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Abstract

This paper estimates the impact of the extension of compulsory schooling from 5 to 8 years in Turkey—which increased women's schooling by more than a year—on marriage and birth outcomes of teenage women, using regression discontinuity design, where we compare month-year of birth cohorts of all women. We find very strong incarceration effects of the new policy; the increased compulsory schooling years reduce the probability of teenage marriage by age 16 and first-births by age 17 substantially. However, these effects are short-lived; they dissapear after age 17 for marriage and after age 18 for first-births because the policy increases the marriage hazard rate at age 17, shortly after these women leave school, and there is no policy effect on the time to first-birth after marriage. These findings indicate either small or no human capital effects of the policy on marriage and first-birth decisions.

JEL classification: J12, J13, I21, I28, D10.

Keywords: Teenage marriage; births; education; compulsory schooling; regression-discontinuity; month-year of birth.

1. Introduction

Teenage marriage and births remain at significant levels in Turkey. According to the 2013 Turkish Demographic and Health Survey (TDHS), of the population of 25- to 49-year-old women, 41 percent were married by age 20, 22 percent were married by age 18, and four percent were married by age 15. The percentage having their first-birth in their teenage years is also high: 26 percent gave birth to their first child by age 20. Adolescent marriage is widespread in other parts of the world as well. Among women of 20-24 years of age, the percentage of those marrying before age 18 was 75 in Niger, 66 in Bangladesh, 56 in Mozambique, 47 in India, and 41 in Nicaragua and Ethiopia, according to the most recent Demographic and Health Survey (DHS) statistics for these countries (International Center for Research on Women). There is also a strong correlation between teenage marriage and education in several developing countries. In Turkey, among women aged 25 to 49 in 2013, while the median age of marriage was 18.9 for those with no school degree, it was 24.6 for those with a high school diploma or above. However, does this imply that an increase in women's educational attainment decreases teenage marriage and births?

In this study, we examine the effects of an increase in women's educational attainment—through the implementation of a longer compulsory schooling duration policy—on age at marriage, age at first-birth, and the time to first-birth after marriage. We also investigate the channels through which the new compulsory schooling policy affects teenage marriage and births. One channel is the incarceration effect (also called the institution effect), which takes place because schooling and marriage are incompatible events. The second channel is the human capital effect, which refers to the effects of education through its effects on wages, preferences, and knowledge.

For this purpose, we use a major education reform in Turkey in 1997 that extended compulsory schooling from 5 to 8 years. An important distinguishing characteristic of our study is the strength of the exogenous variation in schooling due to the length of the extension of compulsory schooling in Turkey (an additional three years) and the high percentage of children whose behavior is affected. In fact, the number of students in grades 1 to 8 increased by 15 percent (from 9 million to 10.5 million) in the three-year period following the implementation of the policy, compared to a 1-percent decline in the preceding three-year period (Turkish Ministry of Education, 2011). Kırdar et al. (2015) report that the policy increases women's completed years of schooling by more than a year, part of which resulted from spillover effects of the policy on high school completion.

In many developing countries, teenage marriage and births are associated with several adverse outcomes, including poorer mother and child health (see, for instance, Alam [2000] for Bangladesh, Raj et al. [2009, 2010] for India), worse educational outcomes (see, for instance, Lloyd and Mensch [2008] for sub-Saharan Africa), and a higher probability of domestic violence (UNICEF, 2005). There is also evidence for causal effects of early marriage; for instance, Field and Ambrus (2008) find a positive effect of delayed age at marriage on years of schooling in Bangladesh. In Turkey, for mothers younger than 20, the neonatal mortality rate was almost twice as high and the perinatal mortality rate was nearly three times that for mothers aged 20 to 29 during the period from 1998 to 2008 (2008 TDHS). In a study conducted in eastern Turkey, where teenage marriage is especially common, Edirne et al. (2010) find that teenage mothers have less education and are more likely to face domestic violence, and their births have a higher risk of preterm delivery and low birth weight.[1]

[1] There is also substantial evidence for adverse implications of teenage marriage and childbearing in developed countries, where the focus is on the implications of childbearing. This literature goes beyond establishing associations and uncovers adverse causal effects of teenage motherhood on women's education (Levine and Painter, 2003; Holmlund, 2005), labor market outcomes (Klepinger et al., 1999; Chevalier and Viitanen, 2003;

In conservative countries, where giving birth out of wedlock is socially condemned and therefore rare, mandating that teens stay in school for a longer time is a potentially important intervention on childbearing because longer schooling delays the entry of girls into the marriage market. An important characteristic of the transition to motherhood of women in Turkey is that almost all are married at the time of giving birth. The sociology literature reports a rigid sequence of events for completion of education, marriage and the birth of the first child in other countries as well (Blossfeld and De Rose, 1992; Marini, 1984). A key fact in this sequence of events is that the gap between the age at marriage and first-birth is quite narrow in Turkey. Given this narrow gap and the fact that marriage and schooling are generally incompatible events, a change in the timing of marriage due to increased schooling would directly translate into a change in the timing of fertility.

The data in this study come from the 2008 and 2013 Turkish Demographic and Health Surveys, which are nationally representative and contain detailed information on transitions to marriage and fertility. Our sample includes both ever-married and never-married women. The structure of the data is suitable for a regression discontinuity design (RDD) in estimating the effects of the compulsory schooling policy. We use the variation in policy exposure across month-year of birth cohorts as well as year of birth cohorts, although the analysis with month-year of birth is based on 2013 data only (as month-year of birth information is available only for ever-married women in 2008). In the estimation, we experiment with alternative time windows by gradually taking narrower intervals around the discontinuity. We also allow for

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Fletcher and Wolfe, 2009), and health outcomes (Webbink et al., 2008). On the other hand, studies that investigate the causal relationship between adolescent fertility and child health outcomes report mixed results (Rosenzweig and Wolpin, 1995; Wolpin, 1997). Adverse intergenerational effects of early childbearing are also reported (Francesconi, 2008; Hunt, 2006). At the macroeconomic level, İyigün (2000) shows that early childbearing may lead to a development trap with low human capital. There are also studies that report negative economic and health implications of teenage marriage in developed countries (Dahl, 2010; Le Strat et al., 2011).

very flexible functional forms for the running variable (up to sixth order polynomials), as well as split time trends before and after the discontinuity. In addition, we conduct a battery of robustness checks, including falsification checks.

Our results indicate that the extension of compulsory schooling reduces the probabilities of teenage marriage and births—but only upto a certain age. The probability of marriage by age 16 and, as a result of the rigid sequence of marriage and childbearing, the probability of giving birth by age 17 are both reduced by about 50 percent. However, the policy also increases the probability of marriage at age 17 conditional on not getting married until that age. Due to this *catching-up effect* shortly after the girls are out of school, the probability of being ever-married reverts to its pre-policy level after age 17. We find no evidence of a policy effect on the time to first-birth after a woman is married. Consequently, the policy effect on ever giving birth also disappears after age 18. In essence, we find strong evidence for the incarceration effect, whereas the human capital effect is either very small or non-existent.

The outline of the paper is as follows. Section 2 places our study in the relevant literature. Section 3 provides background information on marriage and fertility behavior as well as the education system in Turkey, and Section 4 outlines the conceptual framework for this study. Data and descriptive statistics are given in Section 5, and the identification strategy and estimation are explained in Section 6. Section 7 presents the results, while Section 8 provides a discussion of key findings. Section 9 concludes the paper.

2. Relevant Literature

2.1. Relevant Literature in the International Context

A number of quasi-experimental studies investigate the causal link between education and marriage as well as fertility. The quasi-experimental literature includes settings in both developed and developing countries. While Black et al. (2008) and Skirbekk et al. (2004)

explore the effects of education policies on marriage and birth outcomes (intention-to-treat effects) as we do, others estimate the effect of education on certain marriage and fertility outcomes using IV methodology. In terms of marriage outcomes, Lefgren and McIntyre (2006) find that education has no effect on whether a woman in the US ever gets married; on the other hand, examining the timing of marriage, Breierova and Duflo (2004) and Skirbekk et al. (2004) find that education delays the age at marriage in Indonesia and Sweden, respectively. Using structural estimation, Brien and Lillard (1994) also find that education increases the age at marriage in Malaysia.

In terms of fertility outcomes, Amin and Behrman (2011), Breierova and Duflo (2004), Cygan-Rehm and Maeder (2013), and Osili and Long (2007) report that the number of children decreases with education in the US, Indonesia, Germany, and Nigeria, respectively, using IV methodology. However, Fort et al. (2011) find the opposite result exploiting compulsory schooling reforms in 8 European countries as sources of exogenous variation in education. For Arabs in Israel, Lavy and Zablotsky (2015) find that education decreases the total number of live births but has no effect on age at marriage. Skirbekk et al. and Monstad et al. (2008) for Norway and Amin and Behrman for the US report that age at first birth is delayed by education. Behrman (2015) assesses the effect of schooling on women's desired fertility across three East African countries and measures a negative impact. Some studies focus exclusively on teenage fertility. For instance, Black et al. (2008) for the US and Norway, Silles (2011) for Great Britain and Ireland, Ozier (2011) for the US, Grönqvist and Hall (2013) for Sweden, and DeCicca and Krashinsky (2015) for Canada show that teenage pregnancy is reduced by education, whereas McCrary and Royer (2011) find no such effect in the US.[2]

Our examination of teenage births is most similar to that of Black et al. (2008), who also estimate the causal impact of changes in compulsory schooling laws on teenage births. However, there are significant contextual differences between the teenage fertility setting of Black et al. (for Norway and the US) and ours. In Turkey, marriage is virtually a necessary condition for teenage fertility; therefore, unlike Black et al., we study the transition to marriage along with the transition to motherhood. For the same reason, the channels through which compulsory schooling could affect teenage births are different in our context. In fact, we examine whether any change in the time to first-birth is caused by a change in the time to marriage or in the time until first-birth after marriage (or both).

A key distinguishing feature of our study is that we examine the policy effect not only on the probability of ever giving birth by age (as Black et al. do) but also on the hazard rate of first-birth by age so that we can separate the level and timing effects of the policy. In fact, this allows us to uncover a key finding: the policy increases the hazard rate for marriage at age 17 and the hazard rate for first-birth at age 18, counterbalancing the negative effects on the

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[2] Some experimental studies have examined the effects on teenage marriage and childbearing of programs aiming to retain girls in school. For instance, Baird et al. (2010), evaluating a cash transfer program to girls in Malawi, find a drop in teenage marriage and pregnancy one year after the program for participants who were out of school at the baseline, but not for schoolgirls. In another study in the same setting, Baird et al. (2011) do not detect any effect of a conditional cash transfer policy on marriage because the policy effect on school enrollment is small; however, they find a strong negative effect of an unconditional cash transfer policy on marriage because of the income effect on the large group of school dropouts. Duflo et al. (2006) evaluate the effects of a program that reduces schooling costs in Kenya by providing free school uniforms conditional on school attendance, along with two other inventions, and find that both teenage marriage and childbearing rates go down.

hazard rates at earlier ages and thereby bringing back the probability of marriage by age 17 and the probability of birth by age 18 to pre-policy levels.

In terms of methodology, many of the studies reviewed above use either difference-in-differences methodology or instrumental variables. We use instead a regression discontinuity design exploiting the variation in policy exposure across month-year of birth cohorts. In fact, Lee and Lemieux (2010) argue that RDD is potentially more credible than other quasi-experimental approaches. Lee (2008) shows that there is no need to assume that the treatment variable is "as good as" randomly assigned in RDD because of invidivuals' imprecise control over the assigment variable. In this sense, Lee argues that RDD is much closer to randomized experiments (the "gold standard" of program evaluation methods) than other quasi-experimental methods. McCrary and Royer (2011), Ozier (2015), and Silles (2011) all use RDD to examine the effect of schooling on teenage fertility. Of these, the identification strategies in McCrary and Royer (2011) and in Silles (2011) are based on a comparison of birth cohorts. However, Silles compares cohorts only at year of birth rather than month-year of birth level. While McCrary and Royer (2011) compare cohorts at month-year of birth level as we do, their sample is a select group of mothers, whereas we have a sample of all women.

2.2. Relevant Literature in the Turkish Context

Kırdar et al. (2009) is the first study to examine the causal impact of schooling on marriage and birth outcomes in the Turkish context. The data they employ come from the 2003 and 2008 THDS. This study, by also using the 2013 TDHS (which has only recently became avialable), significantly improves on Kırdar et al. (2009) in a number of dimensions. The 2013 TDHS allows us to separate the policy effect from time trends much more convincingly for the following reasons. First, we use month-year as well as year of birth as the running variable so that we can zoom in around the cut-off. (Since month-year of birth information is available only for the ever-married sample in the 2008 THDS, it was not used in Kırdar et al.

[2009].) Second, data points on the right-hand-side of the cut-off increase when we use the 2013 THDS. Consequently, the results of this paper are much more robust to the functional form of the running variable and split trends around each side of the cut-off. Moreover, our estimates are much more precise.

Kırdar et al. (2009) was followed by Dincer et al. (2014) and Gunes (2015), which also examine the effect of schooling on marriage and birth outcomes in the Turkish context using the same policy change.[3] Our paper substantially differs from the latter two papers in terms of content, methodology, and results. In terms of content, the key differences of our paper are that it separates the level (quantum) effects of the policy from the timing (tempo) effects and it disentangles the human-capital and incarceration effects of increased schooling on marriage and birth outcomes.

Our results are also starkly different from the results of these two papers in important ways. Gunes (2015) claims that the policy effect on fertility persists beyond age 18, in fact, up to age 22; while Dincer et al. (2014) find that a 10-percentage point increase in the proportion completing 8 years of schooling decreases the number of children between 0.11 and 0.16—for women between ages 18 to 22. We, however, find no policy effect on the number of children born to a woman beyond age 18—although we find an effect by age 18. This difference, we assert, stems from our superior identification methodology.

[3] There is a growing literature that examines the effects of the extension of compulsory schooling on various outcomes in Turkey. Early examples of this literature include Kırdar et al. (2009) and Yüret (2009). More recent studies such as Aydemir and Kırdar (2013), Mocan (2013), and Torun (2015) estimate the returns to schooling, using the change in the law as an instrument. While Cesur et al. (2014) examine women's health outcomes, Cesur and Mocan (2013) and Güleşçi and Meyersson (2013) study changes in religiosity and lifestyles with the extension of secular compulsory schooling.

