. Although the policy was successful in increasing initial enrollment into lower secondary schools (Grade 7), the policy was not as effective at keeping students in the system until Grade 9. By 2006, only 92.1% of Grade-9-age population were enrolled in the formal system, a significant drop from the cohort's Grade 7 enrollment rate of 99.5% in 2004. This is due, at least partly, to the lack of a mechanism that effectively monitors completion of lower secondary education.[4] Another set of statistics shows that attrition starts in secondary education and is more prominent in boys than in girls. As shown in Fig. A2, enrollment in elementary education during 1998–2012 was close to 100% of the school-age population, with boys enrolling more than girls by 1 to 3 percentage points. Enrollment in secondary education ranged from 62 to 88% during the same period, and the enrollment in girls was 4–6 percentage points higher than that of boys' by starting from 2004 onwards. This gender difference in enrollment widened in tertiary education to as large as over 13 percentage points.

As shown in Fig. A1, significant increases in enrollment were seen starting with the sixth graders in the 2002 academic year who had to continue their study into lower secondary education instead of finishing schooling at the primary level.[5] This discontinuity allows for a regression discontinuity (RD) design using the date of birth as a threshold. Most children start Grade 1 if they turn 6 years of age before an academic year starts on May 17, so the threshold birthdate for our RD design is May 17, 1990. We will discuss our RD design, along with other empirical details, in the next sections.

3. Data

We use data from the 14-province dataset in the Thai 2015-2016 Multiple Indicator Cluster Survey (MICS). The survey covers various aspects of children and women in Thailand, from nutrition to early childhood development, education, and reproductive health, among others. The survey's geographical coverage was designed from an equity perspective. Five out of the 14 provinces are in the Deep South of Thailand, an area with longstanding political unrests that result in limited and diminishing access to education, healthcare, and social welfare. The rest of the provinces were selected based on poverty rates. The sample in this survey is representative of the population at the province level. Though this dataset might not be representative of the Thai population, it reflects the situation prominent in the most vulnerable groups of the Thai population. The survey was conducted between November 2015 and May 2016 and covers 17,210 households.

Our primary focus is on the women module in which 15,607 women aged between 15 and 49 were interviewed. We focus on education, marital, and fertility outcomes among women who were born within 60 months of May 1990. These women aged between 20 and 30 years old at the time of the survey.

As shown in Table A1, 55.7% of the women in our sample live in non-municipal areas. 61.5% of their households are led by male. The household heads, on average, are 48.3 years old and have 6.7 years of education. Around 70% of the women live in households with Thai household heads, and about 33% of them live in Muslim-led households. Compared to the Thai population, our sample contains more Muslims and non-Thais because five out of the 14 provinces surveyed are in the Deep South, an area dominated by Malay-descendent Muslims. A major drawback of this dataset is the lack of childhood demographic covariates. For this reason, we are not able to test for balance in demographic or socioeconomic background across the two sides of the RD threshold.

The women in our sample, on average, have 11.4 years of education. 83.3% complete lower secondary school or higher. 21.6% complete lower secondary education, while 35.4% finish higher secondary school,

[3] Thailand is administratively divided into provinces (*changwats*). Each province is divided into districts (*amphoes*) and each district is then divided into subdistricts (*tambons*). Subdistricts, on average, consist of 10 villages, each with over 100 households. Most subdistricts have at least one school that offers lower-secondary education. Some large subdistricts also have secondary schools which provide both lower- and higher-secondary education.

[4] According to the implementation guideline of the Compulsory Education Act of 2002, admissions to Grades 1 and 7 were cross-checked with rosters of 7-year-old population from house registration and Grade 6 students in the Ministry of Education system, respectively. There was no formal cross-checking for Grade 9 completion in the guideline.

[5] The academic year in Thailand at that time ran from mid-May to late February or early March the following calendar year. The policy was announced in December 2002, so its full effect was on the sixth graders in the 2002 academic year.

and 21.1% have college degrees. The younger cohorts in this sample, age-wise, should have finished high school and be in college at the time of the survey provided that they started and completed elementary and secondary education on time. When compared with the other women in the survey, this sample is relatively young and have more education.

The questionnaire categorizes marital status into three groups: not living with anyone, living with a partner, and legally married. We consider a woman to be married if she lives with or legally marries her partner because a significant number of marriages in Thailand are not lawfully registered Thai Risk Family Survey BE 2550 (2007).[6] Using this definition of marriage, 75% of the women in our sample have ever married, and 69% are currently married. Conditioning on ever married, the average age when the women first married or started cohabitation is 19.6 years old. 68.2% of the sample has ever given birth, and the average age when they gave the first birth is 21.2 years. Most of these women gave the first birth when they were at least 19 years old and should have completed high school.

One problem with the original data format is truncation due to age—we do not observe women beyond their ages at the survey time. With the youngest cohort around 20 years old and the eldest around 30 years old, this truncation makes comparisons of marital and fertility outcomes between the treated and control groups difficult as we would be comparing younger women with shorter time for marriage and fertility to the older women. To mitigate this concern, we convert the original data into a women-age panel format using information on marriage and birth timings.[7] This panel data format allows us to compare marriage and fertility outcomes between the treated and control groups at each particular age up to 20 years old. We were able to construct a complete birth history for 94% of the women in our sample, and our main results are robust to whether the women with incomplete birth history are excluded from our estimation.

4. Methodology

We use a donut-hole regression discontinuity (RD) design (Almond & Doyle, 2011; Barreca et al., 2011; Card & Giuliano, 2014; Fukushima et al., 2016; Kırdar et al., 2018) to estimate the effects of the increase in compulsory schooling on education, marital, and fertility outcomes. Following various works on changes in compulsory education, we exploit the discontinuity in the birth date (Ali & Gurmu, 2018; Black et al., 2008; Erten & Keskin, 2018). Specifically, individuals born after May 17, 1990 were in Grade 6 or below when the policy was announced, and they had to continue onto lower secondary schools to finish Grade 9 instead of finishing at Grade 6.

Our RD design is not sharp due to at least four reasons. First, failing/marginal students who were in Grades 7–9 at the time of the policy announcement were also affected by the policy because they then had to finish Grade 9 rather than dropping out.[8] Second, while most elementary schools in Thailand follow the May 17 birthdate cutoff for admission, a small number of schools, mostly private, establish their own admission guidelines and could select other cutoff dates. Third, some parents decide to send their children to school sooner or delay schooling. Finally, the implementation and the enforcement of this policy were not perfect. The education statistics from the Ministry of Education indicate that only 78–92% of Grade-9-aged children were enrolled in school between 2002 and 2006, as shown in Appendix Fig. A1 (Ministry of Education, 2023) .

A common approach if we were to follow a fuzzy RD design is to use the change in the compulsory schooling law as an instrumental variable (IV) for women's education. One of the key assumptions behind this IV framework is the exclusion restriction assumption—the increase in compulsory schooling affects our outcomes only through the increased women's education. This assumption is unlikely to hold because the policy can also affect marriage and fertility through general equilibrium effects of expectations of increases in men's education[9] and therefore, affecting the marriage markets (Attanasio & Kaufmann, 2017). For these reasons, we follow Kırdar et al. (2018) and employ a donut-hole RD design in which individuals born within 12 months of the birth threshold are excluded.

Let Y_i be the outcome of interest, then our main estimating equation can be written as

$$Y_i = \alpha + f_0(x_i^{'})(1 - T_i) + f_1(x_i^{'})T_i + \beta T_i + \epsilon_i, \tag{1}$$

$$x_i^{'} = x_i - x_0,$$

$$\forall x_i^{'} \in (-w, \ -d) \cup (d, \ w)$$

where T_i is our treatment dummy variable that is equal to one for all women born after the May 1990 birth month threshold, x_0. Our running variable, $x_i^{'}$, is the difference between each woman's month of birth, x_i, and x_0.[10] $f_0(\cdot)$ and $f_1(\cdot)$ are time trends before and after the treatment, respectively. We present the linear, quadratic, and cubic functional form results of the main model. In our main specifications, we choose 60 months around the threshold as an estimating window, w, and 12 months on each side of the threshold as the donut hole exclusion, d.[11] Province fixed effects are included in all estimations to account for possible time-invariant province-specific unobservables such as province locations. Age fixed effects are also included in the models that utilize the women-age panel data. For all fertility and marital outcomes, our standard errors are clustered at the month-year of birth level following our treatment definition and to be consistent with the literature (Erten & Keskin, 2018).[12] It should be noted that since we are using the reduced form equation for estimation, we are effectively estimating the effects of the policy rather than an increase in education per se. Moreover, as we evaluate the effects of the policy on a large number of outcomes, we also adjust standard errors for multiple hypothesis testing following Simes (1986). Thus, for each outcome variable in our main specifications, we report results based on both standard p-values and p-values adjusted for multiple-hypotheses testing.

The key identification assumption for our RD design is that variations in the birth months in a small window around our cutoff must be exogenous. That is, individuals born just before and just after May 1990 are systematically similar except for whether they were affected by the increase in compulsory education. This assumption is likely to hold

[6] "Thai Risk Family Survey BE 2550 (2007)" http://www.socialwarning.m-society.go.th/risk/3.html

[7] As an alternate specification and to more fully utilize the data, we also estimate a hazard model for women outcomes and show the results for birth and marriage outcomes in Appendix Fig. A8.

[8] Note, however, that larger effects of the law are expected from the Grade 6 and below cohorts because the transition from elementary to secondary education had been a major culprit of dropping out from the education system. For this reason, our RD threshold is specified at the Grade 6 cohort, who were at the margin of a major attrition. We acknowledge that the Grade 7-9 cohorts in our control group could also be affected by the law, and hence our treatment effect estimates might be attenuated downward.

[9] We also note here that we find some evidence for increased men's education in some subsamples. We find some countervailing significant effects on rural vs. urban men.

[10] The MICS dataset contains only the month and year of birth, so we cannot define our running variable based on the date of birth. This drawback is inconsequential in our donut-hole RD design because the individuals born immediately around the birthdate threshold are essentially excluded.

[11] Our results hold for different bandwidths and windows of estimation. The choice of this particular bandwidth as our main specification is explained further in this section.

[12] We also present results using standard errors clustered at the school cohort level as a robustness check.

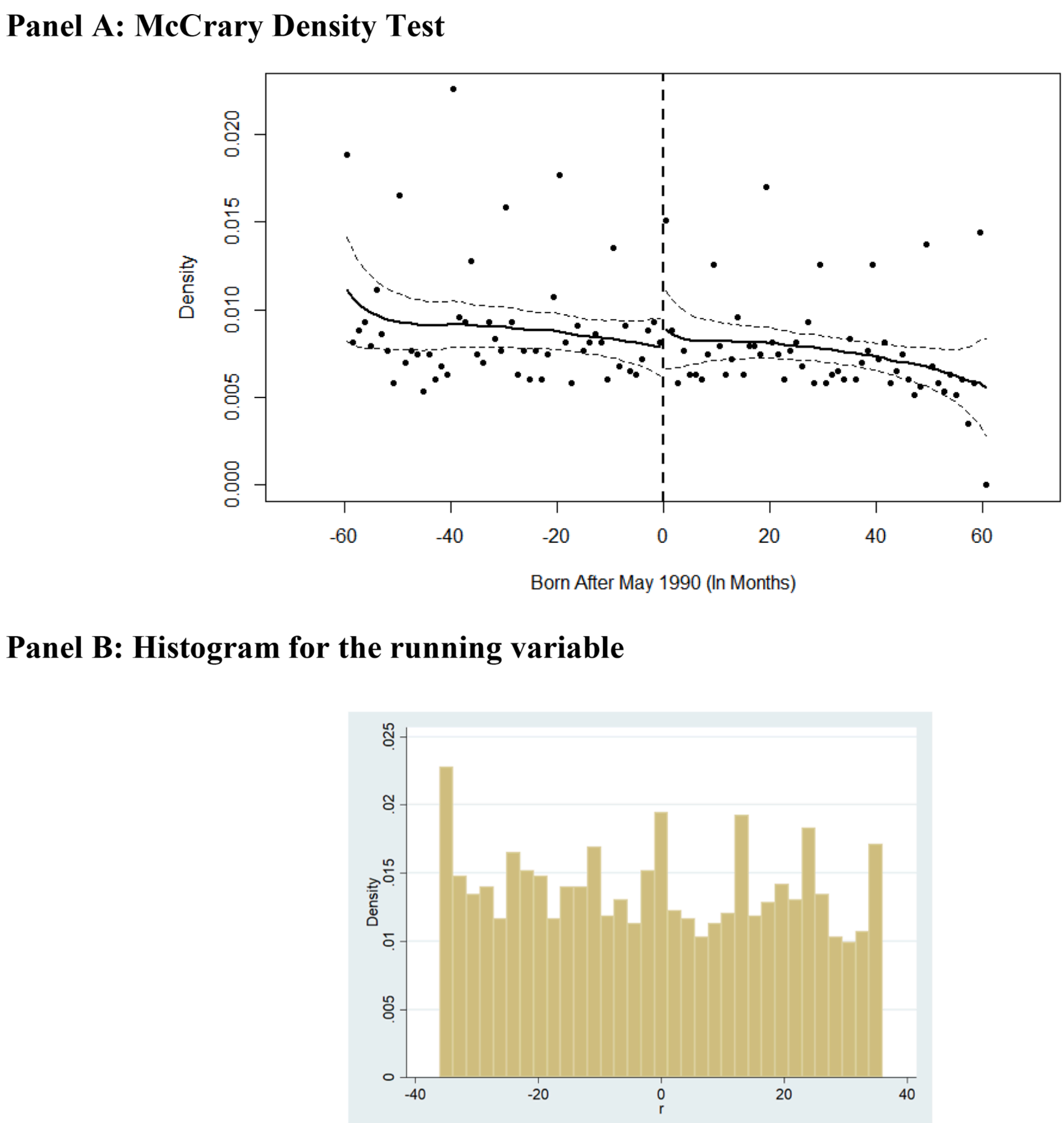

Fig. 1. Panel A: McCrary Density Test, Panel B: Histogram for the running variable.
Note: The sample is women who were born within 60 months of May 1990. The graph illustrates no discontinuity in the density of the running variable, the month of birth, around the threshold. Note: The graph depicts nonrandom heaping of the running variable inside the donut hole, necessitating the use of donut hole regression discontinuity design.