In terms of methodology, Dincer et al. uses an instrument based on geographical variation in policy intensity. However, in the Turkish context, the policy intensity is far from being "as good as" randomly assigned across locations. Hence, an instrumental variable generated through geographical variation is not likely to be valid. On the other hand, Gunes (2015) uses a difference-in-differences methodology. However, the trends on marriage and birth outcomes across regions exhibit substantial variation in Turkey (where level differences across regions are also substantial); thus, the common trend assumption in the difference-in-difference methodology simply does not hold in this context. Our paper goes well beyond these papers in terms of identification strategy and its internal validity by using RDD as well as by utilizing richer data. In fact, Lee and Lemieux (2010) show that RD design generates local variation that is "as good as" randomly assigned.

3. Background Information

3.1. Marriage and Fertility

Despite the rise in non-marital unions in the West, marriage as a social institution remains strong in Turkey. Almost all women marry by age 49 and 98 percent give birth at least once. Moreover, divorce is an unlikely event, with its rate estimated at less than 2 percent among women of 15-49 years of age (TDHS-2013). Hence, one can say that, for an average woman in Turkey, marriage is for life. Giving birth out of wedlock is even rarer; according to TDHS-2013 data, almost all births are given by married women. Age at first-birth is also closely linked to age at marriage; the lapse of time between marriage and first-birth is on average 1.6 years. The average fertility rate currently stands at 2.26 births per woman (TDHS-2013).

Marriage occurs early in life. The median age at first marriage is 21.0 years in TDHS-2013. However, age at marriage has been increasing in Turkey. According to TDHS-2013, while the median age for marriage is 20.2 for the 45-49 age group, it is 22.0 for the 25-29 age group; in fact, from 1993 to 2013, the median age at marriage has increased by almost two years. Despite the rise over time in age at marriage, the incidence of teenage marriage remains high. Of the women aged 20-24 in TDHS-2013, 29 percent are married before age 20 and 15 percent are married before age 18.

As a result of their early marriage, Turkish women give their first birth at an early age as well. According to the TDHS-2013, the median age at first-birth for 25- to 49-year-old women is 22.9. Age at first-birth has also increased over time with the rising age at marriage. In TDHS-2013, while the median age at first-birth is 22.0 for women aged 45-49, it is 24.0 for women aged 25-29. Nonetheless, the incidence of first-birth during teenage years remains high even among the younger generations. Of the women aged 20-24 in TDHS-2013, 17 percent had given birth before age 20 and 6 percent before age 18.

The Civil Code prohibits marriage before age 17; however, a marriage can still happen through a religious ceremony before this age. Indeed, 2.5 percent of 16-year-old women in 2013 were married, for instance. Many couples have both a religious and a civil marriage ceremony; however, the percentage of women who have a religious marriage only is significant, especially among teenage women. The TDHS-1993 and TDHS-2013 data indicate that the percentage of 15- to 19-year-old married women who had only a religious marriage ceremony was 33 percent in 1993, and dropped only to 22 percent by 2013.

3.2. New Compulsory Schooling Policy

Before the change in the basic education law, the education system in Turkey consisted of five years of primary, three years of lower secondary, and three years of high school. The first tier was compulsory. In the summer of 1997, the Ministry of National Education (MONE) increased compulsory education from five to eight years by combining the first two tiers through amendments made to the Basic Education Law (no. 4306, dated 16 August 1997). The new law covered all children who did not already hold a primary school diploma at the beginning of the 1997-98 school year, without making exceptions for any group—although non-compliance was common both before and after the policy.

Although improving the enrollment at the lower secondary level was a long-established goal, the exact timing of the implementation of the new policy had to do with the political developments of that time. The secular government that came to power in 1997 wanted to prevent children from enrolling in religious schools at an early age. This policy reform would delay this by three years. It is important to note that the timing of the policy was unexpected and its impact on schooling outcomes was realized in a short period of time.

Before the enactment of the law, the net school enrollment rate was 89.4 percent in five-year compulsory schooling, 52.8 percent in lower secondary, and 38.5 percent in upper secondary schooling (Turkish Statistical Institute, 2012). Figure A1 in the Appendix displays how the number of students in grades 1 to 8 changed over the school years for urban and rural areas. From the 1997-98 school year to the 2000-01 school year, the number of students in grades 1-8 in urban areas increased by 13.7 percent compared with a 1.8-percent increase in the preceding three years, and the number of students in grades 1-8 in rural areas increased by 20 percent compared with a 7-percent fall in the preceding three years. Kırdar et al. (2015) report significant spillover effects to high school grade levels; in fact, the number of students in high school in urban areas increased by 27 percent from the 2000-01 school year to the 2003-04 school year, compared with a 10.5-percent rise in the previous three-year period.

To accommodate this substantial increase in the number of students, the government implemented a number of initiatives such as expanding the number of classrooms in existing schools, bussing an additional half a million (mostly rural) children to nearby schools, and constructing almost 600 boarding schools in more remote areas (Kırdar et al. [2015]). Accordingly, the share of MONE in the public investment budget soared from 15 percent in 1997 to 37.3 percent in 1998. As a result of this substantial investment in schooling infrastructure, certain measures of schooling quality such as student-to-teacher and student-to-classroom ratios remained relatively constant (Kırdar et al. [2015]). In fact, examining the results of the 1999 and 2007 TIMMS international test scores, Aydemir and Kırdar (2015) show that there is no deterioration in schooling quality, at least in mathematics and science, relative to other countries.

Since the new law covered children who had completed grade four or lower at the end of the 1996-97 school year, compulsory schooling became eight years for all students who had started the first grade as of September 1993, but it was five years for those who had started earlier. (There is virtually no grade repetition at this schooling level in Turkey.) The education law stipulates that children start school in the September of the year in which they complete their 72nd month (*Resmi Gazete*, no. 21308, dated 7 August 1992). Therefore, children born in or after January 1987 were affected by the policy. However, the treatment status of the birth cohorts right around the point of discontinuity is fuzzy. This is mainly because not all children start school at age six in Turkey. Children who were born before January 1987 but started school later than age 6, which is frequent in Turkey, were bound by the policy. On the contrary, children who were born after January 1987 but started school earlier than the normal school start age (and thus had a primary school diploma by the time of the policy) were exempt from the policy. Besides, although the Ministry of Education claims that enforcement of the reform was immediate, the establishment of the required infrastructure might have been delayed by a school-year in certain areas, in which case some children among the 1987 birth-cohort would not be affected by the policy.

4. Conceptual Framework

Our conceptual framework is centered on marriage models, particularly those that explain the timing of marriage, because the timing of first-birth in Turkey is largely determined by the timing of marriage. The most prominent theories in this area are the marriage model of Becker (1973, 1991) and the search models of Keeley (1977, 1979) and Oppenheimer (1988).

Becker's theory of the marriage market is structured around the specialization hypothesis, according to which marriage occurs because the total welfare of the husband and the wife is higher in the married state than the sum of their individual welfares in the single state (due to specialization of each partner in activities in which they have a comparative advantage, typically market work for husband and household work for wife). According to this theory, the gains to specialization decrease with the increasing education of women. However, in our context, education level increases for both men and women; therefore, the effect on the gains to specialization is not obvious. Keeley (1977) uses the main features of Becker's theory within a search model to explain the age at marriage. Within this framework, the age of entry into the marriage market and the duration of search for a partner determine the age at marriage, where the duration of search depends on the gains and costs of searching. For women, a higher level of education could decrease the search costs for a partner (by increasing ability in and, therefore, efficiency of the search process), which would increase the duration of the search. Moreover, with the increase in education level and thus the earnings of younger men, the gains from waiting and searching are further increased for women. On the other hand, in Oppenheimer's theory, the critical aspect that determines the age at marriage for both men and women – in a traditional setting where women are not in the labor force – is the time at which the uncertainty surrounding men's career paths is resolved.

Examining the earlier work in both the economics and the sociology literature, Brüderl and Diekmann (1997) discuss two main channels through which education influences marriage within the specialization hypothesis: the institution effect and the human capital effect. According to the institution effect, schooling and marriage are incompatible events. Thornton et al. (1995) point out three main reasons for this: i) students are not prepared for adult roles,

ii) school requires a lot of time, iii) financial independence is needed for marriage. This argument is consistent with Becker's specialization hypothesis in that the gains from specialization cannot be realized until after schooling is completed: women would not have sufficient time for home production and men would not have sufficient income. The institution effect is also referred to as the "incarceration effect" in the economics literature.

According to the human capital effect, even after schooling is completed, increased education has a bearing on marriage choices. A particular channel through which this happens is market work. With increased schooling, the opportunity cost of marriage and raising children increases for women. This is the channel that is emphasized in the work of Becker and in the economics literature in general. Another channel for the human capital effect is a change in marriage preferences with higher schooling levels. Axinn and Barber (2001) discuss "ideational theories", which explain how schooling changes fertility preferences. An example is that schooling could increase consumption aspirations, thus increasing the costs of childbearing and decreasing the demand for children. Similar arguments could be applied to marriage; hence, a longer education period could change marriage preferences.

Education affects fertility decisions through a number of channels.[4] First, the effect of education on marriage translates into an effect on fertility in the rigid sequence of marriage and fertility decisions in Turkey, as well as in several other countries. However, there are additional human capital effects of education on fertility. First, a higher level of education for women increases the opportunity cost of raising children by increasing their market wage rate

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[4] Glewwe (2002) reviews the literature on the relationship between schooling and marriage and fertility in developing countries.

(Willis, 1973). This would decrease the desired number of children.[5] In addition, Rosenzweig and Schultz (1989) show that education increases the effective contraceptive use of women. Education can provide better knowledge of contraceptive methods via curricula or develop the ability to acquire information about them. The second channel was formalized by Grossman (1972), for which Glewwe (1999) finds empirical evidence in Morocco. Finally, as claimed by Mason (1986), higher bargaining power in fertility decisions for more educated women is another possible channel through which education influences fertility decisions.

5. Data and Descriptive Statistics

The data we employ come from the 2008 and 2013 rounds of the Turkish Demographic and Health Survey (TDHS) of Hacettepe University of Turkey, which is nationally representative. The target population in TDHS surveys is women at the reproductive age (15-49). The sample includes both ever-married and never-married women. The surveys include information on the timing of marriage and the timing of first-birth for ever-married women, as well as information on schooling attainment in the form of the highest grade completed. These three pieces of information are the key outcome variables in this study. We also use information on location of birth in our analyses—both in terms of the area of residence (rural vs. urban) and the geographical region of residence (12 NUTS-1 level regions).

The 2013 survey includes information on the month as well as the year of birth for all women, whereas the 2008 survey includes this information only for ever-married women. Therefore, we use two separate samples. In the first, we pool the 2008 and 2013 surveys and define cohorts according to year of birth. In addition, we drop the 1986 and 1987 birth cohorts from

[5] This is on condition that the substitution effect dominates the income effect (higher demand for children due to a higher level of income). The income effect would be stronger if there is positive assortative mating; in other words, women who acquire more schooling marry more educated men with higher incomes.

the sample due to the fuzziness in the treatment status of these two birth-year cohorts right around the cut-off (as explained in Section 3.2) and call this gap the *bubble* around the cut-off in the rest of the paper.[6] Since the surveys draw among women aged 15-49, this sample includes women—ever married or never married—born between 1959 and 1998, except for the 1986 and 1987 birth cohorts. We call this sample (A). In the second sample, we use the 2013 survey only and define cohorts according to month-year of birth. We further disaggregate this sample according to the width of the time window taken around the bubble. Sample (B1) takes 4-year intervals around the bubble (1982-85 and 1988-91 birth cohorts), sample (B2) 7-year intervals around the bubble (1979-85 and 1988-94 birth-cohorts), and sample (B3) 10-year intervals (1976-85 and 1988-97 birth-cohorts)—again among both ever married and never married women. In samples (B1) to (B3), month of birth is an additional variable, unlike in sample (A). Basic descriptive statistics on person-level characteristics are displayed for sample (A) in Table A1 and for samples (B1) to (B3) in Table A2 of the Appendix. There are 20,552 women in sample (A), 2,325 in sample (B1), 4,066 in sample (B2), and 5,946 in sample (B3).

In the first part of our analysis of marriage and birth outcomes, we examine the effect of the policy on the level of marriage and first-birth outcomes by age. Using the information on age at marriage and age at first birth, we construct histories of ever-married status and ever-given-birth status until age 19 or until the current age at the time of the survey, whichever comes earlier. This means that the number of birth cohorts on the right-hand side of the discontinuity changes by age. For instance, all birth cohorts until 1998 contribute to the sample for age 15,

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[6] Note that this would not be needed if we estimated the impact of schooling—rather than the policy impact—because in that case we would be applying fuzzy RDD to our 'fuzzy' data. However, since we estimate the reduced form impact of the policy on potential outcomes, we use a sharp RDD; hence, we remove the fuzziness from our data based on an exogenous covariate. This issue is discussed further in Section 6.

whereas only birth cohorts until 1994 contribute to the sample for age 19 (as the latest survey is in 2013). Hence, the number of observations fall after age 15 with age in samples (A) and (B3) but not for samples (B1) and (B2) because both the 4-year and 7-year intervals around the bubble end before 1994.

In the second part of our analysis, where we examine the policy effects on the timing of marriage and first-birth using duration analysis, we construct event histories of time to marriage and time to first-birth. In this case, women enter the risk set of marriage at age 14 and the risk set of first-birth at age 15. Women who get married before age 14 are dropped from the marriage sample, and women who give birth before age 15 are dropped from the birth sample. Individuals exit the risk set when they get married in the first case and give their first-birth in the second case. The duration is censored in the right if women do not marry/give birth until age 19 or until their current age at the time of the survey, whichever comes earlier. Unlike the level analysis, where marriage and first-birth outcomes are examined at each age separately, the data are pooled over ages in the timing analysis. Basic descriptive statistics on person–age level characeristics in the survival samples (B1) to (B3) are given in Table A3 of the Appendix.

The effects of the education reform on three different schooling outcomes—eight-year basic education completion, high school completion, and average years of schooling—are illustrated in Figure 1, where panel (A) uses sample (A) with year of birth as the running variable and panel (B) uses sample (B2) with month-year of birth as the running variable. As can be seen from panels (A1) and (B1), there is a substantial increase in the proportion of women who complete the 8^{th} grade with the policy; this proportion jumps from about 40 percent before the policy to almost 70 percent after the policy. Another interesting feature of Figure 1 is the rise in the percentage of women who complete the 11^{th} grade (high school) following the policy implementation, displayed in panels (A2) and (B2). In other words, the new education policy seems to affect grade completion rates well beyond the new compulsory schooling levels. Panels (A3) and (B3) indicate an important jump in the total years of schooling between the 1985 and 1988 birth cohorts. In fact, the total years of schooling increases by more than a year with the policy.

Changes in the percentage of women who are ever married at each age from 14 to 19 are displayed in Figure 2 for the year of birth cohorts in sample (A) and in Figure 3 for the month-year of birth cohorts in sample (B2). The discontinuity in the percentage of ever married at the time of the policy is visible at ages 14, 15, and 16 in both figures. On the other hand, the graphs suggest no discontinuity in the percentage of ever married at or above age 17. Similarly, the way the percentage of women who have ever given birth at each age from 14 to 19 changes is given in Figure 4 for the year of birth cohorts in sample (A) and in Figure 5 for the month-year of birth cohorts in sample (B2). In this case, the jumps at the cut-off are more visible at ages 15, 16, and 17. Given the fact that the gap between the time of first-birth and the time of marriage is just over a year, the discontinuities in Figures 3 and 5 are consistent with the discontinuities in Figures 2 and 4, respectively. There are also strong time trends in Figures 2 to 5, but especially in Figures 2 and 4 with a wider time window. Therefore, in the identification strategy, explained in the next section, it will be critical to disentangle the policy effects on marriage and birth outcomes from these secular time trends.

6. Identification Strategy and Estimation

We use the variation in the years of compulsory schooling across different month-year of birth cohorts as well as that across different year of birth cohorts, to identify the causal impact of the new compulsory schooling policy on marriage and first-birth decisions. The structure of our data, illustrated in Figures 1 to 5, fits a regression-discontinuity design, as there is a discontinuous jump in the outcome variables at the time of the policy, and the relationship between the outcome variables and the covariate that determines the timing of the jump (running variable) is continuous. Our data-generating process can be written as

$$E(Y_{0i} \mid x_i) = f_0(x_i^{'}), \tag{1}$$

$$E(Y_{1i} \mid x_i) = f_1(x_i^{'}) + \rho, \tag{2}$$

$$x_i^{'} = x_i - x_0, \tag{3}$$

where Y_0 and Y_1 are the outcome variables before and after the policy, respectively. The running variable (year of birth or month-year of birth) is denoted by x, which is normalized using the time of discontinuity (x_0). The relationships between the outcome variable and x before and after the discontinuity are denoted by $f_0(.)$ and $f_1(.)$, respectively. The policy effect on the outcome variables is denoted by ρ. Hence, we estimate the following model,

$$Y_i = f_0(x_i^{'})(1 - D_i) + f_1(x_i^{'})D_i + \rho D_i + \eta_i, \tag{4}$$

where D denotes the treatment variable, which is one when the running variable is above the threshold and zero otherwise. This threshold is January 1987. In equation (4), η is the error term.[7] While we take split time trends before and after the cut-off as in equation (4) in certain specifications, in others we take a single time trend by imposing $f_0(.) = f_1(.) = f(.)$. In addition, control variables for place and month of birth are added to equation (4) in certain specifications.

[7] In this study, we estimate the effect of the compulsory schooling policy rather than use it as an instrument for education. The compulsory schooling policy in this study makes substantial changes in both women's and men's distribution of education. This certainly alters both the marriage market and labor market opportunities for women. As a result, the exclusion restriction assumption—that the policy affects marriage and childbearing outcomes of a woman through the change in her education only—is likely to be violated.

A key assumption of RDD is that the variation in the treatment variable is as good as randomized around the cut-off. This requires that individuals not manipulate their birth date to be on one particular side of the cut-off. In our context, since month-year of birth is determined well before the announcement of the policy, such a manipulation is not possible. Another critical assumption is that the relationship between the outcome variables and the running variable is continuous except at the cut-off. We check this assumption using placebo tests. First, we examine any potential effects of the policy on baseline covariates at the cut-off. Second, we search for discontinuities in the relationship between the outcome variable and the running variable away from the cut-off. The results of these checks are given in Section 7.4.

A critical aspect of our identification strategy is to distinguish the discontinuous jump from the smooth function f(.) or the split functions $f_0(.), f_1(.)$. For this reason, we assess the robustness of our identification strategy to various choices. First, we take various time windows, as explained in the Data Section, which is akin to the optimal bandwidth choice in the non-parametric RDD literature. Second, we take split polynomials on both sides of the cut-off, as well as a single polynomial throughout the time window. Third, we take very flexible polynomial speficiations to disentangle the time trends from the policy effect properly. The order of polynomials taken depends on the time window, whether or not the time trends are different before and after the cut-off, and whether the running variable is month-year of birth or year of birth. With sample (A), where the running variable is year of birth, the order of single and split polynomials goes up to four and two, respectively. When the running varable is month-year of birth, the order of single and split polynomials goes up to two and one, respectively, with sample (B1); up to four and two, respectively, with sample (B2); and up to six and three, respectively, with sample (B3). We take lower orders of polynomials when the time window is narrower because the risk of misspecification of the functional form is lower in that case. As argued by van der Klauuw (2008, p. 235) in his review of RDD, "A linear control function is likely to provide a reasonable approximation of the true functional form within a small neighborhood of the cut-off."[8]

Using month-year of birth as the running variable instead of year of birth imposes a weaker identification assumption. When we use year of birth as the running variable with sample (A), we compare birth cohorts that are three years apart around the cut-off. However, with month-year of birth as the running variable with samples (B1) to (B3), we compare birth cohorts that are 25 months apart around the cut-off. In fact, we further narrow this interval in certain robustness checks. Another advantage of month-year of birth as the running variable is that it allows the specification of more flexible functional forms for the running variable (due to the higher number of data points on each side of the cut-off). On the other hand, the advantage of using sample (A) is the higher number of observations, which becomes especially important at lower ages when the incidence of marriage and birth events is low.

The exclusion of the 1986 and 1987 birth cohorts for which the treatment status is fuzzy is critical in this study because we estimate policy effects. (If we did not exclude these cohorts, we would seriously underestimate the policy effect.) Were we to estimate, instead, the effect of education on marriage/first-birth using a 2SLS analysis, this exclusion would not be needed because in that case—within a Wald-estimator interpretation—both the numerator on the policy effect on marriage/first-birth and the denominator on the policy effect on education would be adjusted by the fuzzy treatment in the same way. In fact, as shown by Hahn et al. [2001], the fuzzy regression discontinuity design—which is equivalent to a two-stage least squares estimation—addresses this problem using the random assignment of the instrumental

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[8] We also use the Akaike information criterion to examine the relative performance of alternative specifications. Lower-order polynomial specifications perform clearly better with 4-year intervals and generally better with 7-year intervals. At the same time, as we widen the time interval, the relative performance of high-order polynomials improves vis-à-vis the performance of low-order polynomials.

variable. Note that the use of the bubble requires that we extrapolate the time trends within the bubble until the cut-off point.

If the timing of the education reform were correlated with some unobserved characteristics that also affect marriage and fertility decisions, we would get biased estimates. For instance, if the policy change had come right after some shock that decreased school enrollment rates while increasing marriage and fertility, there would have been be a problem. In this sense, it is important to emphasize that the timing of the policy had to do with the political circumstances of the time (as discussed earlier).

Another potential concern with the analysis of the effects of a new policy is that it could coincide with other interventions that would influence the outcome of interest. This is especially the case if better-than-average economic conditions allow higher investment in public services on health, education, or social transfers. For this reason, we check for potential discontinuties in 1997 in a number of health (health expenditures per capita, infant and under-5 mortality rates), employment (employment rates of men and women aged 15 to 24), and growth outcomes (GDP per capita at ppp growth rate), as well as investment in education (education's share in public expenditure)—which is presented in Figure A2 of the Appendix. While there is a tremendous jump in the share of education in public expenditure, there are no visible discontinuities in the other outcomes. This is probably not surprising, as the timing of the policy was independent of economic conditions, as indicated above.[9] An exception to the absence of other interventions that could influence marriage and birth outcomes is the change in the Civil Code in 2002, whose effect we examine, also using a RD design.

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[9] Two other notable educational interventions were implemented much later: the public conditional cash transfer (CCT) policy in 2003 and a NGO-driven CCT policy in 2005. Moreover, the effects of these policies would be trivial, due to the limited number of beneficiaries, compared to those of the compulsory schooling policy.

In the estimation of the policy effect on marriage and first-birth outcomes by age, we run equation (4) for three outcome variables: i) ever-married status, ii) ever-given-birth status, and iii) time to birth after marriage. All equations are estimated at each age separately, using logit regressions. We leave the inclusion of additional control variables as robustness checks because there is no variation in the outcome variable for some values of these variables in the regressions for lower ages. (In RDD, the inclusion of these variables would not matter for the estimated coefficients but it would matter for standard errors.) Standard errors are clustered at the level of birth-year or month-and-birth-year in accordance with the running variable because the policy variable does not exhibit variation across individuals within these clusters.

We use duration analysis to estimate the policy effect on the time to marriage and time to first-birth. Here, the analysis pools data from different ages within the same estimation procedure; therefore, a time index (for age) is introduced. We choose a logistic form for the hazard function (for marriage and first-birth), given by

$$\log\left[\frac{h_{it}}{1-h_{it}}\right] = b(t) + f_t(x_i) + \rho_t D_i, \tag{5}$$

where t denotes the waiting time concept—which is age, h_{it} is the discrete time hazard rate at time t, b(t) is the baseline hazard rate at time t. The baseline hazard function we choose is non-parametric: a piece-wise constant baseline hazard is used, so we have dummies for all ages. The parameters of the functional form relationship between the running variable and the log odds of the hazard ratio ($f_t(.)$) and the impact of the education policy (ρ_t) vary by age.

7. Results

In this section, we first examine the effects of the education policy on the schooling outcomes of teenage women, which bring about changes in marriage and birth outcomes. Then we present our findings on the policy effect on teenage marriage and births by age and on the policy effect on the marriage and first-birth hazard rates at each age. Finally, we present the results of a number of robustness checks.

7.1. Impact of the Education Policy on Schooling

Table 1 presents estimation results on the policy effect on three schooling outcomes for women: 8th grade completion in panel (A), 11th grade (high school) completion in panel (B), and years of schooling in panel (C). The first two binary outcomes are estimated using logistic regressions, whereas years of schooling is estimated using OLS regression. Because of the minimum-age restrictions imposed (age 18 in panel (B) and age 22 in panel (C)), we are unable to use 10-year intervals around the bubble in panel (B) and 7-year and 10-year intervals around the bubble in panel (C). (There are fewer data points on the right-hand side of the bubble than on the left-hand side in these cases.)

As can be seen in panel (A), there is strong evidence, statistically significant at the 1 percent level, that the new policy increases the completion probability of grade 8 for women. Quantitatively, the policy increases the odds of the 8th-grade completion rate by a factor of about two. In addition, as suggested by the graphical illustration in Figure 1, the policy in fact increases high school completion, which is not compulsory. Almost all coefficient estimates in panel (B) are statistically significant; and in the two cases where they are not, the coefficient magnitudes are very similar to those of the statistically significant coefficients. Panel (C) of Table 1 shows that the policy increases the years of schooling by about one year.[10] All coefficients are statistically significant at least at the 5 percent level, except for one case where the magnitude of the coefficient estimate is very similar to the others.[11]

7.2. Policy Effect on Marriage and First-Birth of Teenage Women

In this subsection, using logistic regressions we first present the policy effect on the odds of being ever married by age, and on the odds of ever giving birth by age for teenage women. Then to better gauge the magnitude of the policy effect, we present the effects on the predicted fractions of women ever married and of women ever given birth by age.

The estimation results on the effects of the education policy on being ever married and on ever giving birth are presented in Table 2 for samples (B1) and (B2) and in Table 3 for sample (B3), where the running variable is month-year of birth. The results for sample (A), where the running variable is year of birth, are given in Table A4 of the Appendix. In these tables, the results for being ever married are given in panel (A)s, and the results for ever giving birth are given in panel (B)s. In Tables 2 and 3, the minimum age for which results are presented is 13 for marriage and 15 for first-birth, whereas it is 12 for marriage and 13 for first-birth in Table A4 of the Appendix due to larger sample sizes.

As can be seen in panel (A) of Table 2, for both time windows and with all specifications, there is evidence that the education policy decreases the probability of being ever married by ages 15 and 16. Consistent with the rigid sequence of marriage and birth outcomes in Turkey discussed earlier, there is also evidence across all specifications in panel (B) that the

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[10] When no bubble is taken, the estimated policy impact on years of schooling is about 0.75 years; i.e., the policy effect on years of schooling falls by more than 25 percent due to the fuzziness of treatment status right around the cut-off. This is the very reason we conduct our analysis with a bubble around the cut-off.

[11] Kırdar et al. (2015) examine the policy effect on the probability of completing each grade level from grade 5 to 11 by gender and rural/urban status. Their findings are consistent with the findings here.

probabiliy of ever giving birth by ages 16 and 17 decreases with the policy. The results in Table 3, with the wider time window, confirm these findings. However, statistical significance is in general lower in Table 3, especially for birth outcomes in panel (B). Note that the coefficients of the specifications with higher-order polynomials in Table 3, where the time window is wider, are more consistent with the coefficients in Table 2.[12]

Tables 2 and 3 also show that the policy effect on being ever-married completely dissappears by age 17. Similarly, in the rigid sequence of marriage and fertility, the policy effect on ever giving birth disappears by age 18. In fact, the estimated odds ratios for marriage by age 17 and for birth by age 18 are close to one; thus, the lack of evidence of a policy effect in both cases is not due to high standard errors. In addition, Tables 2 and 3 indicate that the estimated effects on marriage by ages 13 and 14 and on first-birth by age 15 are also substantially lower than one; however, they are generally statistically insignificant due to the lower frequency of marriage events at these ages.

The results for sample (A), presented in Table A4 of the Appendix, are also consistent with those in Tables 2 and 3. An important difference is that the coefficients for marriage by age 14 and for giving birth by age 15 are also statistically significant. This presumably stems from the larger size of sample (A). Table A4 also shows that the estimated effects on marriage by ages 12 and 13 are even greater in magnitude than those by ages 14 to 16; however, they are statistically insignificant with higher order polynomials due to the lower frequency of marriage events at these ages. A similar finding holds for giving birth by age 14 in panel (B).