because parents could not have manipulated birthdates of their children in anticipation of this policy. We perform the McCrary Density Test and show an insignificant estimate in panel A of Fig. 1 (McCrary, 2008). However, there is some heaping in the birthdates around the threshold (panel B of Fig. 1), which encourages us to use the donut hole exclusion. We also note here that we cannot cleanly test for balance of control covariates, such as socioeconomic background at birth, around the discontinuity because all demographic information in the survey is contemporaneous and hence endogenous to the policy. However, for completeness, we show the balance of the control covariates despite the endogeneity concerns and find no statistically significant effects of the policy on these variables in Appendix Table A2 and Fig. A3.

Another important assumption in the RD design is that the relationship between the outcome variable and the running variable is continuous everywhere except at the cutoff. We verify this assumption by imposing various placebo cutoffs and show that there are no discontinuities around these placebo cutoffs (see Fig. 2).

Fig. 3 presents the effects of the change in compulsory schooling law on education using the described donut-hole RD design. Panel A illustrates a small jump in fraction of women with lower secondary education as their highest degree, while panel B exhibits a clear jump in fraction of women completed at least Grade 9. This could be because (1) the accumulative effect is more apparent, and (2) the policy might have incentivized many non-marginal students to increase their education to stay competitive in the labor market. We will discuss these effects along with the effects of the policy on marriage and fertility outcomes in detail in the next sections.

In our main specifications, we choose 60 months around the

[13] Results for other placebo cutoffs are available upon request.

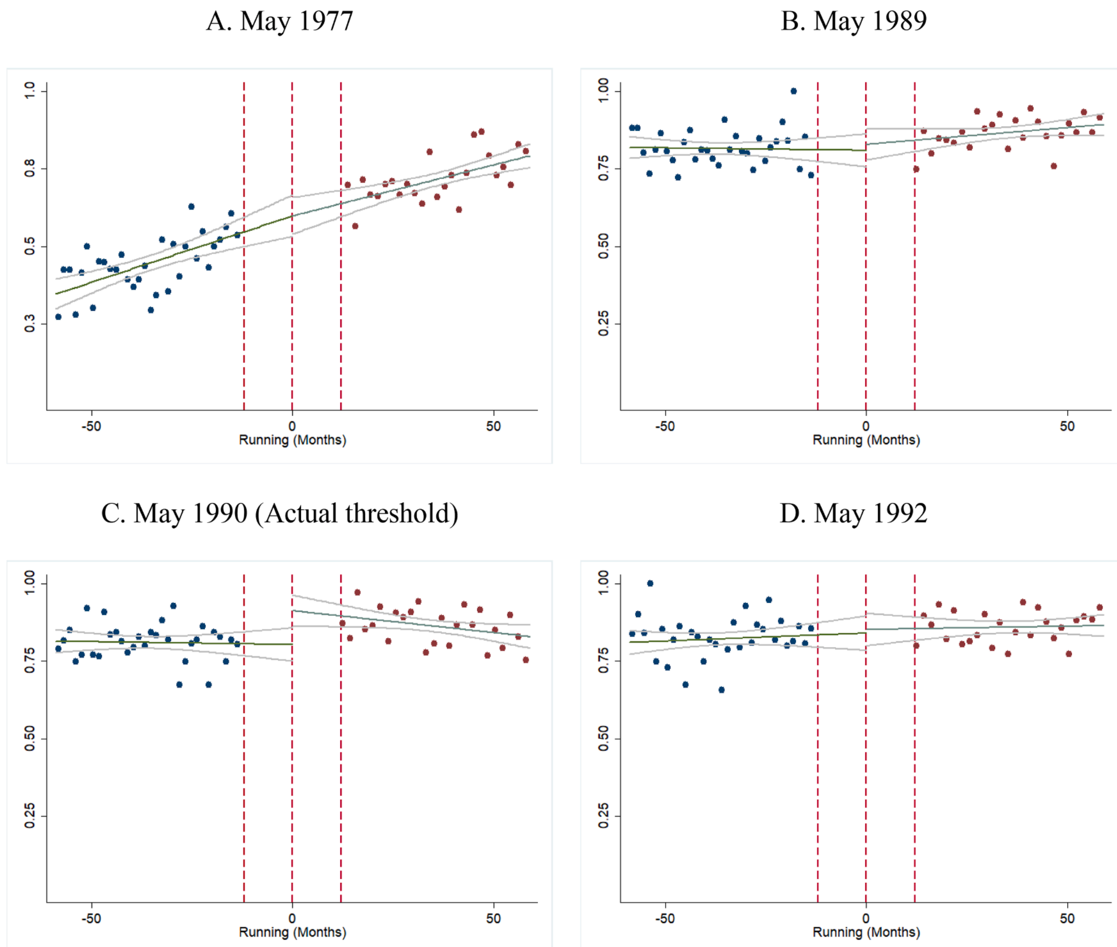

Fig. 2. Discontinuities in fraction of women who completed at least lower secondary education.[13]
Note: The sample in each panel includes women who were born within 60 months of each cutoff, excluding those born within 12 months of the cutoff. The running variable is the month of birth. May 1990 is the actual threshold while May 1977, May1989, and May 1992 are placebo cutoffs.

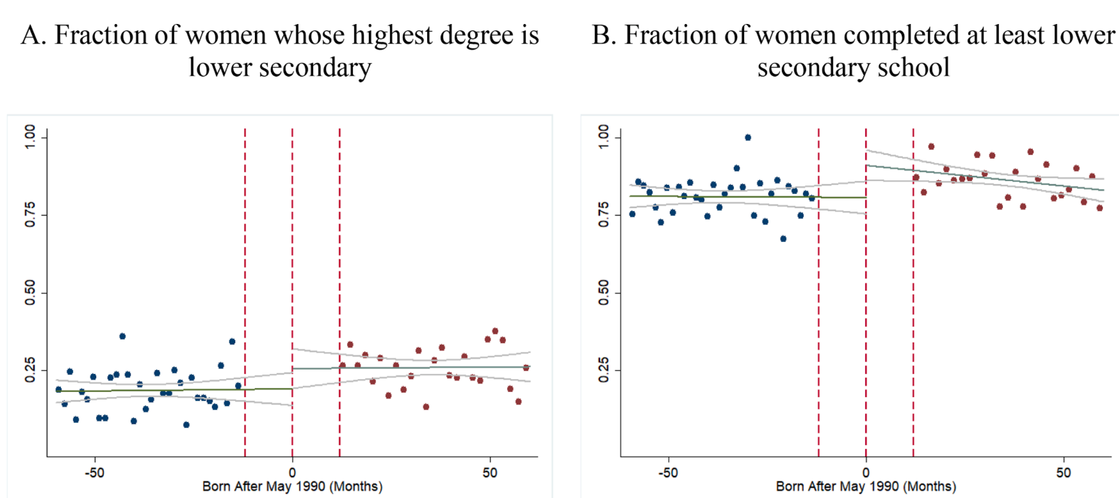

Fig. 3. Effects of the increased compulsory education on women's education.
Note: The sample in each panel includes women who were born within 60 months of the birth cutoff, excluding those born within 12 months of the cutoff. The running variable is the month of birth.

threshold as an estimating window, *w*, and 12 months on each side of the threshold as the donut hole exclusion, *d*, due to technical reasons. The 60-month window allows us to fully observe higher-secondary education outcomes as the women in this window should have already finished this education level at the time of the survey. Specifically, these women aged between 20 and 30 years old; most students should have completed their secondary education at 18–19 years old. In addition, this window also corresponds to the legal age for marriage in the Thai

Table 1
Impact on Schooling.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Females				Males			
Dependent Variable	Lower Secondary Education	Completing at least Grade 9	Vocational Education in High School	Academic Track in High School	Lower Secondary Education	Completing at least Grade 9	Vocational Education in High School	Academic Track in High School
Linear	0.069**	0.089***	0.019**	-0.0131	0.022	-0.024	-0.017	-0.011
	(0.015)	(0.000)	(0.043)	(0.679)	(0.195)	(0.346)	(0.107)	(0.788)
	[0.058]	[0.000]	[0.058]	[0.679]	[0.39]	[0.461]	[0.39]	[0.788]
Quadratic	0.069*	0.075***	0.043**	-0.106**	0.045	0.029	-0.006	0.0032
	(0.061)	(0.005)	(0.014)	(0.012)	(0.154)	(0.157)	(0.615)	(0.956)
	[0.061]	[0.018]	[0.018]	[0.018]	[0.314]	[0.314]	[0.819]	[0.956]
Cubic	0.149***	0.076***	0.039***	-0.0571	0.046	0.054*	-0.028**	0.121***
	(0.007)	(0.005)	(0.003)	(0.269)	(0.331)	(0.094)	(0.024)	(0.003)
	[0.009]	[0.009]	[0.009]	[0.269]	[0.331]	[0.126]	[0.048]	[0.011]
Province FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	2942	3010	2942	2942	2527	2565	2527	2527

Note: p-values in parentheses. Simes (1986) corrected p-values adjusted for multiple-hypotheses testing are also noted below each coefficient. Standard errors are clustered at the cohort level.
*** Significant at 1% level,
** significant at 5% level,.
* significant at 10% level.

law, which requires a parental consent for a marriage of individuals younger than 20 years old.[14] Note however that this age range does not allow us to observe the women's total fertility or eventual marital outcomes. As a robustness check, we also use 48- and 72-month estimating windows. The 72-month window covers women aged 19–31 years old; the 48-month window covers women aged 21–29 years old.

The 12-month choice of the donut hole size stems from the possible different ages and birthday cutoffs when women enrolled into Grade 1. First, the law effective at the time that the affected cohort enrolled in Grade 1 specified eight years old as the school age for primary education, but seven years was widely used in practice.[15] Second, parents may also send their children to school sooner than the school age. In this case, children born in late May, for example, could enter Grade 1 roughly one year sooner than when they were supposed to. In addition, the actual birthday cutoffs might not be May 17 for non-public schools. Note that we also use the 6- and 24-month donut holes on each side of the threshold and eliminate the donut hole (only excluding the month of May) as another robustness check.

While our choices of polynomials and bandwidths are chosen from a theoretical and technical perspective, they are consistent with the data-driven choices derived from Placebo Zone Selection Models (PZMS) based on Kettlewell and Siminski (2022). This framework essentially uses observations far away from the RD threshold (the placebo zone) to estimate the choices of bandwidth, polynomial, and other parameters that minimize the root mean squared error (RMSE). Specifically, candidate models with different parameter combinations are tested repeatedly in the placebo zone where a pseudo-treatment effect is known to be zero. The selected model is then the one that yields the lowest RMSE across all pseudo-treatments. We use the PZMS framework than those provided in Imbens and Kalyanaraman (2012) (IK) and Calonico et al. (2014) (CCT) as it allows all the model parameters to be optimized all at once. The simulation results in Kettlewell and Siminski (2022) also show lower RMSE than those based on IK and CCT. However, we also provide IK and CCT results for completeness.[16]

The key assumption behind the PZMS framework is that the RD threshold is chosen randomly in the domain of the running variable. This should hold in our case as the new law was a result of a political reform. The timing of this reform can be thought of as random with respect to the fertility decisions made at least a decade prior. Given the applicability of the model to our setting, we test the robustness of our results using the PZMS framework. In our sample, the PZMS framework chooses bandwidths from 45 to 96 months, depending on the dependent variables. The framework always picks the linear functional form over the quadratic and cubic polynomials as the best fit. We show in Table 3 and Fig. 6 that the results using the PZMS model parameters are similar to those from our main specifications. For completeness and consistency with the literature, we also derive optimal bandwidths using CCT and IK Selection procedures, which yield a more conservative optimal bandwidth between 28 and 53 months but still produce similar findings (in Appendix Table A4 and Appendix Fig. A9).