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[12] According to the Akaike information criterion, either the first-order single or the first-order split polynomials are the best models in both panels (A) and (B) with 4-year intervals, except for one age; single or split polynomials with orders up to two are the best models in both panels with 7-year intervals, except for one age; whereas fifth- and sixth-order single polynomials outperform lower orders in many cases in both panels with 10-year intervals.

The estimates for both marriage and birth outcomes by ages 18 and 19 with sample (A) are less stable across specifications compared to those in Tables 2 and 3 because the data points on the right-hand side of the cut-off are even fewer at these higher age levels with sample (A), which becomes especially problematic with split time trends.[13]

Overall, the results are quite consistent across different time windows and running variables. At the same time, the estimates in Table 2—for both sample (B1) and sample (B2)—are more stable across the various specifications than those in Table 3 and Table A4. Therefore, in quantifying the effects of the 1997 education reform, we refer to the estimates in Table 2. Based on these estimates, we calculate the baseline and policy values of the predicted probabilities of being ever married and ever giving birth by age for the cohort born between December 1986 and January 1987 (the 1044.5 month-year of birth cohort). We present these in Table 4 for the specifications with split linear time trends. The magnitude of the policy effect on the probability of being ever married at ages 15 and 16 is striking; using sample (B2), the probability of marriage by age 15 decreases by 3.8 percentage points, from 6.5 to 2.7 percent, and that by age 16 drops by 5.7 percentage points, from 11.9 to 6.2 percent. In terms of percentage changes, the fall in the probability of marriage by age 15 is 58.5 percent, and that by age 16 is 47.9 percent. Similarly, the policy effect on first-birth outcomes is also quite remarkable. According to the analysis with sample (B2), the probability of giving birth by age 16 decreases by 2.1 percentage points, from 3.4 to 1.3 percent, and the probability of giving birth by age 17 decreases by 4.6 percentage points, from 8.1 to 3.5 percent. While the percentage drop in giving birth by age 15 is 61.8 percent, it is 56.8 percent by age 17.

[13] We also reduce sample (A) to 10-year intervals and 5-year intervals around the bubble and repeat our estimations with these samples. The results, presented in Tables A5 and A6 of the Appendix, are very consistent with those in Table A4 of the Appendix.

7.2.1. Impact of the Education Policy on the Time to First-Birth after Marriage

The drop in the percentage of women who give birth at young ages can be attributed to two different mechanisms. First, it could be brought about by a change in the age at marriage and, in fact, we illustrate above that the percentage of women who get married at young ages decreases with the policy. Second, as a result of the higher education levels caused by the education policy, married women could be delaying the birth of their first child. In this subsection, we tackle this question: after a woman is married, does the longer compulsory schooling increase the time to first-birth?

In this analysis, the sample includes only ever-married women, and the dependent variable is time to first-birth after marriage in months. The sample here is restricted to women who are 17 or older because, as shown in the previous section, the policy changes the group of ever-married women before age 17.[14] Table 5 presents the OLS estimation results in two different panels. In panel (A), age at marriage is also controlled for, as this becomes a state variable here; in panel (B), the results without controlling for age at marriage (due to concerns about the endogeneity of age at marriage) are presented. Within each panel, the results for our four samples are presented separately. The estimates are quite similar across the two panels. The data present no evidence of a policy effect on the time to first-birth after marriage with any sample. This finding is important because, unlike the time to marriage and time to first-birth analyses, in the time to first-birth after marriage analysis we know for certain that women are out of school in the rigid sequence of schooling, marriage, and fertility in Turkey.

[14] Figure A3 of the Appendix presents the time to first birth after marriage in number of months across the birth cohorts in our sample. This figure does not indicate any discontinuity. Note that the number of months is lower for the youngest birth cohorts because here the sample includes mostly those who chose to marry early.

7.2.2. Policy Effect on Number of Children Ever Born

Here, we examine the policy effect on the number of children ever born to a woman. Using the information on the date of birth of all children, we construct histories of the number of children ever born to a woman in person-age format from age 15 to her current age in the sample, where age is in years and months. Then, we estimate the policy effect on the number of children ever born at selected values of women's age—at 12-month increments from 180 to 240 months; i.e., from age 15 to 20.

The policy effect on the number of children ever born is presented in Table 6 by women's age for samples (B1) and (B2). (Table A7 in the Appendix gives it for sample (B3).) For all time windows and with all specifications, there is strong evidence that the education policy decreases the number of children born to a woman by age 18. According to the estimates in Table 6 for 4-year intervals and for 7-year intervals with higher-order polynomials, the policy decreases the number of children born to an 18-year-old woman by about 0.07 to 0.08. However, this effect is very short-lived; unlike what Dincer et al. (2014) and Gunes (2015) claim, there is no evidence of a policy effect on the number of children born to a 20-year-old woman. This lack of significance is not due to large standard errors; the coefficient estimates are very close to zero (especially in comparison to the estimates for age 18).

7.3. Survival Analysis: Policy Effect on the Time to Marriage and Time to First-Birth

The analysis in Section 7.2 uncovers the policy effect on marriage and first-birth outcomes by age—which cumulates the age-specific effects over age. For instance, the policy effect on marriage by age 16 depends on the policy effect on the probability of marriage at each age conditional on not being married until that age (marriage hazard rate) by age 16. Even if the policy has no effect on the marriage hazard rate at age 16, it could have an effect on marriage by age 16. In this section, using duration analysis, we estimate the policy effect on the timing of marriage and first-birth, thereby uncovering age-specific policy effects.

The estimates for the policy effect on the marriage and first-birth hazard rates are presented in Table 7 for samples (B1) and (B2) and in Table 8 for sample (B3).[15] The estimates for the marriage hazard rate, given in panel (A) of each of these tables, are quite stable over the time windows and the specifications. The key and very interesting finding here is that the policy in fact increases the marriage hazard rate at age 17. (This is statistically significant at the 1 percent level in almost all specifications in both tables.) Moreover, the magnitude of the effect is substantial. With more flexible specifications—the right-most columns, given the time-window and whether or not the time trend is split—the estimates show that the policy increases the odds of the marriage hazard by a factor of more than four. The policy effects on the marriage hazard rate in earlier ages (ages 14 to 16) are all negative, apart from those for age 16 with the least flexible specifications and wide time-window in Table 8. However, the negative effect is statistically significant only for age 15 in both tables. The negative effect is not as strong in magnitude at age 16; while the negative effect is strong in magnitude at age 14, it is statistically insignificant, presumably due to the low incidence of marriage at this age. There is no evidence of a policy effect on the marriage hazard rates at ages 18 or 19.

When we examine the policy effect on the first-birth hazard rate, given in panel (B) of Tables 7 and 8, we see evidence for a positive policy effect on the hazard rate at age 18 in both tables. According to the estimates in Table 7, the policy increases the first-birth hazard rate at age 18 by a factor more than 2. In addition, the odds ratios for age 19 in both tables are substantially higher than one, suggesting a positive policy effect at this age as well. However, its statistical significance is not robust across different specifications. At earlier ages, 15 to 17, the odds ratios are all less than one in both Tables 7 and 8. However, the results on statistical

[15] The results for sample (A) are given in Appendix Table A8. While the findings are similar, statistical significance is generally lower—particularly for specifications with split time trends.

significance are mixed. With sample (B1) in Table 7, there is evidence of a negative policy effect on the first-birth hazard rate at ages 15 and 16, but not age 17. On the other hand, with sample (B2) in Table 7, there is strong evidence of a negative policy effect on the first-birth hazard rate at age 17 but weaker evidence for ages 15 and 16. Also with the more flexible specifications estimated with sample (B3) in Table 8, there is evidence of a negative policy effect on the first-birth hazard rate at ages 16 and 17 but no robust evidence exists at age 15.

In summary, while the marriage hazard rate at ages 15 and below—when students are incarcerated in school—decreases with the policy, once the students are out of school at age 17, they are more likely to get married as a result of the policy. Consequently, after age 17, the probability of being ever married reverts to its baseline (no policy) levels as shown in Tables 2 and 3. Consistent with the rigid sequence of marriage and birth events, while the policy decreases the first-birth hazard rate at ages 15 to 17, it increases it after age 18. As a result, the probability of ever giving birth by age 18 reverts to its pre-policy level.

7.4. Robustness Checks

In this section, we first check the assumption that the relationship between the outcome variables and the running variable is continuous except at the cut-off. Second, we conduct falsification checks in which we examine the effects of policies that did not happen. Third, we check the robustness of our findings to the inclusion of other control variables. Fourth, we inspect the sensitivity of our findings to the length of the bubble around the cut-off. Finally, we examine the potential effects of the change in Civil Code in 2002.

To check the continuity assumption, we first search for discontinuities in the relationship between the outcome variables and the running variable away from the cut-off. For this purpose, we first restrict the sample to individuals who are not affected by the policy, by keeping in the sample only those who are born in 1985 or earlier. Then, we take the 7-year intervals around a two-year bubble (as in the original analysis) that ends in 1985. Hence, this sample includes the 1970 to 1985 birth cohorts excluding the 1977 and 1978 birth cohorts. The threshold for the treatment variable is January 1978 in this case. Table A9 of the Appendix, which provides the results from running equation (4) on this RDD, indicates no discontinuities. Since this falsification test is conducted at a certain cut-off point, we repeat this exercise with alternative cut-off points by sliding the time window to the left incrementally by two years. Tables A10, A11, and A12 of the Appendix provide the results for the 1968-83, 1966-81, and 1964-79 time windows, respectively. Overall, these tables do not indicate any obvious discontinuities, either.[16] As a second check of the continuity assumption, we examine whether the policy has an effect on other covariates at the cut-off. These covariates are the rural dummy and dummies for 12 NUTS-1 level regions. The results, given in Table A13 of the Appendix, indicate no evidence of a policy effect on these covariates.

Our second robustness check concerns testing the effects of education policies that did not take place. For this purpose, we define counterfactual policies by sliding the timing of the policy over time and estimate equation (4) using these counterfactual policy dummies. The actual timing of the policy is slid incrementally by 4 months up to a maximum of 20 months in each direction. Tables 9 and 10 present the results for the policy effect on marriage and first-birth by age and on marriage and first-birth hazard rates at each age, respectively. These results are given for three cases: sample (B1) with split linear polynomials, sample (B2) with split quadratic polynomials, and sample (B3) with single quartic polynomials. The results in Table 9 (Table 10) include the ages for which there is evidence of a policy effect in Tables 2 and 3 (Tables 7 and 8).

[16] The only recurring evidence for a discontinuity across the tables is for the marriage outcome at age 13 in panel I-A, which is likely to stem from the very low probability of the event at this age.

In both Tables 9 and 10, the policy effect gradually diminishes and the odds ratios gradually approach one, as expected, as we move away from the discontinuity. In fact, when we move the actual timing of the policy by more than a year on either side, there is no evidence of a policy effect in either table. At the same time, the policy effect persists in some cases when the threshold of the counterfactual policy is not very distant from that of the actual. This occurs because, with a small perturbation, most of the points on the right-hand side are still treated and most on the left untreated. A similar analysis is conducted with sample (A), where counterfactuals are generated by sliding the timing of the policy by a year incrementally. The results, given Table A14 of the Appendix, confirm the above findings.

In the third robustness check, we check the sensitivity of our findings to the inclusion of other control variables when the running variable is month-year of birth. First, we add month of birth dummies to our level regressions (given in Table A15 for samples (B1) and (B2) and in Table A16 for sample (B3)) and to our timing regressions (given in Table A17 for samples (B1) and (B2) and Table A18 for sample (B3)). Second, in addition to the month of birth dummies, we also add dummies for place of birth in terms of 12 NUTS-1 regions and rural/urban status to our level regressions regressions (given in Table A19 for samples (B1) and (B2) and in Table A20 for sample (B3)) and to our timing regressions (given in Table A21 for samples (B1) and (B2) and Table A22 for sample (B3)). The inclusion of these additional control variables makes very little difference; not only do the qualitative findings hold but the magnitudes of the coefficients are also very similar.

Finally, we examine the potential effects of the 2002 change in Civil Code. Prior to 2002, the Civil Code stipulated age 15 as the minimum age for marriage for women in Turkey. With a law that went into effect on 1 January 2002, the legal age of marriage was increased to 17 years for women.[17] Nonetheless, as explained in Section 3.1, a non-trivial percentage of women marry before the legal age through religious ceremonies. For instance, according to the 2008 TDHS, 3.84 percent of 16-year-old women were already married. Moreover, a significant percentage of women remained only with a religious marriage even after reaching the legal age of marriage. The percentage of 15- to 19-year-old married women who had only a religious marriage ceremony was 33 percent in 1993, and dropped only to 29 percent by 2008 (TDHS data). Therefore, we might not expect the Civil Code to be binding on marriage decisions. Nonetheless, we conduct an empirical examination of this issue.

An individual born in December 1984 is not affected by the 2002 Civil Code because she was already 17 years old when the policy went into effect on January 1 of that year. Those born after January 1985 are affected, but the duration for which they are affected depends on their birth date. For instance, somebody born in January 1985 was affected only for a month because she was not allowed to get married in January 2002 but was able to marry in February 2002 (when she completed her 17th year). In the same manner, somebody born in January 1986 was affected for a duration of 13 months, and somebody born in December 1986 was affected for a duration of 24 months.

We limit our analysis to individuals born before January 1987 to ensure that the individuals in our sample were not affected by the education policy. As explained above, the 1985 and 1986 birth cohorts were affected by the new Civil Code to varying degrees. Our sample also includes the 1983 and 1984 birth cohorts who were not affected by the Civil Code. We conduct a RDD analysis using this time window (1983-1986), month-year of birth as the running variable, and January 1985 as the cut-off. This time, however, the key variable of

[17] Under unusual circumstances such as pregnancy and with parental consent and a court decree, a 16-year-old is allowed to get married after the policy and a 14-year-old before the policy.

interest is not a dummy variable for the policy but a variable for the duration of exposure to the new policy in months (which varies from 1 to 24).