5. Results

We present econometric evidence using the donut-hole regression discontinuity design on the effects of compulsory education policy on schooling outcomes for male and female children and other outcomes like fertility, age at marriage, and propensity to ever give birth. As shown in Fig. 3 and discussed in the previous section, we observe a jump in female education at the cutoffs. Table 1 presents the estimation results for the policy effect on completion of different levels of schooling for women and men. All estimations control for province fixed effects and use a flexible specification for the running variable. For our main specifications, we also report the p-values corrected for multiple hypothesis testing as recommended in Simes (1986). Table 1 (1) shows that the compulsory schooling law has a statistically significant positive impact on increasing the lower secondary education for females in all specifications. The estimates vary from 6.9 percentage points in linear and quadratic specifications to 14.9 percentage points in the cubic specification. On the other hand, Table 1 (5) shows no statistically significant impact on completion of lower secondary school for male children. For females, these effects persist beyond the lower secondary school. Looking at the cumulative effect on females completing at least

[14] Individuals must be at least 17 years old to register their marriages under the Thai law. Individuals younger than 20 years old, however, need the presence and consent from their parent to register their marriages.

[15] The Elementary Education Act B.E. 2523 (1980) states that children turning eight years old should enroll in Grade 1. That age, however, is seven years old in the Compulsory Education Act B.E. 2545 (2002).

[16] We show robustness of our results to IK/CCT bandwidth selection procedures in Appendix Table A4 and Appendix Fig. A9.

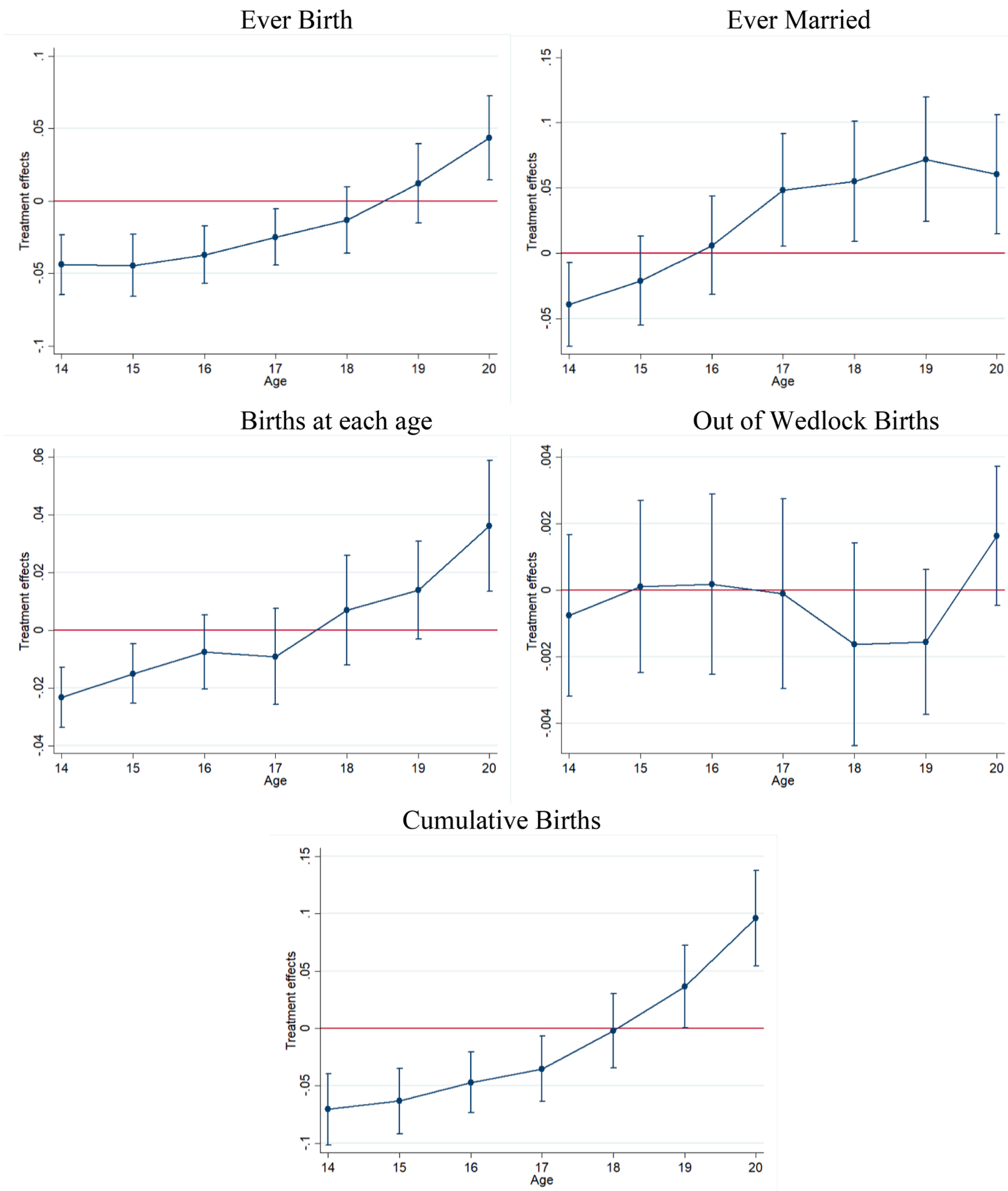

Fig. 4. Fertility and Marital Outcome Graphs.
Note: These graphs plot the estimated coefficients and 90% confidence intervals derived from our main specifications, for women in each age group. The various panels depict the impact of compulsory schooling reform on fertility and marital outcomes for cohorts of women affected by the new law. Standard errors are clustered at the month-year of birth level. All specifications include age and province fixed effects. *** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

grade 9, we see that the policy is able to push females to complete at least lower secondary school by between 7.6 to 8.9 percentage points (in Table 1 (2)). The results are statistically significant through different specifications at the conventional levels. Therefore, even though the compulsory schooling law was directed towards lower secondary education, we see spillover effects on school attendance and completion beyond the lower secondary level.

We also look at which kind of education increases in higher secondary school. There are two tracks that students can take in higher secondary school – vocational education (trade school) or the academic track. We see in Table 1 (3) that the compulsory education law increases vocational education in higher secondary school between 1.9 and 4.3 percentage points. This hints towards affecting the marginal girl child – one who may have dropped out of school but due to the policy remained in the school and went on to choose the vocational track to improve her chances in the labor market. Correspondingly, we do not see the same increase in academic track education for girls, at the conventional significance levels in Table 1 (4).

There is no significant impact of the policy on increasing male education, as shown in Table 1 (5)–(8). The results for males are also corroborated by the graphs in Appendix Fig. A4. We do not observe the jump at the cutoffs for both lower secondary education as well as completing at least Grade 9. There are also no significant effects on increasing the vocational or academic track in upper secondary

Table 2
Heterogeneity in Effect of Compulsory Education.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Rural		Urban		Non-Muslims		Muslims	
Dependent Variable	Lower Secondary Education	At least Grade 9	Lower Secondary Education	At least Grade 9	Lower Secondary Education	At least Grade 9	Lower Secondary Education	At least Grade 9
Linear	0.102*	0.119***	0.03	0.0552*	0.061*	0.087***	0.089***	0.095**
	(0.081)	(0.000)	(0.332)	(0.078)	(0.067)	(0.000)	(0.010)	(0.012)
	[0.121]	[0.000]	[0.398]	[0.234]	[0.104]	[0.000]	[0.035]	[0.035]
Quadratic	0.125**	0.107***	-0.017	0.054	0.0773**	0.071***	0.072	0.122**
	(0.031)	(0.003)	(0.706)	(0.139)	(0.030)	(0.004)	(0.163)	(0.016)
	[0.047]	[0.008]	[0.706]	[0.208]	[0.06]	[0.014]	[0.19]	[0.025]
Cubic	0.27***	0.119**	-0.019	0.0612	0.146**	0.099**	0.142***	0.095
	(0.001)	(0.013)	(0.694)	(0.185)	(0.019)	(0.046)	(0.000)	(0.195)
	[0.004]	[0.027]	[0.694]	[0.405]	[0.057]	[0.094]	[0.001]	[0.408]
Province FE	YES	YES	YES	YES	NO	NO	NO	NO
Observations	1653	1687	1289	1323	1953	2008	989	1002

Note: p-values in parentheses. Simes (1986) corrected p-values adjusted for multiple-hypotheses testing are also noted below each coefficient. Province FE are not included in the religion heterogeneity regressions due to non-presence of Muslims in some provinces. Standard errors are clustered at the cohort level.
*** Significant at 1% level,.
** significant at 5% level,.
* significant at 10% level.

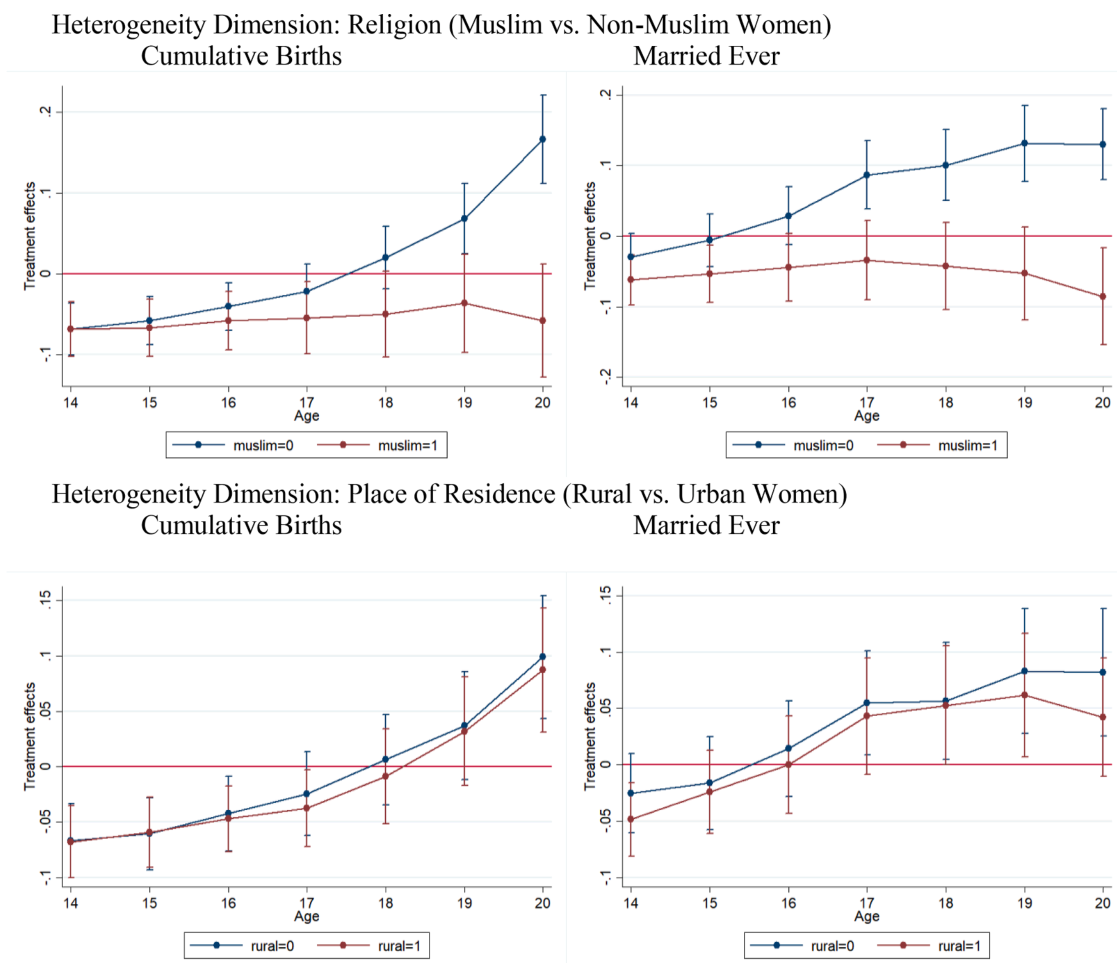

Fig. 5. Heterogeneity in Fertility and Marital Outcomes.
Note: These graphs plot the estimated coefficients and 90% confidence intervals derived from our main specifications, for women in each age group. Standard errors are clustered at the month-year of birth level. All specifications include age and province fixed effects.

education in most of the specifications. In Thailand, males have higher dropout rates due to factors such as problems in school, economic hardships leading to working for the family, parent's migration, etc. (refer to Appendix Fig. A6). We observe higher dropout rates for males in our sample as well (see Appendix Table A3). Moreover, the policy mostly works in nudging parents to enroll their children in school but is not effective in monitoring and reducing dropout rates (see Appendix Table A3). This could explain the ineffectiveness of the policy in

Table 3
Robustness Checks – Optimal Bandwidth Selection.

Panel A: Impact on Schooling

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Females				Males			
Dependent Variable	Lower Secondary Education	Years of Education (Total)	Completing at least Grade 9	Vocational Education in High School	Lower Secondary Education	Years of Education (Total)	Completing at least Grade 9	Vocational Education in High School
Linear	-0.132	1.12**	0.13**	0.043**	-0.072	-0.003	0.054**	0.004
	(0.216)	(0.037)	(0.012)	(0.023)	(0.195)	(0.988)	(0.047)	(0.861)
Optimal Bandwidth	95.42	45.05	45.05	95.42	86.26	63.37	45.05	95.42
Observations	5445	2230	2230	5445	4285	2877	1848	4768

Panel B: Heterogeneity in Effect of Compulsory Education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Rural		Urban		Muslims		Non-Muslims	
Dependent Variable	Years of Education	At least Grade 9	Years of Education	At least Grade 9	Years of Education	At least Grade 9	Years of Education	At least Grade 9
Linear	0.725***	0.132***	1.158***	0.052	1.317***	0.119***	0.619**	0.123***
	(0.001)	(0.000)	(0.005)	(0.285)	(0.001)	(0.008)	(0.018)	(0.003)
Optimal Bandwidth	63.37	77.11	67.95	63.37	75.53	67.95	67.95	67.95
Observations	1896	2425	1622	1480	1329	1218	2460	1479

Note: p-values in parentheses. The optimal bandwidth for each of the specifications are noted as per the Placebo Zone Model Selection procedures. The procedure yields the linear specification as the optimal level for our model. Standard errors are clustered at the cohort level.
*** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

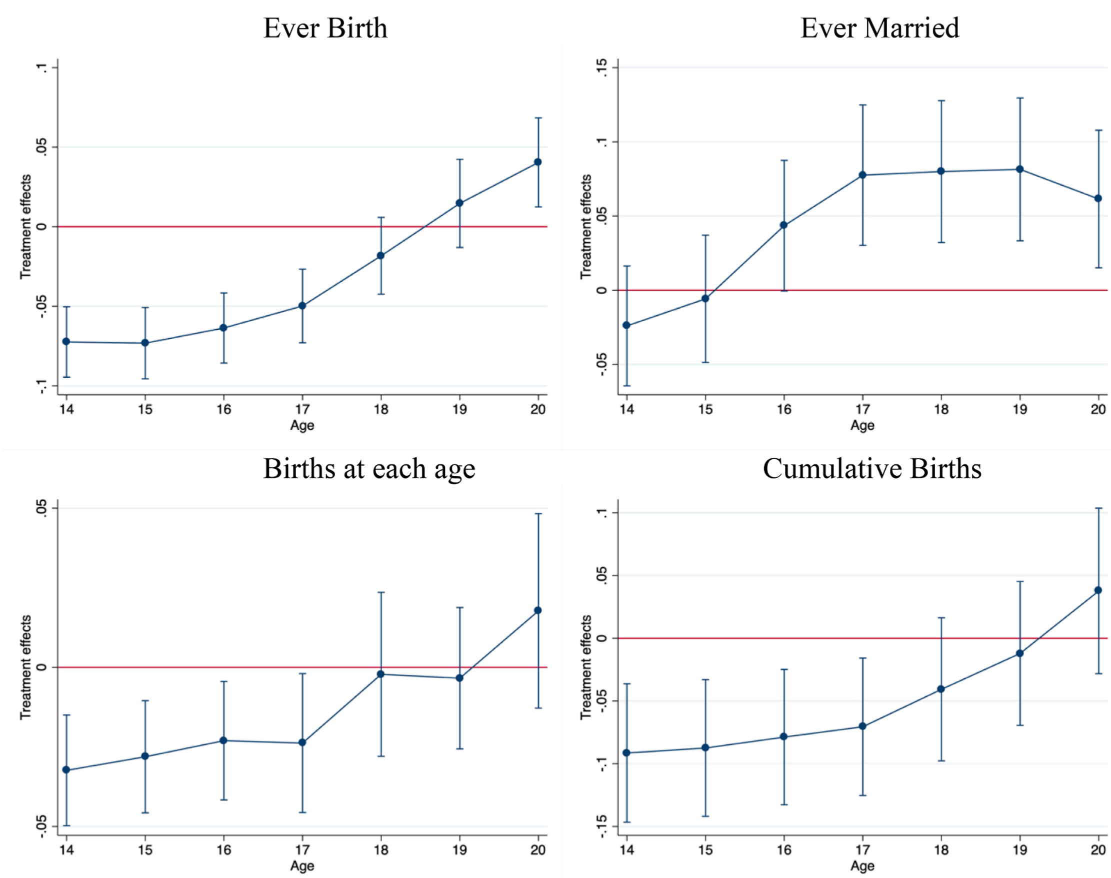

Fig. 6. Robustness Check – Fertility and marital outcome graphs with optimal bandwidth selection.
Note: These graphs plot the estimated coefficients and 90% confidence intervals using bandwidths derived from placebo zone model selection (PZMS) specifications, for women in each age group. The various panels depict the impact of compulsory schooling reform on fertility and marital outcomes for cohorts of women affected by the new law with optimal bandwidth selection. All specifications are estimated with a 12-month donut hole. Standard errors are clustered at the month-year of birth level. All specifications include age and province fixed effects.

Table 4
Robustness Checks – Alternate Model Specifications.

Age	No Donut Hole (60-month window)		Alternate window (72 months)		Alternate window (48 months)		Alternate Donut Hole (24-mo donut, 60-mo window)		Alternate Donut Hole (6-mo donut, 60-mo window)	
	(1) Ever Married	(2) Ever Birth	(3) Ever Married	(4) Ever Birth	(5) Ever Married	(6) Ever Birth	(7) Ever Married	(8) Ever Birth	(9) Ever Married	(10) Ever Birth
14 years	−0.0517**	−0.045***	−0.0349**	−0.0398***	−0.0321	−0.0416***	−0.0327	−0.0427***	−0.0536***	−0.0455***
	(0.005)	(0.000)	(0.050)	(0.000)	(0.146)	(0.008)	(0.169)	(0.002)	(0.002)	(0.000)
15 years	−0.0316*	−0.0442**	−0.0145	−0.0407***	−0.0166	−0.0420**	−0.0131	−0.0436***	−0.0348*	−0.0462***
	(0.098)	(0.000)	(0.456)	(0.000)	(0.481)	(0.011)	(0.599)	(0.002)	(0.053)	(0.000)
16 years	−0.00646	−0.0374***	0.0288	−0.0321***	0.00448	−0.0375**	0.0192	−0.0343***	−0.0103	−0.0396***
	(0.749)	(0.000)	(0.154)	(0.001)	(0.866)	(0.018)	(0.485)	(0.007)	(0.603)	(0.000)
17 years	0.0265	−0.0246**	0.0628***	−0.0169*	0.0402	−0.0312*	0.0695**	−0.0195	0.0279	−0.0292***
	(0.241)	(0.010)	(0.007)	(0.098)	(0.183)	(0.058)	(0.026)	(0.112)	(0.212)	(0.004)
18 years	0.0361	−0.0112	0.0703***	0.00823	0.0481	−0.0271	0.0787**	0.00461	0.0361	−0.0165
	(0.139)	(0.313)	(0.003)	(0.467)	(0.141)	(0.124)	(0.016)	(0.764)	(0.138)	(0.184)
19 years	0.0547**	0.0095	0.0764***	0.0373**	0.0667*	−0.00421	0.0937***	0.0331*	0.0535**	0.00663
	(0.033)	(0.460)	(0.002)	(0.011)	(0.050)	(0.837)	(0.006)	(0.096)	(0.036)	(0.649)
20 years	0.0439*	0.0400***			0.0620*	0.0208	0.0695**	0.0685***	0.0448*	0.0353**
	(0.088)	(0.010)			(0.057)	(0.315)	(0.029)	(0.002)	(0.075)	(0.035)
21 years					0.0486	0.0488**				
					(0.128)	(0.045)				
N	25,208	25,208	23,037	23,037	18,123	18,123	15,248	15,248	23,400	23,400

Note: p-values in parentheses. Estimates with a linear specification are noted for each of the models. Standard errors are clustered at the month-year of birth level. All specifications include age and province fixed effects.
*** Significant at 1% level,.
** significant at 5% level,.
* significant at 10% level.

Table 5
Other Robustness Checks.

Age	School Cohort Clustering		Falsification Test (birth year:1978)		Common Trend		Complete Birth History Only		No Province FE	
	(1) Ever Married	(2) Ever Birth	(3) Ever Married	(4) Ever Birth	(5) Ever Married	(6) Ever Birth	(7) Ever Married	(8) Ever Birth	(9) Ever Married	(10) Ever Birth
14 years	−0.0390**	−0.0439**	0.0477	0.0167	−0.0359*	−0.0378***	−0.0409**	−0.0469***	−0.0364*	−0.0432***
	(0.020)	(0.032)	(0.104)	(0.133)	(0.051)	(0.002)	(0.026)	(0.000)	(0.078)	(0.001)
15 years	−0.0208	−0.0445**	0.0502	0.0138	−0.0177	−0.0383***	−0.0214	−0.0444***	−0.0183	−0.0437***
	(0.208)	(0.031)	(0.115)	(0.228)	(0.370)	(0.002)	(0.268)	(0.000)	(0.402)	(0.001)
16 years	0.00617	−0.0373**	0.0446	0.0154	0.00929	−0.0312***	0.00856	−0.0375***	0.00883	−0.0366***
	(0.687)	(0.024)	(0.153)	(0.176)	(0.671)	(0.009)	(0.683)	(0.000)	(0.714)	(0.004)
17 years	0.0485***	−0.0249*	0.0355	0.00679	0.0517**	−0.0188	0.0477*	−0.0233**	0.0512*	−0.0242**
	(0.007)	(0.098)	(0.272)	(0.599)	(0.043)	(0.109)	(0.052)	(0.023)	(0.061)	(0.048)
18 years	0.0551***	−0.0133	0.00498	0.00832	0.0582**	−0.00721	0.0593**	−0.0109	0.0578**	−0.0126
	(0.008)	(0.395)	(0.874)	(0.564)	(0.032)	(0.606)	(0.027)	(0.346)	(0.048)	(0.379)
19 years	0.0720***	0.0120	−0.0193	0.00843	0.0752***	0.0181	0.0766***	0.0171	0.0747**	0.0127
	(0.003)	(0.557)	(0.510)	(0.595)	(0.008)	(0.275)	(0.006)	(0.255)	(0.014)	(0.453)
20 years	0.0606***	0.0435*	−0.0175	−0.0136	0.0637**	0.0496***	0.0654**	0.0507***	0.0633**	0.0442**
	(0.004)	(0.091)	(0.566)	(0.404)	(0.018)	(0.006)	(0.016)	(0.003)	(0.029)	(0.015)
N	20,907	20,907	20,033	20,033	20,907	20,907	20,194	20,194	20,907	20,907

Note: p-values in parentheses. Standard errors are clustered at the month-year of birth level, unless otherwise noted. All specifications include age and province fixed effects, unless otherwise noted.
*** Significant at 1% level,.
** significant at 5% level,.
* significant at 10% level.

increasing male education. In Thailand, girls might have a lower chance of enrolling, but once they are in, they are less likely than boys to drop out and instead continue their schooling (Tantiwiramanond, 1998).

Since we observe clear effects on female schooling completion, we next turn to look at marriage and fertility decisions by women. There is a rich literature that finds a strong link between women's education and fertility decisions both in developed and developing countries (Black et al., 2008; Keats, 2018; Lam & Duryea, 1999; McCrary & Royer, 2011; Osili & Long, 2008). The policy may impact fertility decisions through delayed marriage and general equilibrium effects affecting marriage outcomes. By constructing a women-age panel, we are able to estimate the impact on these outcomes at each age group between the treated and control groups. These coefficients and confidence intervals are plotted in Fig. 4. We observe a significant drop in ever being married at age 14 by 4.1 percentage points. However, the drop does not sustain beyond age 14, and by age 17, we see an increase in the probability of ever being

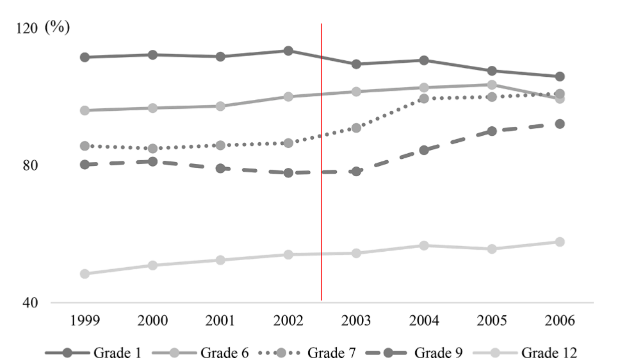

Fig. A1. Students in the formal education system as a percentage of school-age population.[34]
Source: Data from Brief Education Statistics (2000–2006) from the Ministry of Education.