The estimation results, presented in Table A23 of the Appendix, suggest there might be small effects of the 2002 Civil Code on the marriage hazard rate at age 17. However, this result is not robust across specifications. While there is evidence of a positive policy effect on the marriage hazard rate at age 17 with single polynomials of various orders, there is no evidence with split polynomials. In addition, even when there is evidence of an effect, the magnitudes of the effects are tiny (the highest odds ratio is around 1.3) compared to the estimated effects of the education policy on the marriage hazard rate at age 17 given in Tables 7 and 8 (which are around 3-4). Furthermore, this effect might be partly capturing the effect of the education policy because the 1986 birth-cohort is partly affected. Hence, we conclude that there is either no or a small effect of the Civil Code on the marriage hazard rate at age 17, and even if such an effect exists, its magnitude is very small compared to the effect of the education policy. There is no evidence of an effect of the Civil Code on the marriage hazard rate at other ages.

8. Discussion

In the conceptual framework discussion in Section 4, we identified two main channels through which education affects marriage and, therefore, birth outcomes: the incarceration effect and the human capital effect. Here we interpret our key findings within this framework.

We find a very strong incarceration effect of the education policy on teenage marriage and births. The probability of marriage drops substantially during the ages that girls are mandated to stay in school with the new education policy. At the same time, our findings show that the effect of the new education policy on teenage marriage and births persists for a few years beyond the new compulsory schooling years. While most children would complete grade 8 by age 14, we find that the effect of the policy on marriage persists until age 16.

If there was only an incarceration effect of the policy on marriage, the women who delay their marriage because of the policy, which would otherwise happen at grade levels 6 to 8 (ages 12 to 14), would get married once the new compulsory schooling years are over. This 'catching-up effect' would mean that the hazard rate of marriage at ages 15 and 16 would be higher with the policy and, thus, the probability of marriage by age 15 or 16 would revert to its original level. However, this is not observed; the policy effect on the probability of marriage persists by age 16. This could suggest that a human capital effect is in play. Black et al. (2008) find that the effect of compulsory schooling on teenage fertility in the US and in Norway also persists beyond the years the children are forced to stay in school and interpret this as evidence for human capital effect.

However, our analysis also reveals the marriage hazard rate at age 17 is higher with the policy than it would be in the absence of it; and, consequently, the probability of marriage reverts to its original level by this age. Therefore, even if there were a human capital effect, its effect is short-lived. Furthermore, this short-lived policy effect on marriage—between the end of newly mandated schooling years (which corresponds to age 14) and age 16—might not be brought about by a human capital effect because the effect of the new education policy on grade completion persists well beyond the new compulsory schooling years. In fact, high school graduation rates (high school graduation usually takes place at age 17) increase remarkably with the policy. Therefore, the policy effect on marriage and first-birth beyond the new compulsory schooling years may still result from an incarceration effect.

In addition, we find no evidence of a human capital effect on the time to first-birth after marriage. Here, we can make a definitive statement about the lack of evidence of a human capital effect; an increase in the time to first-birth after marriage could not be a result of the incarceration effect of the policy because in the rigid sequence of schooling, marriage, and fertility in Turkey, all married women are out of school. In essence, we conclude that there is either a small or no human capital effect of the education policy on marriage and first-birth decisions of teenage women in Turkey.

Our estimated magnitude of the effect of the compulsory schooling policy on teenage fertility is much greater than those reported in developed countries. For instance, one of the largest effects Black et al. (2008) find is that the policy of mandating women to stay in school until age 17 reduces the probability of birth before age 19 by 8.8 percent in the US. In comparison, we find that mandating women in Turkey to stay in school until completing grade 8 (roughly age 14) reduces the probability of giving birth by age 17 by a striking 50 percent. In Western societies, teenage fertility may not necessarily be planned, whereas in Turkey, it is clearly planned along with marriage. Thus, schooling puts a very strong brake on teenage fertility in Turkey by preventing marriage, whereas it causes only a deceleration in the US and Norway by decreasing the chances of pregnancy. Nonetheless, similarly large effects are reported for other developing countries. For instance, Ozier (2015) reports that secondary school completion in Kenya also causes a near elimination of pregnancy among compliers by age 18.

Poverty is often pointed out as an important underlying factor for early marriage in several countries.[18] Within Keeley's (1979) search model, poverty can be considered as increasing search costs, thereby reducing the age at marriage. The new compulsory schooling policy in Turkey decreased the monetary costs of school attendance in grades 6 to 8 because the accessibility of schools increased substantially. The government had to either provide schools that included these grade levels even in sparsely-populated areas or transport the students in these areas to a school on a daily basis. This decline in the costs of school attendance would obviously increase the opportunity cost of marriage.

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[18] For instance, UNICEF (2005) reports for Senegal that girls in the poorest 20^{th} percentile of households are more than 4 times as likely to be married than those in the wealthiest 20^{th} percentile of households.

Previous studies on early marriage in Turkey point out cultural factors, rather than economic factors, as the key driving elements. For instance, Edirne et al. (2010) find that the parents of teenage mothers have less education and are more likely to follow traditional matrimonies, but do not have lower household income than other parents. In their qualitative study in eastern Turkey, Ertem and Koçtürk (2008) highlight the importance of "protecting family honor" as a reason for marriage once girls reach the age of menarche. If, in fact, cultural traditions are the main driving factor of early marriage and fertility, this study shows that it is not impossible to alter the course of these cultural traditions, at least in Turkey.

9. Conclusion

In this paper, we estimate the impact of the extension of compulsory schooling from 5 years to 8 in Turkey on the marriage and fertility decisions of teenage women. We find that the rise in years of compulsory schooling indeed reduces the probability of marriage and giving birth for teenage women. Moreover, the magnitude of this effect is quite substantial. The proportion of women married by age 16 drops by 5.7 percentage points (48 percent), and the proportion of women who give birth by age 17 drops by 4.6 percentage points (57 percent) for the first birth-cohort affected by the policy. Nonetheless, shortly after these women are out of school, marriage and fertility hazard rates rebound. In fact, the marriage hazard rate at age 17 and the resulting fertility hazard rate at age 18 are higher with the policy than they would have been in the absence of it. Consequently, the policy effect on the probability of marriage by age 17 and the probability of first-birth by age 18 dissappear. In addition, we find no evidence for a policy effect on the time to first-birth after a woman is married. Therefore, we conclude that there is a very strong incarceration effect of the new compulsory schooling policy on marriage and, therefore, on first-birth in Turkey. On the other hand, there is either a short-lived human capital effect or none at all.

This study shows that increasing education by an extension of compulsory schooling years delays marriage and childbearing of teenage women in an upper-middle income (World Bank classification) developing country. What remains to be seen in the Turkish context is the effect of this policy on total fertility, as well as the spacing of births, which can be answered only after the initial cohorts affected by the policy complete their fecund period. Nevertheless, previous empirical literature shows that increasing the age at marriage and childbearing has important socioeconomic implications. Given the causal links between age at marriage and age at first-birth with other demographic outcomes (like divorce, the number and spacing of children), health outcomes (such as maternal and child mortality), and economic outcomes (labor force participation, migration, welfare take-up), as well as the inter-generational effects of early marriage and childbearing, the rise in age at marriage and age at first-birth would have important implications on the aggregate welfare.

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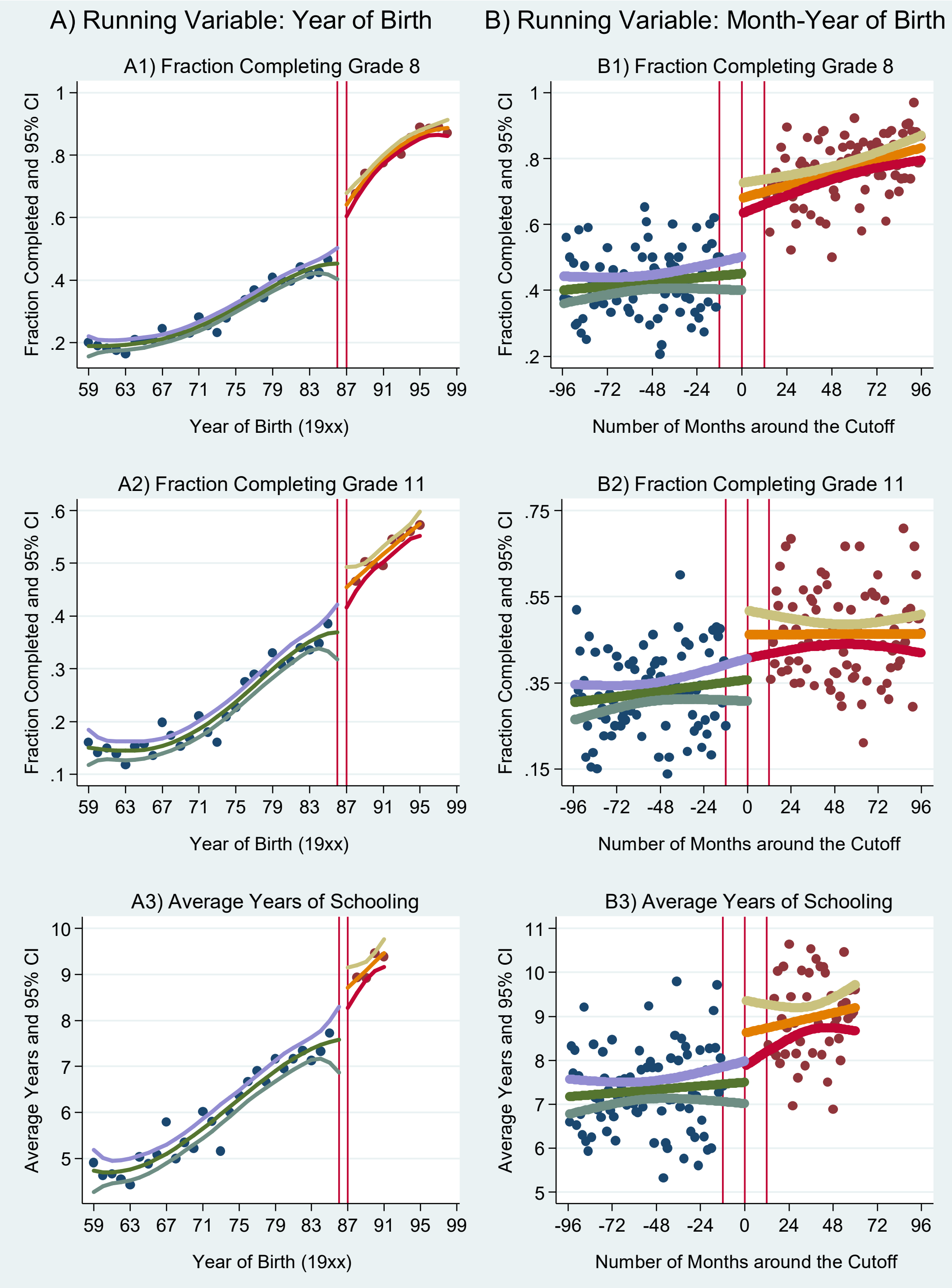
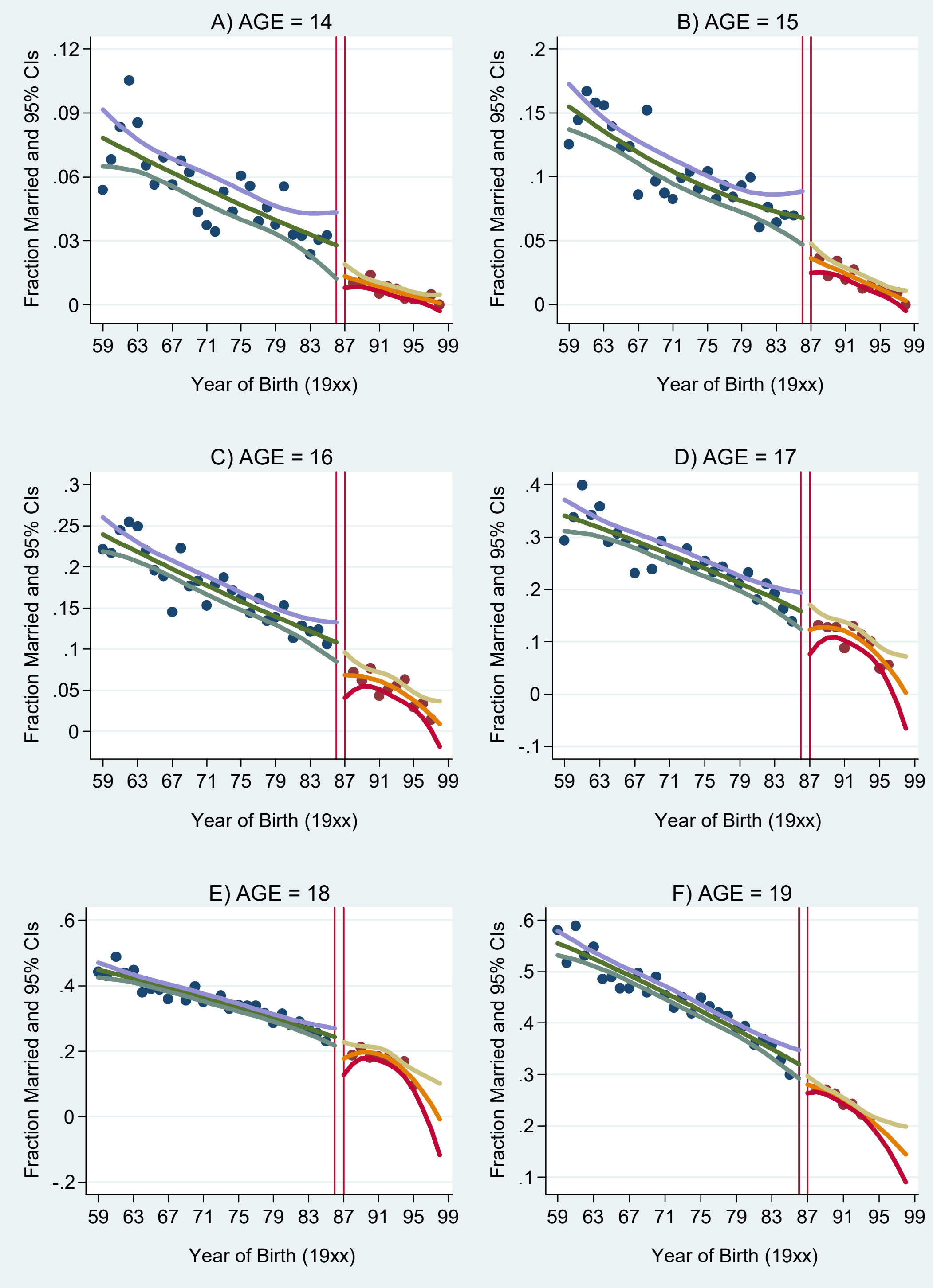
Figure 1: Policy Effect on Schooling Outcomes



Notes: a) The sample is drawn from both the 2008 and 2013 DHS in panel (A), but only from the 2013 DHS in panel (B).
b) In both panels, 1986 and 1987 birth cohorts are excluded due to the fuzziness of treatment status.
c) The sample includes individuals aged above 15 in panel A1, above 18 in panel A2, and above 22 in panel A3.
d) Quartic polynomials are fit before the cut-off in panel (A), whereas after the cut-off quadratic polynomials are fit in panels (A1) and (A2), but linear in panel (A3).
e) Linear polynomials are fit before and after the cut-off in panel (B).
f) In panels (A2), (A3), and (B3), the graphs are truncated on the right hand side due to higher age restrictions.