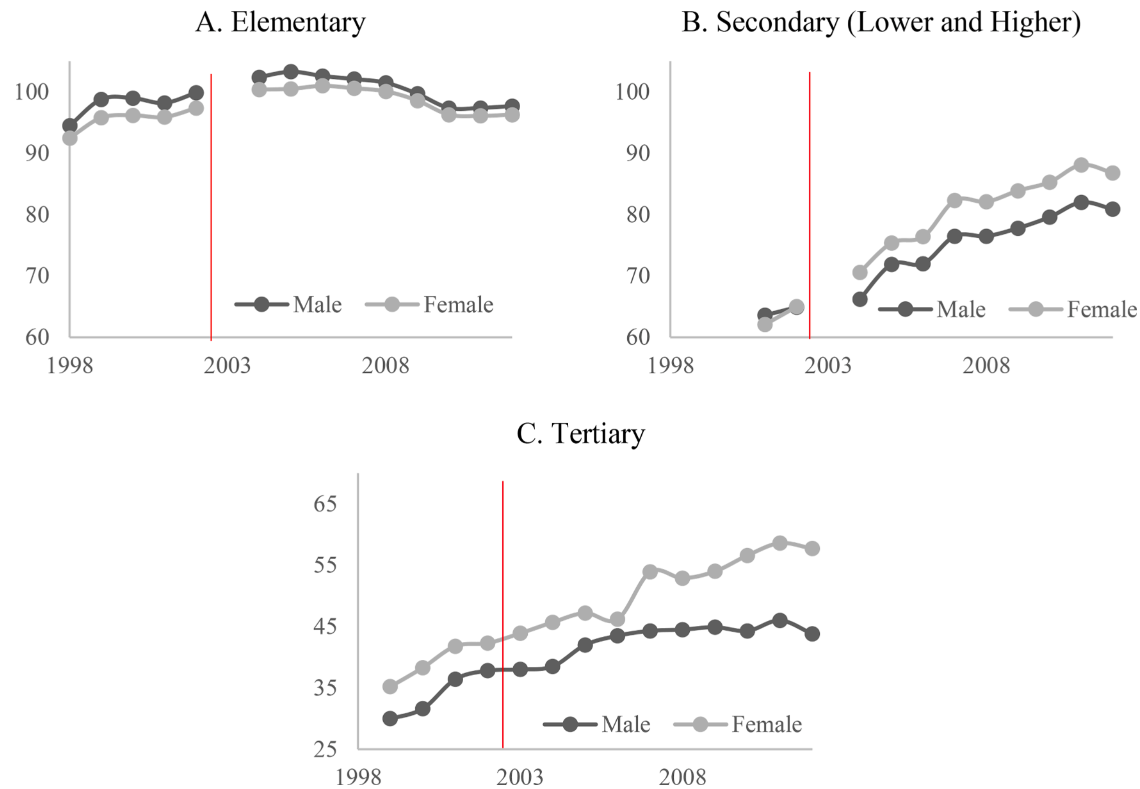

Fig. A2. Gross school enrollment (%) by sex and education level.
Source: World Development Indicators (The World Bank, 2012)

married.

We document a decrease in probability of ever giving birth, births at each age, and cumulative births during school years (i.e., until 17 years). The probability of ever giving birth decreases with the largest drops around 4–5 percentage points at 14 and 15 years. Fertility then increases after school years, and it is significantly higher when the women reach 20 years. We do not observe any statistically significant fall in out of wedlock births due to this policy. Compulsory schooling is effective in postponing fertility from age 14–17 years to 18–20 years. This posits as a direct effect of being in school, known in the literature as "incarceration effect". The woman delays her fertility as young motherhood during schooling can impose a significant time and opportunity cost. As argued, that the schooling law impacts the marginal girl child and by keeping her in the school, the law is effective in dissuading fertility but only while she is in the school (until 17 years of age) due to the time costs. This is consistent with literature evaluating the link between education and fertility and the postponement of fertility to later years in many developing countries (Breierova & Duflo, 2004; Kırdar et al., 2018; Osili & Long, 2008).

Education can also affect fertility through increases in human capital accumulation, known as the "human capital effect". Since more education leads to greater human capital accumulation, increases in female education can lead to lower early fertility or postponement of fertility to later years (Black et al., 2008; Osili & Long, 2008). This can occur due to positive assortative mating, changes in child health, labor market opportunities, and other general equilibrium effects in marriage markets (Black et al., 2008; Keats, 2018; Lam & Duryea, 1999; McCrary & Royer, 2011; Osili & Long, 2008). It should be noted that even though the compulsory schooling law was enacted until Grade 9, we observe persistent effects in decline in fertility even in the higher secondary school (two years after Grade 9).[17] Since the impact of the law on educational attainment persist beyond the mandated lower secondary schooling, it hints towards the effects of the policy in increasing human capital accumulation. Therefore, the postponement of fertility to after school completion indicates presence of human capital effects, even though the effects are short lived. To analyze the link between education and fertility, we look at the marriage rates as an explanation. In our sample, the highest marriage/cohabitation rates are in the nineteen and twenty-year-old, leading to the higher percentage of first births between the age of 20–21 years (see Appendix Fig. A7). Therefore, the policy is effective in delaying the first birth for women from school to after-school years, indicating a catch-up in first births as women get married after school. However, there are benefits to be noted from a delay in child-bearing age. Happel et al. (1984) develop a fertility timing model and show that date of first birth influences the mean and dispersion of household's income distribution. Therefore, by delaying fertility to later ages, the compulsory schooling law could potentially change inter-temporal and intergenerational outcomes.

5.1. Heterogeneity

We now turn to look at the differential impact of the policy vis-à-vis two dimensions – first, if the affected cohort of young women belongs to a rural area, and second, if the affected cohort is of the Muslim religion. We can expect heterogeneity along these dimensions due to the marginal impact the policy can make on these populations. In rural areas, where

[17] Per the law, the compulsory schooling required students to be in school until Grade 9 or 16 years of age. However, the average age of students completing lower secondary school in Thailand is 15 years. Since we observe significant declines in fertility until age 17, there are two years of spillover effects of the law in these provinces.

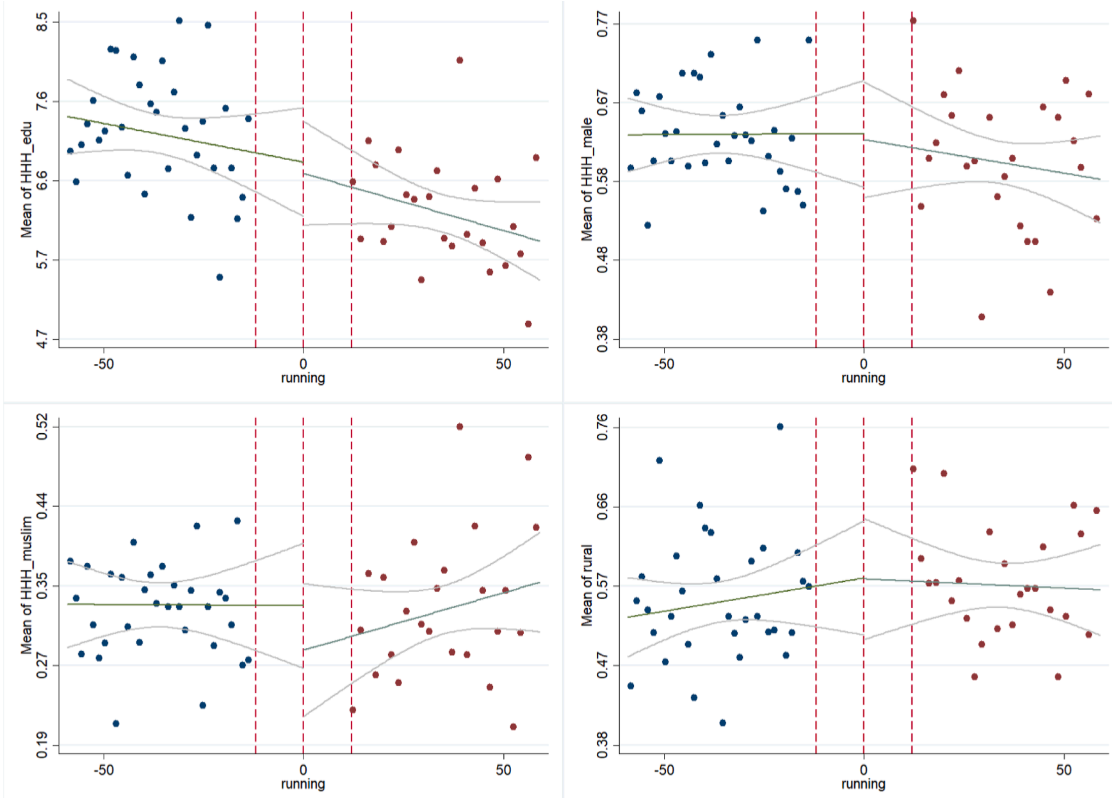

Fig. A3. Covariate Balance.
Note: The figure shows covariate balance across household head's education, gender, religion, and place of residence.

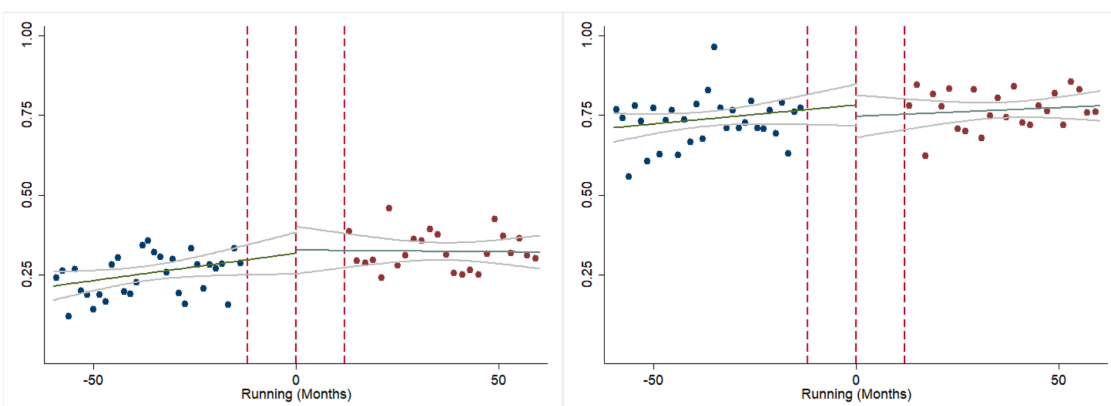

Fig. A4. School Completion for Males.
Note: The left-hand side panel plots fraction of males with Grade 9 as their highest level of education and the right-hand side panel plots the probability of completing at least Grade 9.

education is lower than in urban areas, there would be more marginal children affected by the policy. We visually see this effect in Appendix Fig. A5. We see a clear jump in increasing the completion of at least Grade 9 for rural women but not as much for urban women.

Table 2 presents the RD estimates for the effect of compulsory schooling on secondary school completion as well as at least completing Grade 9 for various groups. As we look at Table 2 (1)–(4), we observe significant impact of the law on grade completion for rural women but mild effects on urban women.[18] For rural women, the statistically significant effect ranges from 10 to 27 percentage points increase in secondary schooling in different specifications. We also see the law effective in increasing completion of at least Grade 9 for rural women by about 11 percentage points while it increases for urban women by about 5.2 percentage points.[19] Women in rural areas see a significant increase

[18] We also have slightly smaller sample sizes for urban women that may contribute to low power and not being able to detect significant changes.

[19] We also observe increase in total years of education for both rural and urban women. However, these are not significantly different from each other. The results are presented in Table 3 Panel B.

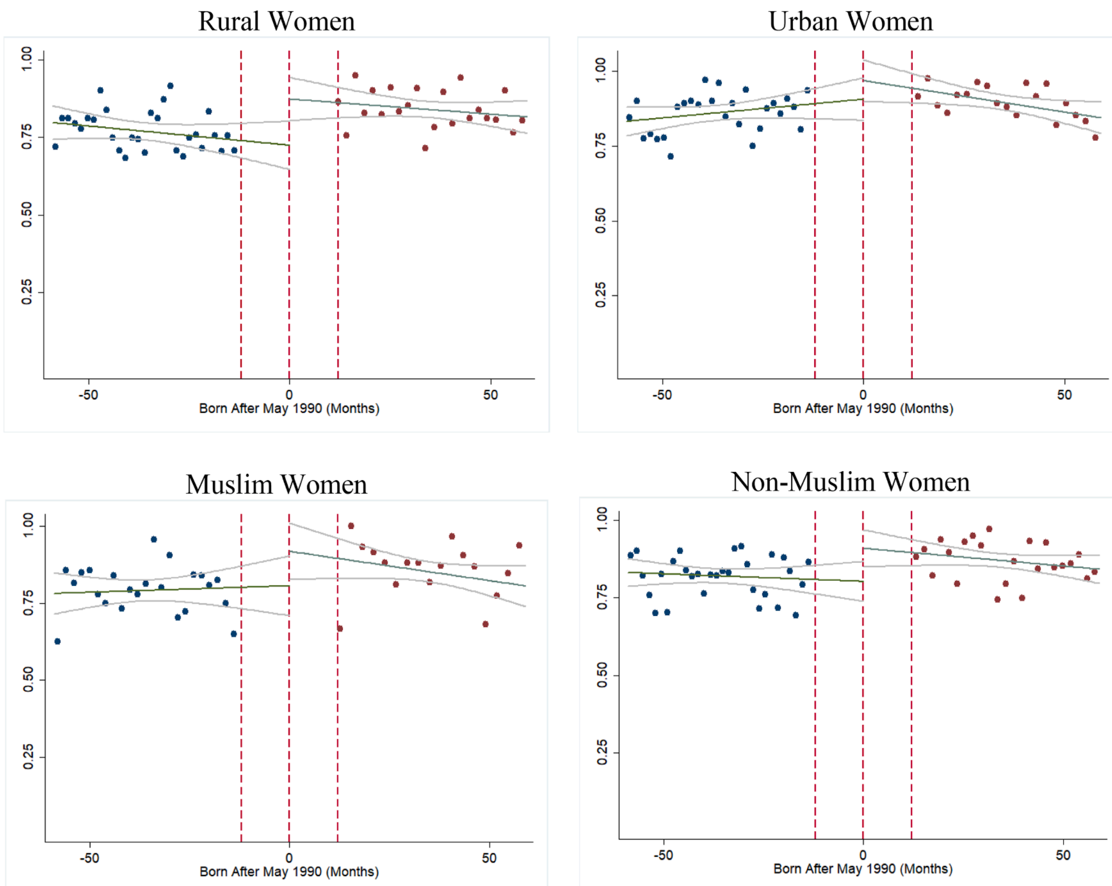

Fig. A5. Heterogeneity in Educational Attainment.
Note: The panels show the heterogenous impact of compulsory schooling laws on completing at least lower secondary school for rural vs. urban women (top panel) and Muslim vs. Non-Muslim women (bottom panel).

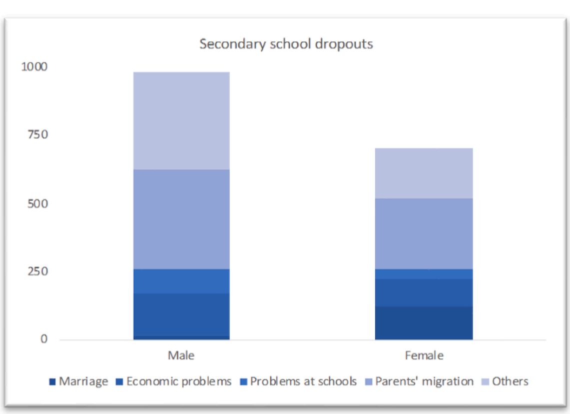

Fig. A6. Secondary School Dropout Rates.
Note: This figure plots the secondary school dropout rates from the public school system in the North in 2013 by gender. The different shades represent the various reasons provided for children to drop out of school. Source: Dropout Statistics for Administrative Areas 15–16 (2013–2014) by Administrative Area 15 Regional Education Office

in completion of at least Grade 9.[20] This is expected as rural areas would have more marginal students to be impacted by this law leading to a stronger household response to the compulsory schooling law. However, it is also possible that the implementation of policy may have been better in the rural areas due to the nature of the school system. Schools in rural areas are mostly public schools regulated by the Office of Basic Education Commission (OBEC). Urban (or municipal) areas usually have other types of schools (for example, private schools and public schools run by other government units), which fall under different regulators. This might make the implementation more homogenous in the rural areas. Both these factors could explain the stronger results on education for rural women. Given that we do not have data on the intensity of implementation, we are not able to separate these channels.

Table 2 (5)–(8) illustrates the impact on educational attainment for Muslim vs. non-Muslim women. We see that the magnitude of impact of education is similar across these two groups. We also do not find any statistically significant difference between Muslim and non-Muslim women in completion of lower secondary schooling or of at least Grade 9 (see Appendix Table A5). Table 2 (8) and (6) depict that the law was successful in helping Muslim young women complete at least Grade 9 by 9.5 to 12 percentage points more than before while the same estimates for non-Muslim young women were 7 to 9.9 percentage points. However, the type of education acquired by Muslim women is significantly different from the other group. We find a statistically significant

[20] The statistical test for difference in coefficients using hypothesis testing from Seemingly Unrelated Regressions (SUR) is presented in Appendix Table A5.

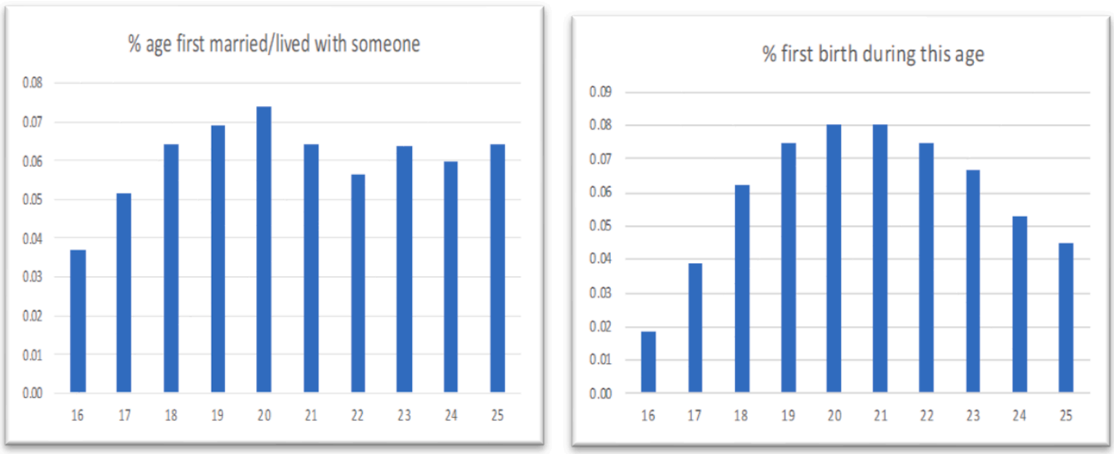

Fig. A7. Age distributions.
Note: These figures plot the age distribution for women's first marriage and first births in our sample.

decrease in vocational education for Muslim women (see Appendix Table A5).[21]

We also evaluate the presence of any differential fertility and marriage effects across these groups. Fig. 5 gives a visual representation of the effect on marriage and fertility along various dimensions pertaining to religion and place of residence.[22] We find a significant decrease in marriage for Muslim women. This decrease is statistically significant at the age of 14-15, with a significant portion of the confidence intervals falling in negative territory for the remainder of the study period, until the age of 20. This sustained decrease contrasts with the non-Muslim women's increased marriage as they get older, resulting in statistically significant differences in these outcomes by religion from 16 years onwards (see Appendix Table A5). The cumulative births for Muslim women decrease by about 6 percentage points, which sustains until the age of 17 years.[23] Interestingly, we do not find an increase in births for Muslim women after school years and it is significantly lower than non-Muslims (from 18 to 20 years). The type of education acquired by Muslim women can explain the persistent effects on fertility and marital outcomes. As discussed, since Muslim women opt to not pursue vocational education and instead pursue college education, these academic pursuits can lead us to observe the continued depressed effects on fertility even after high school. This is in line with increasing female empowerment and its link with fertility as a result of education (Doepke & Tertilt, 2018; McCrary & Royer, 2011).

The "human capital effect" is predominant and persists for a longer time in the Muslim population as we do not observe the catch-up in fertility as we do for the non-Muslim population. We explore two explanations. First, it has been shown that men usually desire larger families than women, and if education leads to increased household bargaining power for women, we should see a fall in fertility (Doepke & Tertilt, 2018). Since the gender gap in education in the southernmost provinces in Thailand, inhabited mostly by Muslims, is more pronounced; the policy has a greater impact on Muslim girls' education, changing preferences for females, and perhaps increasing the bargaining power for them in the households.[24] Second, it can also be argued that differences in labor market opportunities and wages would impact marriage and fertility choices of women, and if these are different for Muslim women, that could explain the differential impact. MICS does not have data on labor force participation rates or wages, but we look at the 2015 Labor Force Survey to analyze the human capital impact of increased education. We observe that the female labor force participation rate for women in the 20–24 years age group is higher in the South (the region mostly inhabited by Muslims) than the whole kingdom.[25]

It is also possible that educating Muslim women has a more general equilibrium effect in the marriage market that leads to assortative mating and marrying a more educated spouse. Note that we do not see a concurrent increase in Muslim men's education due to the policy in our sample. Yet, the women might marry males older than them and later in life. We cannot check for these mechanisms due to data limitation. Nonetheless, our analysis suggests that Muslim women experience lower marriage rates than their counterparts for a sustained period of time, suggesting a delay in marriage and possibly more time spent in search in the marriage market, perhaps, leading to better matches. These mechanisms could explain the additional increase in education for Muslim women beyond the compulsory level and the impact on marriage and fertility.

On the other hand, we do not find any statistically differential impact on marriage and fertility between rural and urban women (see Appendix Table A5 and Fig. 5). Both rural and urban women display a fall in fertility and marriage until 17 years, with a corresponding increase in these rates after school years. The fall in fertility and marriage during school going age results from the impact of the law in increasing education in rural and urban areas. However, despite a stronger increase in education levels in rural women due to the compulsory schooling laws, we do not note a corresponding big effect on marriage and fertility vis-à-vis the urban women. We explore two mechanisms that can explain the dampened effect on fertility and marriage for rural women. First, it is possible that the imposed cost of having children during school years is less due to a larger family. We check the household size and number of elderly living in a household in MICS and note that both are larger in

[21] We also see an increase in college education. However, these results vary in magnitude and significance across different specifications and are not consistent. Results available on request.

[22] We also check heterogeneity based on wealth, household head's education, and type of educational degree but do not find any statistically significant difference in the outcomes of these populations. Results available on request.

[23] Although the magnitudes are not statistically significantly different from non-Muslims in these early years (refer to Appendix Table A5).

[24] We do not observe any increases in Muslim male education in our sample, effectively decreasing the gender gap in education for Muslims. Results available on request.

[25] The Labor Force survey for 2015 is collected by the National Statistical Office (http://www.nso.go.th/sites/2014en/Pages/Statistical%20Themes/Population-Society/Labour/Labour-Force.aspx)

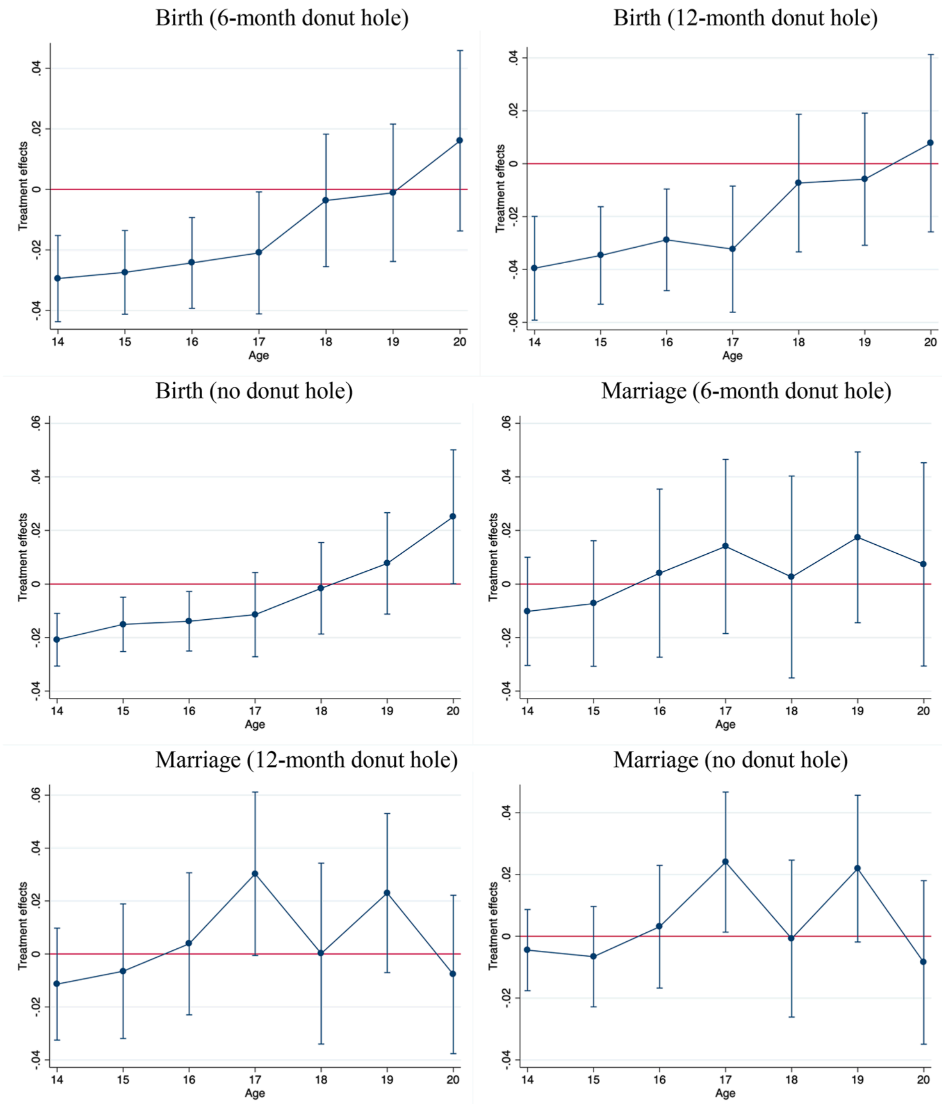

Fig. A8. Alternate Specification - Hazard Model Estimates.
Note: These graphs plot the estimated coefficients and confidence interval derived from hazard models (with different donut holes), for women in each age group. The bandwidth is selected by PZMS. Standard errors are clustered at the month-year of birth level. All specifications include age and province fixed effects.

rural areas.[26] This could be a potential channel explaining the muted effects on fertility for rural women despite seeing a pronounced increase in education levels. Second, absence of labor markets or lower wages in rural areas could explain lower incentive in decreasing fertility or marriage. Therefore, we note that labor force participation rate for women with lower and upper secondary education is lower than in urban areas, according to data from the Labor Force Survey in 2015. There is also a wide disparity in the wages earned by females in rural areas (10,804 THB) vs. in urban areas (15,748 THB).[27] The lower returns to education in rural areas could potentially explain why we do not see the corresponding differential effects on fertility and marriage. Therefore, complementary investments in labor market opportunities could lead to a longer-term impact of compulsory education policies and provide a stronger economic incentive to delay childbearing.