Figure 2: Fractions of Women Ever Married at Selected Ages by Year of Birth



Notes: a) The sample is drawn from both the 2008 and 2013 DHS.
b) In both panels, 1986 and 1987 birth cohorts are excluded due to the fuzziness of treatment status.
c) Quartic polynomials are fit before the cut-off, quadratic polynomials are fit after the cut-off.

Figure 3: Fraction of Women Ever Married at Selected Ages by Month-Year of Birth

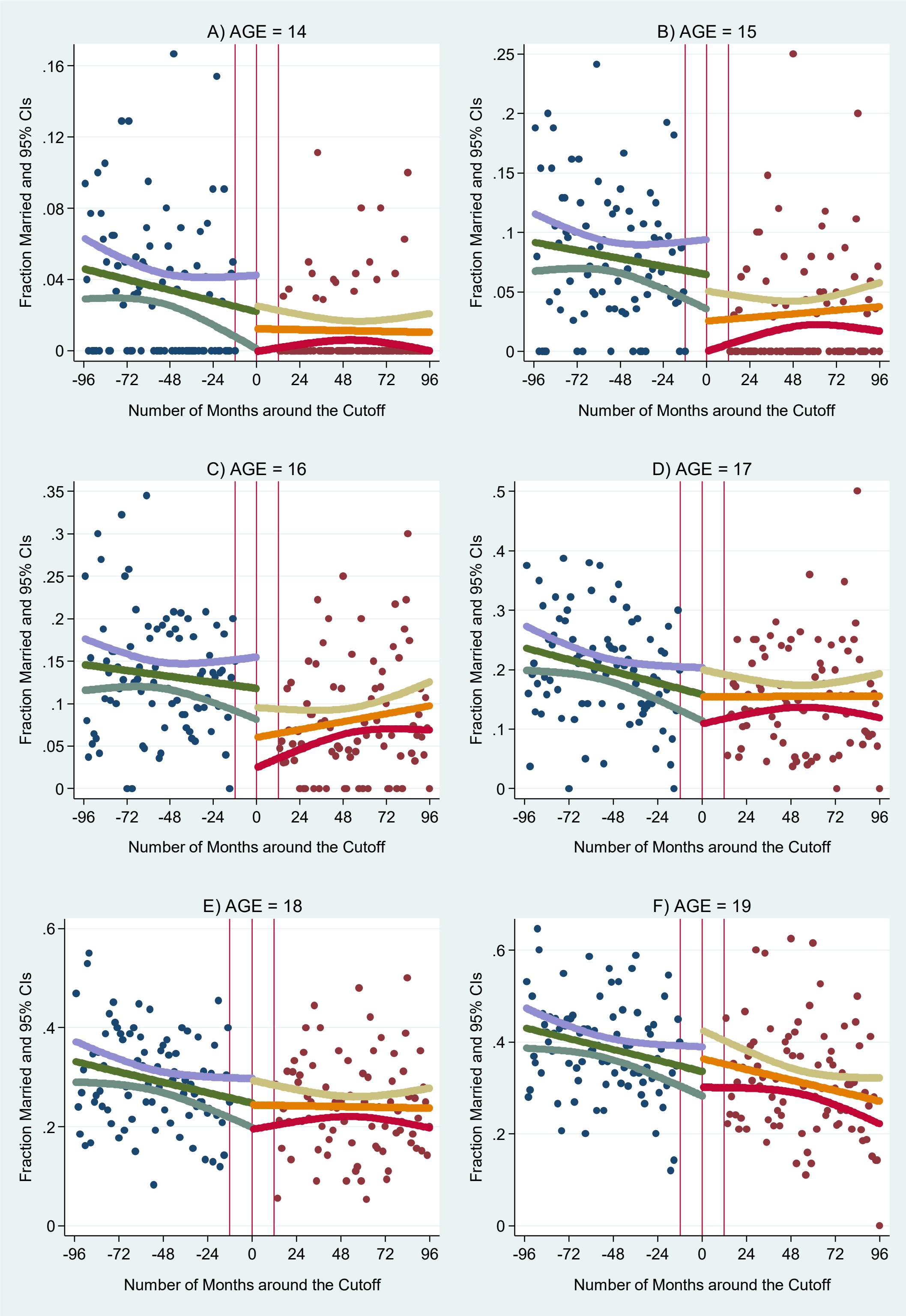
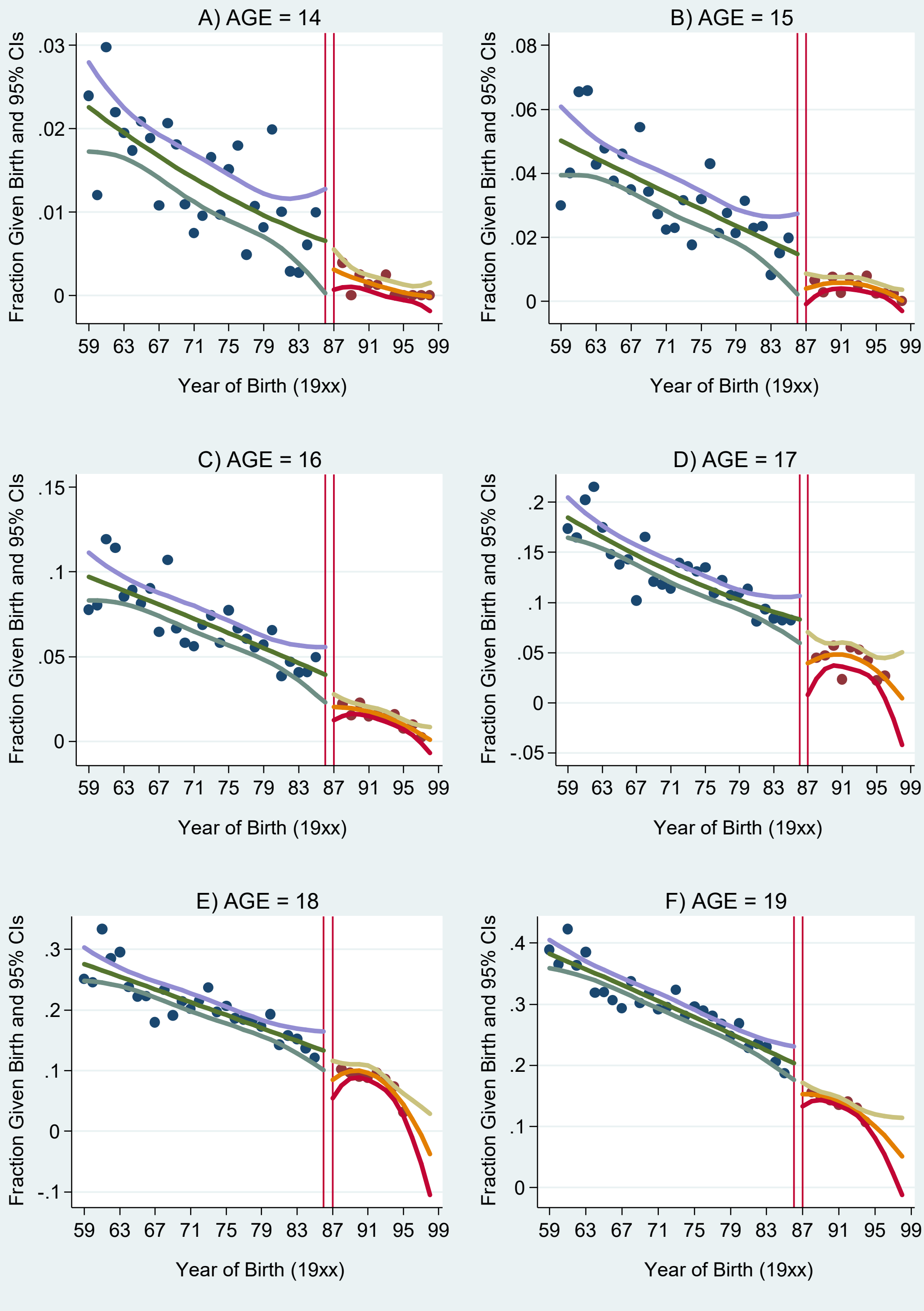


Figure 4: Fractions of Women Ever Given Birth at Selected Ages by Year of Birth



Notes: a) The sample is drawn from both the 2008 and 2013 DHS.
b) In both panels, 1986 and 1987 birth cohorts are excluded due to the fuzziness of treatment status.
c) Quartic polynomials are fit before the cut-off, quadratic polynomials are fit after the cut-off.

Figure 5: Fraction of Women Ever Given Birth at Selected Ages by Month-Year of Birth

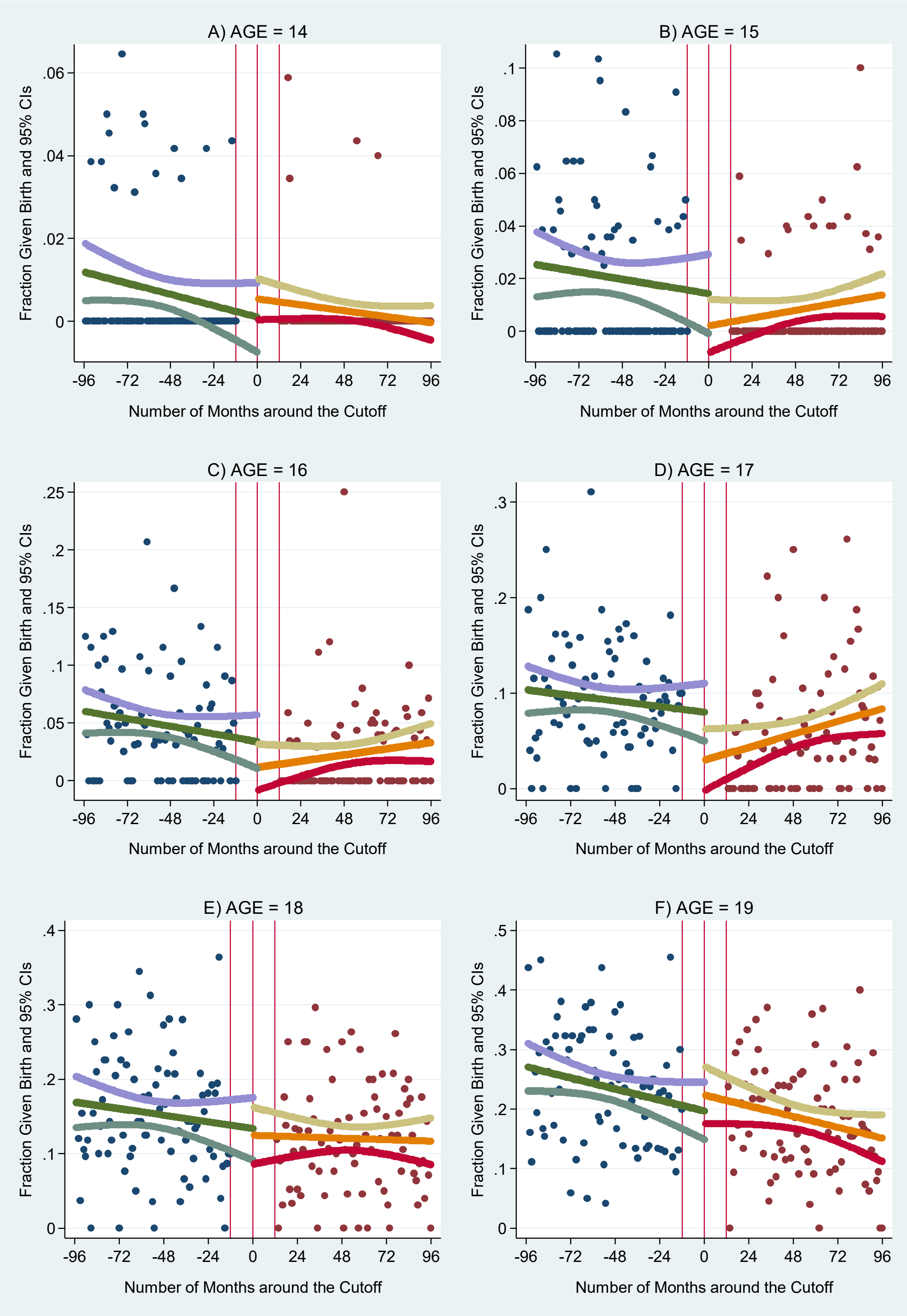


Table 1: Policy Effect on Schooling Outcomes

	Single Time Trend						Split Time Trends			
Degree of Polynomial	One (1)	Two (2)	Three (3)	Four (4)	Five (5)	Six (6)	One (7)	Two (8)	Three (9)	Obs.
A) Policy Effect on the Odds of Completing Grade 8 - Logistic Regression Results										
Year of Birth	2.806***	1.906***	1.909***	2.120***	--	--	2.033***	1.683***	--	21,845
1959-1998	[0.269]	[0.203]	[0.189]	[0.272]	--	--	[0.140]	[0.196]	--	
Month-Year of Birth	2.520***	2.331***	--	--	--	--	2.375***	--	--	2,325
4-year intervals	[0.603]	[0.526]	--	--	--	--	[0.537]	--	--	
Month-Year of Birth	2.590***	2.420***	2.439***	2.461***	--	--	2.442***	2.361***	--	4,065
7-year intervals	[0.447]	[0.397]	[0.613]	[0.622]	--	--	[0.399]	[0.740]	--	
Month-Year of Birth	2.114***	1.906***	2.702***	2.589***	2.657***	2.661***	1.921***	2.894***	2.284*	5,945
10-year intervals	[0.296]	[0.252]	[0.531]	[0.514]	[0.708]	[0.728]	[0.250]	[0.683]	[0.967]	
B) Policy Effect on the Odds of Completing Grade 11 - Logistic Regression Results										
Year of Birth	1.257***	1.131	1.167***	1.296***	--	--	1.274***	1.180**	--	19,321
1959-1995	[0.062]	[0.085]	[0.066]	[0.110]	--	--	[0.056]	[0.085]	--	
Month-Year of Birth	1.799**	1.791**	--	--	--	--	1.784**	--	--	2,325
4-year intervals	[0.472]	[0.464]	--	--	--	--	[0.462]	--	--	
Month-Year of Birth	1.500**	1.497**	1.691*	1.665*	--	--	1.493**	1.696	--	4,065
7-year intervals	[0.256]	[0.255]	[0.481]	[0.462]	--	--	[0.254]	[0.601]	--	
C) Policy Effect on Years of Schooling - OLS Regression Results										
Year of Birth	1.039***	0.708***	1.240***	1.030***	--	--	0.858***	0.915	--	16,448
1959-1991	[0.135]	[0.248]	[0.350]	[0.264]	--	--	[0.195]	[0.560]	--	
Month-Year of Birth	1.043*	1.024*	--	--	--	--	1.034*	--	--	2,293
4-year intervals	[0.530]	[0.528]	--	--	--	--	[0.528]	--	--	