We explore further the reasons for the ineffectiveness of the policy in increasing male education in our sample. There may be subsample heterogeneity and migration could also partly explain the null effects of the policy on male education. For males, we do find subsample countervailing effects of the policy on education attainment by region, leading to null overall effects. First, we document heterogeneity by region, focusing on the deep south provinces in Thailand. Though the gender gap in education in Thailand has been closing, there are documented exceptions in the three southernmost provinces in Thailand (Tantiwiramanond, 1998). These southern provinces are a part of our MICS sample and explain the strong effects of the policy on improving only the educational status of girls (and not boys). Since girls may have a lower chance of enrolling in these regions, the policy could in fact help in getting these girls to school.[28] Lastly, we also check for subsample effects by gender-specific migration rates of the region. There is documented evidence indicating the presence of rural-to-urban migration,

[26] Results available on request.
[27] Cleaned data available on request.

[28] We document heterogeneity and find statistically significant increases in female lower secondary education, completion of at least grade 9, and total years of education in this deep south sub-sample to be higher in magnitude than rest of Thailand. Results in Appendix Table A6 panel A.

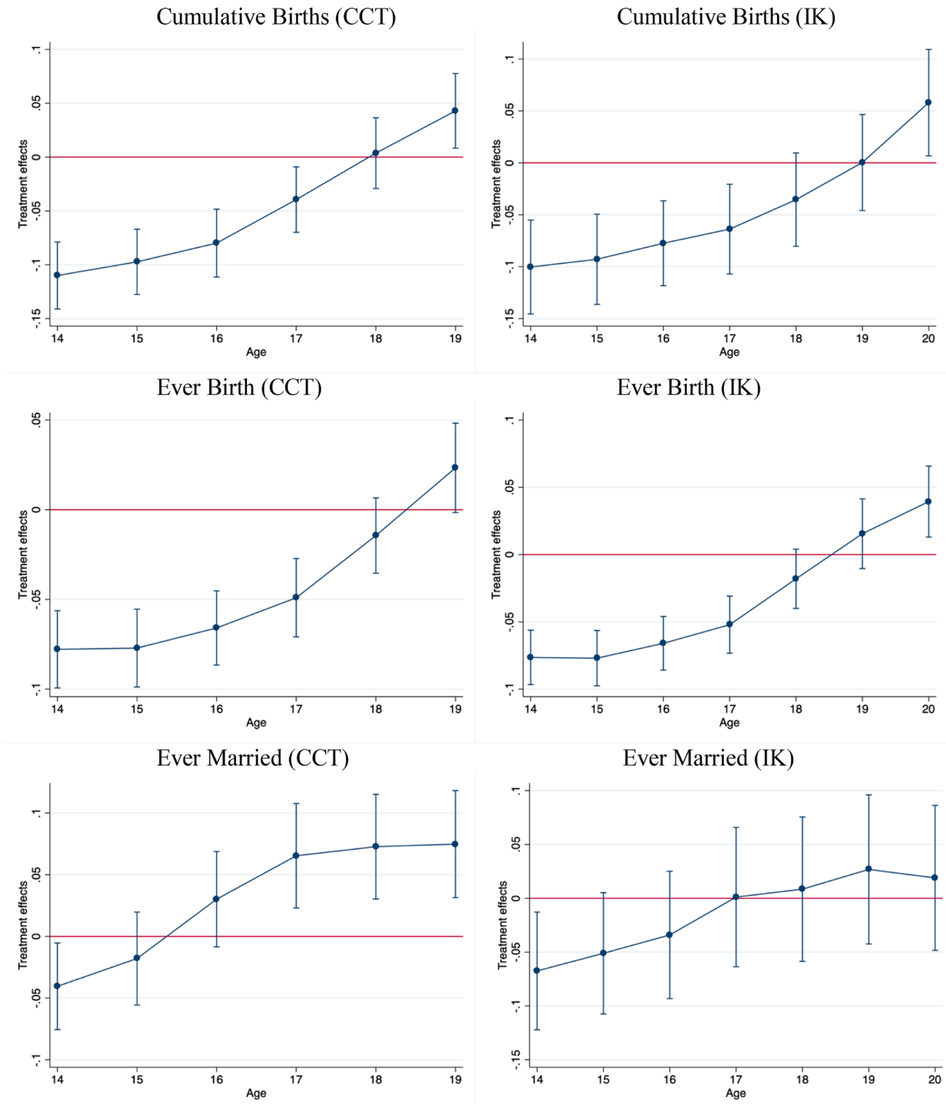

Fig. A9. Alternate Bandwidth Selection – CCT/IK Estimates.
Note: These graphs plot the estimated coefficients and 90% confidence intervals using bandwidths derived from CCT/IK specifications, for women in each age group. The various panels depict the impact of compulsory schooling reform on fertility and marital outcomes for cohorts of women affected by the new law with optimal bandwidth selection. All specifications are estimated with a 12-month donut hole. Standard errors are clustered at the month-year of birth level. All specifications include age and province fixed effects.

with the highest migration rate observed in the northeast region (Curran et al., 2003; NSO, 2011[29]). While our data does not provide information on migration, we analyze the male rural and urban subsamples within northeast region. For males in rural areas in the northeast region, we find an increase in lower secondary education but not in college education. Correspondingly, for urban males, we find increases in the college education in the northeast region. As migration occurs, people with higher human capital may systematically migrate from rural to the urban areas. The increase in college education only for urban males (in northeast region) could point towards the higher human capital in that region at the time of survey.[30]

It is also important to note the effect of female migration on these results. In Thailand, women migrate for jobs in the manufacturing and construction sectors and female migrants send higher remittances back home than their male counterparts (Curran et al., 2003). Since we observe the place of residence only at the time of survey, these migrations from rural to urban areas would bias the impact of the policy on rural women's education downwards, if women with higher human capital systematically migrate to the urban areas for these jobs. Migration could also partly explain the muted effect on fertility seen in rural areas as women with higher human capital migrate to urban areas after school. In the absence of migration, we could have observed stronger, and perhaps persistent declines in fertility for rural women. Unfortunately, given the cross-sectional nature of our data, we are not able to track these migrations of women overtime to be able to estimate the interlinkages between education, human capital accumulation, and migration.

5.2. Robustness

We carry out a series of robustness tests to ensure that our results are not driven by our empirical specification. In Table 3 Panel A, we show the results on schooling by using optimal bandwidth selection procedures (PZMS) for females and males. Since the procedure always picks the linear specification over the quadratic and cubic, we present those results here. Consistent with the main results, we observe a significant impact on completing at least Grade 9, total years of education, and vocational education for females. The magnitude of impact on females completing at least lower secondary education and choosing a vocational track in higher secondary, is, in fact, higher in this model. To be complete and consistent with the literature, we also show the results

[29] Statistics from the Migration Survey 2011 by the National Statistical Office (http://www.nso.go.th/sites/2014en/Pages/survey/Social/Demographic,%20Population%20and%20Housing/Migration-Survey.aspx)

[30] Results presented in Appendix Table A6 panel B.

Table A1
Summary statistics of key variables.

	Mean	SD	N	Min	Max
Education outcomes					
Highest degree - lower secondary	0.216	0.412	3663	0.00	1.00
Highest degree - upper secondary	0.354	0.478	3663	0.00	1.00
Upper secondary - academic	0.304	0.460	3663	0.00	1.00
Upper secondary - vocational	0.051	0.220	3663	0.00	1.00
Highest degree - college	0.211	0.408	3663	0.00	1.00
Years of education	11.350	3.769	3747	0.00	18.00
Completed >= lower secondary edu	0.833	0.373	3747	0.00	1.00
Marriage & fertility outcomes					
Ever married	0.750	0.433	3749	0.00	1.00
Currently married	0.690	0.462	3749	0.00	1.00
Age when first married	19.601	3.463	2810	10.00	30.00
Ever given birth	0.682	0.466	3749	0.00	1.00
Age when given first birth	21.151	3.195	2538	12.25	29.75
Aged <=15 when given first birth	0.008	0.086	3729	0.00	1.00
Aged 16–18 when given first birth	0.098	0.297	3729	0.00	1.00
Aged 19–22 when given first birth	0.248	0.432	3729	0.00	1.00
Aged 23–30 when given first birth	0.244	0.429	2969	0.00	1.00
Demographic and other outcomes					
Woman's age	25.400	2.860	3749	20.00	30.00
Rural area	0.557	0.497	3749	0.00	1.00
Thai household head	0.707	0.455	3749	0.00	1.00
Household head's age	48.275	15.118	3749	17.00	93.00
Male household head	0.615	0.487	3749	0.00	1.00
Household head's education	6.728	4.569	3743	0.00	20.00
Muslim household head	0.332	0.471	3749	0.00	1.00
Panel dataset - women aged 20					
Ever married	0.480	0.500	3749	0.00	1.00
Ever given birth	0.222	0.416	3749	0.00	1.00
# of births given at this age	0.123	0.334	3749	0.00	2.00
Total births at this age	0.373	0.588	3749	0.00	3.00

Note: The sample is restricted to women who were born within 60 months of May 1990. *Marriage is defined as legal marriage or cohabitant with a partner. **Fertility age dummy variables are equal to zero if a woman has never given any birth.

derived using CCT/IK bandwidth procedures in Appendix Table A4 and that they reinforce our main results. In Table 3 Panel B, we observe the heterogeneity in education by place of residence and religion, using the optimal bandwidth selection procedure. The magnitude of impact is greater for all the specifications. We also observe an increase in the years of education for both rural and urban females.[31] The choice of optimal bandwidth varies a lot in the various specifications but the results are consistent with the main specifications. We also present the results on fertility and marriage outcomes for women using the PZMS procedure to choose the optimal bandwidths for our donut hole RD model. These are presented in Fig. 6. Consistent with the main results, we find a decrease in the births up to 17 years of age and then the effects dissipate.[32] The CCT/IK results on fertility and marriage outcomes for women also align with our main model specifications. These are presented in Appendix Fig. A9 for interested readers.

Next, we present the results of different model specifications in the Tables 4 and 5. In the first model specification, we test if adopting a donut hole RD design is driving the result. The results are presented in Table 4 (1) and (2). We see that there is a decrease in fertility for ages 14 to 17, consistent with the main specification. We also observe a decrease in marriage at ages 14 and 15, which reverts back by the age of 19 years. It, also, could be argued that the definition of the estimating window may be driving the results. In Table 4 (3) – (10) we re-estimate the effects using different windows. In Table 4, (3)–(6), instead of using a window of 60 months on each side of the threshold, we change this window to 72 or 48 months. Our results remain similar in magnitude and significance as our original specifications. We also check for impact on heterogeneity results using various alternate window donut-hole specifications (viz. 24 months and 6 months) and these alterations do not change the results, as shown in Table 4 (7)–(10). Moreover, since for the younger women the outcome variables are censored, we now estimate a hazard model that allows the utilization of more data. These results are presented in Appendix Fig. A8. We observe a decrease in births between the ages of 14 and 17 in different donut holes specifications. We also observe a slight fall in marriage in earlier years, though the effects are not statistically significant in those specifications.