Notes: The sample includes observations from both the 2008 and 2013 DHS in the analysis by year of birth, but only from the 2013 DHS in the analysis by month-year of birth. The sample is restricted to ages 15 and above in panel (A), to ages 18 and above in panel (B), and to ages 22 and above in panel (C). In panel (B), 10-year intervals on both sides of the cut-off cannot be taken due to the age restriction. Due to the same reason, in panel (C), neither 7-year intervals nor 10-year intervals can be taken. In all analyses, 1986 and 1987 birth cohorts are excluded. The policy dummy is one when year of birth is greater 1987. Each cell comes from a separate regresion of the specified schooling outcome on the policy dummy as well as the specified time trends. In panels (A) and (B), odds ratios and their standard errors are given, wheras in panel (C), OLS estimates are given. Standard errors are clustered at the year-of-birth level when the running variable is year of birth and at the month-year of birth level when the running variable is month-year of birth. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table 2: Policy Effect on Marriage and First-Birth by Age – Month-Year of Birth as Running Variable, 4-Year and 7-Year Intervals around the Bubble (Samples B1, B2)

	I) 4-Year Intervals			II) 7-Year Intervals					
	Single Time Trend		Split Trends	Single Time Trend				Split Time Trends	
Degree of Polynomial	One (1)	Two (2)	One (3)	One (4)	Two (5)	Three (6)	Four (7)	One (8)	Two (9)
A) Dependent Variable: Ever Married									
Age=13	0.451	0.412	0.385	0.883	1.066	0.812	0.293	0.971	0.194
	[0.478]	[0.458]	[0.445]	[0.697]	[0.774]	[0.943]	[0.416]	[0.708]	[0.371]
Age=14	0.356	0.288	0.282	0.777	0.775	0.439	0.175*	0.742	0.113*
	[0.299]	[0.263]	[0.259]	[0.451]	[0.457]	[0.381]	[0.183]	[0.436]	[0.148]
Age=15	0.321**	0.305**	0.285**	0.449**	0.406**	0.310**	0.232**	0.395**	0.159**
	[0.153]	[0.172]	[0.161]	[0.159]	[0.163]	[0.178]	[0.145]	[0.161]	[0.126]
Age=16	0.521**	0.494**	0.483**	0.540***	0.500***	0.518*	0.442**	0.492***	0.414*
	[0.167]	[0.174]	[0.167]	[0.128]	[0.125]	[0.198]	[0.167]	[0.124]	[0.192]
Age=17	1.359	1.313	1.318	1.043	1.026	1.252	1.158	1.021	1.284
	[0.406]	[0.397]	[0.397]	[0.204]	[0.199]	[0.422]	[0.390]	[0.198]	[0.571]
Age=18	1.128	1.119	1.120	1.069	1.061	1.218	1.195	1.061	1.211
	[0.297]	[0.297]	[0.295]	[0.184]	[0.181]	[0.353]	[0.345]	[0.182]	[0.443]
Age=19	1.132	1.124	1.125	1.130	1.133	0.993	0.961	1.130	0.956
	[0.307]	[0.309]	[0.307]	[0.194]	[0.195]	[0.290]	[0.282]	[0.194]	[0.362]
B) Dependent Variable: Ever Given Birth									
Age=15	0.392	0.254	0.289	0.526	0.330	0.580	0.340	0.313	0.427
	[0.398]	[0.333]	[0.371]	[0.297]	[0.228]	[0.664]	[0.479]	[0.232]	[0.733]
Age=16	0.323**	0.210**	0.200**	0.517	0.385**	0.371	0.241*	0.367**	0.164*
	[0.185]	[0.151]	[0.147]	[0.209]	[0.182]	[0.248]	[0.182]	[0.182]	[0.163]
Age=17	0.413**	0.330**	0.313**	0.481**	0.424***	0.335**	0.231***	0.408***	0.154***
	[0.160]	[0.149]	[0.143]	[0.140]	[0.139]	[0.159]	[0.123]	[0.138]	[0.102]
Age=18	1.000	0.968	0.964	0.926	0.922	0.975	0.890	0.914	0.915
	[0.336]	[0.331]	[0.329]	[0.208]	[0.208]	[0.367]	[0.339]	[0.208]	[0.442]
Age=19	1.236	1.236	1.233	1.190	1.197	1.219	1.157	1.191	1.185
	[0.345]	[0.351]	[0.348]	[0.219]	[0.220]	[0.388]	[0.369]	[0.220]	[0.484]

Notes: The data come from the 2013 DHS. The sample includes 48 months on each side of the bubble in panel (I) and 84 months on each side of the bubble in panel (II). The number of observations is 2,325 all cells in part (I), while it is 4,066 in all cells for ages up to 18 and 4,040 in all cells for age 19 in panel (II). The policy dummy is one when year of birth is greater than 1987. Each cell comes from a separate logistic regression of ever-married status in panel (A) and of ever-given-birth status in panel (B) at the specified age on the policy variable and the specified time trends in month and year of birth. Odds ratios and their standard errors are given as estimated parameters. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table 3: Policy Effect on Marriage and First-Birth by Age – Month-Year of Birth as Running Variable, 10-Year Intervals around the Bubble (Sample B3)

	Single Time Trend						Split Time Trends		
Degree of Polynomial	One (1)	Two (2)	Three (3)	Four (4)	Five (5)	Six (6)	One (7)	Two (8)	Three (9)
A) Dependent Variable: Ever Married									
Age=13	0.669	0.959	1.099	1.073	0.650	0.411	0.885	0.805	0.068
	[0.449]	[0.617]	[1.030]	[1.002]	[0.794]	[0.535]	[0.546]	[1.008]	[0.162]
Age=14	0.567	0.796	0.752	0.527	0.566	0.286	0.724	0.366	0.107
	[0.280]	[0.387]	[0.515]	[0.396]	[0.535]	[0.304]	[0.342]	[0.354]	[0.180]
Age=15	0.440***	0.559*	0.474*	0.323**	0.262**	0.184**	0.525**	0.228**	0.076**
	[0.130]	[0.177]	[0.195]	[0.163]	[0.151]	[0.124]	[0.167]	[0.142]	[0.082]
Age=16	0.720	0.827	0.517**	0.346***	0.407**	0.405**	0.791	0.264***	0.403
	[0.145]	[0.173]	[0.148]	[0.110]	[0.160]	[0.168]	[0.165]	[0.100]	[0.254]
Age=17	1.202	1.336*	1.172	0.823	1.096	1.114	1.271	0.790	1.456
	[0.193]	[0.217]	[0.292]	[0.216]	[0.384]	[0.419]	[0.204]	[0.258]	[0.885]
Age=18	1.222	1.328*	1.163	0.785	0.986	1.456	1.275	0.775	1.316
	[0.172]	[0.203]	[0.257]	[0.193]	[0.294]	[0.527]	[0.190]	[0.226]	[0.711]
Age=19	1.125	1.146	1.092	0.903	1.047	1.361	1.126	0.865	1.360
	[0.156]	[0.184]	[0.235]	[0.252]	[0.322]	[0.576]	[0.172]	[0.283]	[0.887]
B) Dependent Variable: Ever Given Birth									
Age=15	0.674	0.784	0.559	0.218*	0.452	0.306	0.710	0.124	0.718
	[0.320]	[0.369]	[0.358]	[0.192]	[0.523]	[0.432]	[0.338]	[0.169]	[1.555]
Age=16	0.602	0.693	0.540	0.234**	0.362	0.205**	0.630	0.122***	0.117
	[0.193]	[0.253]	[0.232]	[0.133]	[0.235]	[0.163]	[0.230]	[0.095]	[0.159]
Age=17	0.756	0.708	0.513**	0.213***	0.262***	0.191***	0.679	0.126***	0.072***
	[0.167]	[0.199]	[0.160]	[0.095]	[0.117]	[0.110]	[0.189]	[0.070]	[0.067]
Age=18	1.056	1.224	1.078	0.578*	0.761	1.196	1.136	0.507*	1.210
	[0.189]	[0.247]	[0.277]	[0.189]	[0.274]	[0.572]	[0.222]	[0.192]	[0.837]
Age=19	1.122	1.213	1.394	1.022	1.040	1.620	1.142	1.114	1.129
	[0.155]	[0.209]	[0.283]	[0.302]	[0.311]	[0.700]	[0.186]	[0.372]	[0.755]

Notes: The data come from the 2013 DHS. The sample includes 10 years on each side of the bubble. The number of observations is 5,946 in all cells for all ages up to 15, 5,921 for age 16, 5,589 for age 17, 5,273 for age 18, and 4,970 for age 19. The policy dummy is one when year of birth is greater than 1987. Each cell comes from a separate logistic regression of ever-married status in panel (A) and of ever-given-birth status in panel (B) at the specified age on the policy variable and the specified time trends in month and year of birth. Odds ratios and their standard errors are given as estimated parameters. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table 4: Policy Effect on Predicted Fractions of Being Ever Married and Ever Giving Birth by Age

	I) 4-Year Intervals				II) 7-Year Intervals			
Age	Baseline	Policy	Diff.	% Fall	Baseline	Policy	Diff.	% Fall
A) Ever Married								
13	0.010	0.004	0.006	60.0	0.010	0.010	0.000	0.0
14	0.027	0.008	0.019	70.4	0.021	0.016	0.005	23.8
15	0.071	0.021	0.050 **	70.4	0.065	0.027	0.038 **	58.5
16	0.117	0.060	0.057 **	48.7	0.119	0.062	0.057 **	47.9
17	0.125	0.158	-0.033	-26.4	0.157	0.160	-0.003	-1.9
18	0.229	0.250	-0.021	-9.2	0.242	0.253	-0.011	-4.5
19	0.313	0.338	-0.025	-8.0	0.331	0.359	-0.028	-8.5
B) Ever Given Birth								
15	0.013	0.004	0.009	69.2	0.012	0.004	0.008	66.7
16	0.035	0.007	0.028 **	80.0	0.034	0.013	0.021 **	61.8
17	0.074	0.025	0.049 ***	66.2	0.081	0.035	0.046 ***	56.8
18	0.112	0.108	0.004	3.6	0.132	0.122	0.010	7.6
19	0.184	0.217	-0.033	-17.9	0.193	0.222	-0.029	-15.0

Notes: Predictions are based on the estimates given in columns (3) and (8) of Table 2; i.e, specifications with split time trends in the form of linear polynomials. Predictions are given for someone born between December 1986 and January 1987. Statistically significant *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table 5: Policy Effect on the Time to First Birth after Marriage

	Single Time Trend						Split Time Trends			
Degree of Polynomial	One (1)	Two (2)	Three (3)	Four (4)	Five (5)	Six (6)	One (7)	Two (8)	Three (9)	Obs.
A) With age at marriage dummies										
Year of Birth	-3.457***	-1.022	-0.008	-0.070	--	--	-1.195**	-0.485	--	12,635
1959-1998	[0.630]	[0.761]	[0.575]	[0.668]	--	--	[0.440]	[0.738]	--	
Month-Year of Birth	1.593	1.735	--	--	--	--	1.681	--	--	1,514
4-year intervals	[1.786]	[1.750]	--	--	--	--	[1.749]	--	--	
Month-Year of Birth	-1.290	-0.270	1.887	1.551	--	--	-0.556	2.422**	--	2,516
7-year intervals	[1.419]	[1.321]	[2.144]	[2.069]	--	--	[1.332]	[0.899]	--	
Month-Year of Birth	-2.229*	-0.113	0.274	0.898	1.999	3.088	-0.703	0.776	4.389**	3,394
10-year intervals	[1.271]	[1.232]	[1.795]	[1.880]	[2.425]	[2.499]	[1.238]	[1.112]	[1.739]	
B) Without age at marriage dummies										
Year of Birth	-3.009***	-0.862	-0.225	-0.276	--	--	-1.413***	-0.671	--	12,635
1959-1998	[0.554]	[0.733]	[0.604]	[0.714]	--	--	[0.410]	[0.677]	--	
Month-Year of Birth	1.351	1.090	--	--	--	--	1.051	--	--	1,514
4-year intervals	[1.876]	[1.828]	--	--	--	--	[1.827]	--	--	
Month-Year of Birth	-1.530	-0.911	1.775	0.658	--	--	-1.231	1.761*	--	2,516
7-year intervals	[1.453]	[1.336]	[2.245]	[2.113]	--	--	[1.353]	[0.944]	--	
Month-Year of Birth	-2.832**	-1.143	-0.061	-0.149	1.233	2.108	-1.883	-0.161	3.832*	3,394
10-year intervals	[1.245]	[1.194]	[1.827]	[1.859]	[2.492]	[2.490]	[1.196]	[1.013]	[1.867]	

Notes: The sample includes observations from both the 2008 and 2013 DHS in the analysis by year of birth, but only from the 2013 DHS in the analysis by month-year of birth. The sample is restricted to ages 17 and above because the policy has no effect on marriage status after age 17. In all analyses, 1986 and 1987 birth cohorts are excluded. The policy dummy is one when year of birth is greater than 1987. Each cell comes from a separate OLS regresion of the time to first-birth after marriage (in months) on the policy dummy and the specified time trends. Standard errors are clustered at the year-of-birth level when the running variable is year of birth and at the month-year of birth level when the running variable is month-year of birth. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table 6: Policy Effect on Number of Children Ever Born – Month-Year of Birth as Running Variable, 4-Year and 7-Year Intervals around the Bubble