We also provide evidence on controlling for various trends, alternative clustering, and falsification tests in Table 5. In Table 5 (1) and (2) we show that our results hold with different level of clustering of standard errors. As we use clustering at the month-year cohort clustering in the original specification for all our marriage and fertility outcomes, we instead look at school cohort level clustering to align with the education cohorts. The results remain significant across various specifications of marriage and fertility outcomes. To ward off any concerns on time trends influencing our results, we carry out two checks. First, if we falsely assign a policy, we should see no impact. This is shown in Table 5 (3) and (4), where changing the birth year to 1978 displays no effect on marriage and fertility outcomes for women.[33] Second, we impose a common trend before and after the policy change and observe if that changes the results. Table 5 (5) and (6) provide evidence that results remain similar if we impose a common trend assumption before and after the policy change. Next, we check if our data structure is important in observing these results. These are presented in Table 5 (7) and (8). Our main results are robust to whether the women with incomplete birth history are excluded from our estimation. Lastly, in Table 5 (9) and (10), we show that excluding province fixed effects does not alter the magnitude or significance of our coefficients.

6. Discussion

Compulsory schooling laws are an important tool by which policy makers try to affect the education distribution in a society. These have been shown to be especially important for females as they may have intergenerational effects on children's schooling and child health. This paper looks at the effects of such a law in Thailand on outcomes for affected cohorts using a donut-hole regression discontinuity framework. We find significant effects on girls' education but not for boys' education. By design this policy is aimed at the lower secondary school level, but it has persistent effects beyond completion of lower secondary school. We see an increase in fraction of women with lower secondary school as their highest level of education along with a significant increase in completion of lower secondary school or higher. The policy is especially effective in changing the education of young women at risk of dropping out of school, as observed by an increase in the number of female students opting for vocational studies in high (or upper secondary) school instead of the academic track.

Exploiting the impact on girls' education, we show that this effect is translated into changing marriage and fertility patterns for girls from 14 to 20 years of age. We observe that the compulsory education law that increased the time spent in school led to decreased probabilities of ever giving birth in the school-age years (14–17 years). Our results are in line with a delay in marriage as documented by Keats (2018), with us recording a fall of probability of ever being married at age 14 by 3.9 percentage points in the context of Thailand, even though this does not persist beyond 14 years and we document an increase at 17 years of age.

[31] Note that even though the magnitudes of the coefficient on total years of education appears to be different between rural and urban population, they are not statistically different from each other.

[32] We also note a slight decrease in marriage initially, though the effects are statistically significant only in some specifications.

[33] We perform this analysis for various alternate birth years. The farther we are from the actual policy implementation, the lesser impact we see on fertility and marriage outcomes. More falsification results available on request.

Table A2
Continuity for Predetermined Variables.

	(1)	(2)	(3)	(4)
	All Women		Women living with parents	
Dependent Variable	Rural	Education of Household Head	Rural	Education of Household Head
Treatment	−0.023	0.298	−0.042	0.164
	(0.382)	(0.291)	(0.233)	(0.559)
Optimal Bandwidth	95.26	62.10	95.26	95.26
Observations	6435	4014	2995	2994

Note: p-values in parentheses. Standard errors are clustered at the month-year of birth level.
*** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

Table A3
Policy Effects on the dropout and enrollment rates, by gender.

Dependent Variable	Dropout in Lower Secondary	Enrollment in Lower Secondary
Treatment	−0.003	0.0638**
	(0.680)	(0.040)
Males	0.0077***	−0.0649***
	(0.004)	(0.000)
Treatment*Male	−0.004	−0.0285
	(0.366)	(0.191)
Province FE	YES	YES
Observations	4722	5834

Note: p-values in parentheses. Standard errors are clustered at the cohort level.
*** Significant at 1% level.
** significant at 5% level
* significant at 10% level.

The policy primarily affects the marginal child leading to the postponement of the timing of their fertility to after-school years, indicating a catch-up effect. The transitory fall in fertility during the school age years also corroborates the finding by Ali and Gurmu (2018) with women delaying fertility to after-school years rather than overhauling women's fertility preferences. Therefore, we find results consistent with the literature – with more education leading to a reduction in teen pregnancy and the corresponding benefits of a reduction in teen pregnancy accrued due to the compulsory schooling law (Black et al., 2008; Boden et al., 2008). Our results also suggest short term human capital effects as the policy is effective in reducing births beyond the lower secondary school years.

We see interesting effects exploring the heterogeneity with respect to the religion of the household. We document similar overall effects on education for both Muslim and non-Muslim population, but the

Table A4
Robustness Checks – Optimal Bandwidth Selection (IK/CCT).

Panel A: Impact on Schooling

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Females				Males			
Dependent Variable	Lower Secondary Education	Years of Education (Total)	Completing at least Grade 9	Vocational Education in High School	Lower Secondary Education	Years of Education (Total)	Completing at least Grade 9	Vocational Education in High School
CCT	0.129***	1.243***	0.131***	0.0713**	-0.0142	0.641*	0.0721	-0.0056
	(0.000)	(0.000)	(0.000)	(0.036)	(0.632)	(0.09)	(0.297)	(0.789)
IK	0.141***	1.075***	0.122***	0.0412**	-0.0230	-0.0708	0.0148	-0.0110
	(0.003)	(0.000)	(0.000)	(0.033)	(0.404)	(0.549)	(0.234)	(0.557)
Optimal Bandwidth (CCT)	29.8	53.2	51.4	31.6	35.2	31.4	27.9	31.5
Optimal Bandwidth (IK)	26	54.2	46.8	48.8	32.6	67.1	89.7	35.6
Observations (CCT)	1162	2698	2580	1280	1279	1069	842	1053
Observations (IK)	975	2773	2288	2341	1109	3127	4596	1279

Panel B: Heterogeneity in Effect of Compulsory Education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Rural		Urban		Muslims		Non-Muslims	
Dependent Variable	Years of Education	At least Grade 9	Years of Education	At least Grade 9	Years of Education	At least Grade 9	Years of Education	At least Grade 9
CCT	1.080***	0.130***	1.618***	0.123**	1.569***	0.175***	0.856***	0.118***
	(0.000)	(0.000)	(0.004)	(0.013)	(0.000)	(0.000)	(0.000)	(0.000)
IK	0.725***	0.164***	1.102***	0.0673***	1.271***	0.126***	0.857***	0.115***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Optimal Bandwidth (CCT)	49.5	43.4	36.3	43.4	44.2	41.3	49.8	47.3
Optimal Bandwidth (IK)	63.9	57.7	113.2	71.7	73.8	46.9	55.2	59.3
Observations (CCT)	1379	1194	724	921	732	665	1643	1561
Observations (IK)	1896	1658	2816	1741	1347	769	1900	2046

Note: p-values in parentheses. The optimal bandwidth for each of the specifications are noted as per the CCT/IK procedures. Standard errors are clustered to account for cluster robust nearest neighbor variance estimation.
*** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

Table A5
Heterogeneity in Effect – Statistical Test for Difference of Coefficients.

Panel A: Impact on Schooling

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Rural vs. Urban				Muslims vs. Non-Muslims			
Dependent Variable	Lower Secondary Education	Years of Education (Total)	Completing at least Grade 9	Vocational Education in High School	Lower Secondary Education	Years of Education (Total)	Completing at least Grade 9	Vocational Education in High School
Linear	0.072	-0.17	0.064*	0.067***	0.028	0.21	0.008	-0.077***
	(0.313)	(0.475)	(0.088)	(0.000)	(0.112)	(0.42)	(0.75)	(0.000)

Panel B: Impact on Fertility and Marital Outcomes

	(1)	(2)	(3)	(4)
	Rural vs. Urban		Muslims vs. Non-Muslims	
Dependent Variable	Married Ever	Cumulative Births	Married Ever	Cumulative Births
15 years	0.0132	0.002	-0.016	-0.009
	(0.417)	(0.769)	(0.298)	(0.288)
16 years	0.005	-0.004	-0.045**	-0.016
	(0.826)	(0.736)	(0.042)	(0.262)
17 years	0.009	-0.01	-0.098***	-0.035
	(0.717)	(0.569)	(0.001)	(0.137)
18 years	0.022	-0.011	-0.123***	-0.077**
	(0.455)	(0.668)	(0.0003)	(0.014)
19 years	0.000	-0.006	-0.161***	-0.12***
	(0.999)	(0.863)	(0.000)	(0.003)
20 years	-0.017	-0.014	-0.193***	-0.23***
	(0.641)	(0.745)	(0.000)	(0.000)

Note: The table lists the difference in linear coefficients and the corresponding p-values (in parentheses) between rural and urban women (columns 1-4) and Muslim and non-Muslim women (columns 5-8) for educational outcomes in our main specification. These statistics are derived from a system of Seemingly Unrelated Regressions (SUR) testing the null hypothesis of equality of coefficients between the two groups.
Note: The table lists the difference in linear coefficients and the corresponding p-values (in parentheses) between rural and urban women (columns 1 and 2) and Muslim and non-Muslim women (columns 3 and 4) for fertility and marital outcomes by age, in our main specification. These statistics are derived from a system of Seemingly Unrelated Regressions (SUR) testing the null hypothesis of equality of coefficients between the two groups.

Table A6
Heterogeneity in effect of the policy.

Panel A: Females in the Deep South vs. Rest of Thailand

	(1)	(2)	(3)	(4)	(5)	(6)
		Deep South			Rest of Thailand	
Dependent Variable	Lower Secondary Education	Years of Education (Total)	Completing at least Grade 9	Lower Secondary Education	Years of Education (Total)	Completing at least Grade 9
Linear	0.103***	0.697*	0.116***	0.054	0.39*	0.078***
	(0.000)	(0.051)	(0.004)	(0.164)	(0.066)	(0.000)
Observations	876	888	888	2066	2122	2122

Panel B: Rural vs. Urban Males in Northeast region

	(1)	(2)	(3)	(4)	(5)	(6)
	Rural Males		Urban Males		All Males	
Dependent Variable	Lower Secondary Education	College Education	Lower Secondary Education	College Education	Lower Secondary Education	College Education
Treat	-0.0632	0.0339	-0.00341	-0.097**	-0.0331	-0.0241
	(0.154)	(0.247)	(0.951	(0.016)	(0.113)	(0.279)
Treat*Northeast	0.183**	-0.0157	0.0156	0.0794**	0.1000*	0.025
	(0.016)	(0.566)	(0.758)	(0.010)	(0.056)	(0.381)
Treat*West and North	0.0875**	0.0262	0.0718	-0.0302	0.081**	-0.0008
	(0.038)	(0.277)	(0.319)	(0.576)	(0.031)	(0.979)
Observations	1416	1416	1111	1111	2527	2527

Note: These coefficients provide the effect of the policy on females in the Deep South region (defined as the three southernmost provinces) vs. the rest of Thailand. p-values in parentheses. Standard errors are clustered at the cohort level.
*** Significant at 1% level, ** significant at 5% level, * significant at 10% level.Note: The regressions are estimated for males using a 12-month donut hole and 60-month window. p-values in parentheses. Standard errors are clustered at the cohort level. The base for the region dummy variables is the southern.
*** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

corresponding effects on marriage and fertility are more prominent for Muslim young women. The persistent effects on Muslim women can be seen as a function of the type of education acquired by this group. There is suggestive evidence that Muslim women undertake college education more than their counterparts (and not an increase in vocational education) indicating a sustained effect on their educational and long-term human capital outcomes. As far as education can bring female empowerment and in conjunction with robust labor market opportunities, increasing female education acts as a factor in changing social norms and decreasing fertility. These results go on to illustrate the importance of the longer-term effect of education in certain socioeconomic groups.

We note some limitations to this study, given the available data.

First, this analysis is representative only at the province level and not of Thai population in general since the MICS data only covers 14 vulnerable provinces. These provinces are socioeconomically inferior to the rest of the country, so our results may not extrapolate to all the Thai population in general. Nonetheless, our results should still be applicable to regions or countries similar to our sample's. Second, we are not able to observe the total fertility or eventual marriage outcomes as the women in our effective sample are aged between 20 and 30 years old at the time of the survey.

This study adds to the literature showing the importance of compulsory schooling laws in changing the outcomes for women in a developing country context. The analysis provides evidence on important measures such as marriage and childbearing and details the possible mechanisms in observing these effects. Given the links between marriage and delayed childbearing on various aspects of the society including family structure, child health, poverty, women's labor force participation, gender wage gap, and other economic outcomes, the compulsory schooling law would have important implications on the economic welfare of women in these provinces in Thailand.

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Data and codes

The analysis uses publicly available MICS data from Thailand. The cleaned data and codes can be provided on request.

Declaration of Competing Interest

No conflicts to declare.

Data availability

Data will be made available on request.

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Appendix

Fig. A1, Fig. A2, Fig. A3, Fig. A4, Fig. A5, Fig. A6, Fig. A7, Fig. A8, Fig. A9, Table A1, Table A2, Table A3, Table A4, Table A5, Table A6

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[34] The percentages are calculated from numbers of students in each year. These numbers include repeaters as well as students who might be under- or over-aged, so the percentages can be above 100%. This could be the reason why enrollment in Grade 1 started out around 111% in 1999 and slightly declined after the policy as the admission age was clearly stated in the law.

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