	II) 4-Year Intervals			II) 7-Year Intervals					
	Single Time Trend		Split	Single Time Trend				Split Time Trends	
Degree of Polynomial	One (1)	Two (2)	One (3)	One (4)	Two (5)	Three (6)	Four (7)	One (8)	Two (9)
Age=15	0.001	0.001	0.001	0.006	0.006	0.005	0.004	0.006	0.003
	[0.008]	[0.008]	[0.008]	[0.005]	[0.005]	[0.009]	[0.009]	[0.005]	[0.011]
Age=16	-0.009	-0.009	-0.009	-0.006	-0.006	-0.002	-0.003	-0.006	-0.003
	[0.011]	[0.011]	[0.011]	[0.008]	[0.008]	[0.014]	[0.014]	[0.008]	[0.018]
Age=17	-0.029	-0.031*	-0.030	-0.016	-0.017	-0.014	-0.015	-0.016	-0.017
	[0.019]	[0.018]	[0.018]	[0.015]	[0.015]	[0.023]	[0.022]	[0.014]	[0.028]
Age=18	-0.070**	-0.071**	-0.070**	-0.052**	-0.052**	-0.072*	-0.074**	-0.052**	-0.085*
	[0.031]	[0.029]	[0.029]	[0.025]	[0.024]	[0.038]	[0.036]	[0.024]	[0.045]
Age=19	-0.003	-0.005	-0.004	-0.018	-0.019	0.007	0.002	-0.018	0.013
	[0.049]	[0.047]	[0.046]	[0.034]	[0.034]	[0.056]	[0.052]	[0.033]	[0.065]
Age=20	0.023	0.022	0.022	-0.004	-0.016	0.082	0.060	-0.016	0.076
	[0.061]	[0.060]	[0.059]	[0.046]	[0.046]	[0.076]	[0.074]	[0.045]	[0.092]

Notes: The data come from the 2013 DHS. The sample includes 48 months on each side of the bubble in panel (I) and 84 months on each side of the bubble in panel (II) (except for ages 19 and 20 in panel II, where there are 72 and 60 months, respectively, on the right-hand-side of the bubble). The number of observations is 2,324 in all cells in part (I), while it is 4,061 in all cells for ages up to 18, 4,035 in all cells for age 19, and 3,750 in all cells for age 20 in panel (II). The policy dummy is one when year of birth is greater than 1987. Each cell comes from a separate OLS regression of the number of children born by the specified age on the policy variable and the specified time trends in month and year of birth. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table 7: Policy Effect on Marriage and First-Birth Hazard Rates by Age – Month-Year of Birth as Running Variable, 4-Year and 7-Year Intervals around the Bubble

	I) 4-Year Intervals			II) 7-Year Intervals					
	Single Time Trend		Split Trends	Single Time Trend				Split Time Trends	
Degree of Polynomial	One (1)	Two (2)	One (3)	One (4)	Two (5)	Three (6)	Four (7)	One (8)	Two (9)
A) Dependent Variable: Marriage status conditional on not being married until that age									
Age=14	0.292	0.205	0.218	0.690	0.553	0.273	0.114	0.557	0.083
	(0.377)	(0.285)	(0.298)	(0.567)	(0.485)	(0.368)	(0.184)	(0.482)	(0.162)
Age=15	0.302**	0.346	0.308*	0.308***	0.267**	0.253*	0.237*	0.259**	0.177*
	(0.176)	(0.243)	(0.220)	(0.128)	(0.137)	(0.179)	(0.189)	(0.137)	(0.185)
Age=16	0.826	0.796	0.796	0.640	0.624	0.876	0.831	0.620	0.914
	(0.401)	(0.398)	(0.398)	(0.197)	(0.199)	(0.464)	(0.467)	(0.200)	(0.653)
Age=17	4.355***	4.970***	5.300***	2.446***	2.459***	3.646***	4.157***	2.507***	6.511***
	(1.729)	(2.042)	(2.275)	(0.670)	(0.686)	(1.585)	(1.974)	(0.716)	(4.296)
Age=18	0.822	0.820	0.818	1.098	1.098	1.125	1.138	1.098	1.004
	(0.304)	(0.301)	(0.298)	(0.282)	(0.284)	(0.454)	(0.445)	(0.283)	(0.487)
Age=19	1.105	1.102	1.098	1.193	1.257	0.653	0.605	1.236	0.583
	(0.486)	(0.499)	(0.498)	(0.361)	(0.364)	(0.317)	(0.315)	(0.356)	(0.399)
B) Dependent Variable: Birth status conditional on not giving birth until that age									
Age=15	0.193	0.051*	0.053*	0.277*	0.139**	0.192	0.071	0.116**	0.035
	(0.225)	(0.081)	(0.081)	(0.194)	(0.117)	(0.272)	(0.115)	(0.108)	(0.073)
Age=16	0.294*	0.192*	0.165*	0.517	0.425	0.281*	0.191*	0.404	0.083**
	(0.197)	(0.168)	(0.155)	(0.271)	(0.273)	(0.216)	(0.177)	(0.269)	(0.105)
Age=17	0.512	0.451	0.430	0.458**	0.443**	0.316*	0.240**	0.430**	0.160**
	(0.286)	(0.263)	(0.255)	(0.171)	(0.175)	(0.190)	(0.161)	(0.175)	(0.135)
Age=18	2.343*	2.358*	2.393*	1.956**	1.947**	2.615**	2.655*	1.924**	3.564*
	(1.062)	(1.075)	(1.109)	(0.586)	(0.556)	(1.263)	(1.325)	(0.554)	(2.318)
Age=19	1.658	1.665	1.643	1.710*	1.743**	1.655	1.678	1.732**	1.618
	(0.667)	(0.651)	(0.633)	(0.468)	(0.463)	(0.757)	(0.759)	(0.456)	(0.907)

Notes: The data come from the 2013 DHS. The sample includes 48 months on each side of the bubble in panel (I) and 84 months on each side of the bubble in panel (II). The samples in panel (A) are restricted to individuals aged 14 and above, while the samples in panel (B) are restricted to individuals aged 15 and above. The number of observations is 12,533 in all columns in panel (A) of part (I), while it is 11,055 in all columns in panel (B) of part (I). In part (II), the number of observations is 21,769 in all columns in panel (A) and 19,221 in all columns in panel (B). The policy dummy is one when year of birth is greater than 1987. The estimates in each column come from a separate logistic regression of marriage hazard status in panel (A) and of birth hazard status in panel (B) on the interactions of the policy variable with each age dummy and the interactions of the specified time trends in month and year of birth with each age dummy. Odds ratios and their standard errors are given as estimated parameters. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table 8: Policy Effect on Marriage Hazard Rate and First-Birth Hazard Rate by Age – Month-Year of Birth as Running Variable, 10-Year Intervals around the Bubble

	Single Time Trend						Split Time Trends		
Degree of Polynomial	One (1)	Two (2)	Three (3)	Four (4)	Five (5)	Six (6)	One (7)	Two (8)	Three (9)
A) Dependent Variable: Marriage status conditional on not being married until that age									
Age=14	0.490	0.671	0.577	0.250	0.411	0.214	0.600	0.166	0.267
	(0.327)	(0.444)	(0.558)	(0.294)	(0.633)	(0.386)	(0.385)	(0.256)	(0.645)
Age=15	0.367***	0.450**	0.352**	0.232**	0.157**	0.135**	0.428**	0.162**	0.060*
	(0.135)	(0.179)	(0.174)	(0.153)	(0.116)	(0.120)	(0.172)	(0.136)	(0.091)
Age=16	1.125	1.202	0.557	0.397**	0.715	0.867	1.167	0.316**	1.361
	(0.296)	(0.304)	(0.212)	(0.168)	(0.436)	(0.504)	(0.295)	(0.170)	(1.275)
Age=17	2.607***	2.827***	2.980***	2.294**	4.316***	4.438***	2.674***	2.810**	10.713***
	(0.628)	(0.643)	(1.029)	(0.848)	(2.159)	(2.261)	(0.614)	(1.343)	(9.700)
Age=18	1.320	1.428	1.023	0.833	0.807	1.247	1.402	0.770	0.715
	(0.282)	(0.327)	(0.309)	(0.298)	(0.326)	(0.644)	(0.309)	(0.315)	(0.524)
Age=19	1.072	1.324	0.858	0.649	0.995	2.027	1.272	0.542	7.460*
	(0.273)	(0.369)	(0.304)	(0.321)	(0.534)	(1.538)	(0.336)	(0.340)	(7.687)
B) Dependent Variable: Birth status conditional on not giving birth until that age									
Age=15	0.605	0.485	0.178**	0.070**	0.218	0.038*	0.459	0.013**	0.011
	(0.368)	(0.294)	(0.156)	(0.074)	(0.321)	(0.070)	(0.290)	(0.023)	(0.032)
Age=16	0.553	0.689	0.543	0.236*	0.205*	0.173*	0.623	0.128**	0.027**
	(0.256)	(0.339)	(0.325)	(0.187)	(0.177)	(0.161)	(0.313)	(0.132)	(0.045)
Age=17	0.876	0.847	0.377**	0.192***	0.221**	0.244**	0.830	0.110***	0.095**
	(0.282)	(0.295)	(0.164)	(0.109)	(0.145)	(0.174)	(0.288)	(0.078)	(0.110)
Age=18	1.798**	2.354***	2.245**	1.669	2.679*	4.553**	2.105***	1.843	8.209**
	(0.488)	(0.584)	(0.858)	(0.712)	(1.524)	(2.851)	(0.510)	(0.962)	(7.646)
Age=19	1.471	1.739**	1.905*	1.691	1.383	2.436	1.662**	2.005	1.357
	(0.350)	(0.427)	(0.652)	(0.735)	(0.657)	(1.548)	(0.390)	(0.957)	(1.174)

Notes: The data come from the 2013 DHS. The sample includes 10 years on each side of the bubble. The samples in panel (A) are restricted to individuals aged 14 and above, while the samples in panel (B) are restricted to individuals aged 15 and above. The number of observations is 30,151 in all columns in panel (A), while it is 26,220 in all columns in panel (B). The policy dummy is one when year of birth is greater than 1987. The estimates in each column come from a separate logistic regression of marriage hazard status in panel (A) and of birth hazard status in panel (B) on the interactions of the policy variable with each age dummy and the interactions of the specified time trends in month and year of birth with each age dummy. Odds ratios and their standard errors are given as estimated parameters. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table 9: Falsification Test of Level Effects of the Policy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
I) 4-Year Intervals around the Bubble, Split Linear Polynomials											
	Location of Chosen Discontinuity compared to the Actual Discontinuity (in months)										
	-20	-16	-12	-8	-4	0	+4	+8	+12	+16	+20
A) Dependent Variable: Ever Married											
Age=15	0.928	0.729	0.537	0.869	0.339*	0.285**	0.489	0.608	0.674	1.590	1.079
	[0.542]	[0.464]	[0.403]	[0.613]	[0.191]	[0.161]	[0.287]	[0.378]	[0.418]	[0.942]	[0.787]
Age=16	0.813	1.004	0.865	0.983	0.735	0.483**	0.776	0.889	1.027	1.426	1.038
	[0.305]	[0.367]	[0.309]	[0.361]	[0.260]	[0.167]	[0.302]	[0.368]	[0.437]	[0.594]	[0.490]
B) Dependent Variable: Ever Given Birth											
Age=16	1.567	1.214	0.642	1.338	0.310	0.200**	0.415	0.333	0.341	0.691	0.655
	[1.163]	[0.984]	[0.582]	[1.160]	[0.231]	[0.147]	[0.292]	[0.251]	[0.275]	[0.549]	[0.495]
Age=17	0.601	0.621	0.545	0.764	0.371*	0.313**	0.837	0.945	1.030	1.583	0.888
	[0.332]	[0.363]	[0.354]	[0.478]	[0.219]	[0.143]	[0.363]	[0.452]	[0.501]	[0.815]	[0.572]
II) 7-Year Intervals around the Bubble, Split Quadratic Polynomials											
	Location of Chosen Discontinuity compared to the Actual Discontinuity (in months)										
	-20	-16	-12	-8	-4	0	+4	+8	+12	+16	+20
A) Dependent Variable: Ever Married											
Age=14	8.170**	4.921	1.925	1.292	0.113	0.113*	0.133	0.074*	0.490	1.220	0.511
	[8.488]	[5.309]	[2.365]	[1.789]	[0.156]	[0.148]	[0.181]	[0.099]	[0.590]	[1.406]	[0.689]
Age=15	1.634	0.922	0.911	0.834	0.170**	0.159**	0.414	0.367	0.729	1.709	0.902
	[1.162]	[0.737]	[0.869]	[0.822]	[0.136]	[0.126]	[0.333]	[0.320]	[0.641]	[1.489]	[0.969]
Age=16	0.990	1.139	1.674	1.478	0.770	0.414*	0.578	0.482	0.812	1.135	0.765
	[0.483]	[0.540]	[0.805]	[0.763]	[0.376]	[0.192]	[0.310]	[0.280]	[0.505]	[0.678]	[0.511]
B) Dependent Variable: Ever Given Birth											
Age=16	3.417	2.121	1.555	1.381	0.188*	0.164*	0.336	0.117**	0.128*	0.455	0.526
	[2.925]	[2.196]	[1.731]	[1.532]	[0.181]	[0.163]	[0.330]	[0.125]	[0.146]	[0.526]	[0.583]
Age=17	0.928	1.009	1.049	0.850	0.238	0.154***	0.385	0.422	0.861	1.662	0.803
	[0.597]	[0.700]	[0.812]	[0.701]	[0.208]	[0.102]	[0.247]	[0.291]	[0.610]	[1.215]	[0.718]
II) 7-Year Intervals around the Bubble, Single Quartic Polynomials											
	Location of Chosen Discontinuity compared to the Actual Discontinuity (in months)										
	-20	-16	-12	-8	-4	0	+4	+8	+12	+16	+20
A) Dependent Variable: Ever Married											
Age=14	4.018*	2.807	1.405	0.941	0.187	0.175*	0.195	0.148*	0.673	1.375	0.647
	[3.373]	[2.483]	[1.448]	[1.119]	[0.198]	[0.183]	[0.214]	[0.160]	[0.661]	[1.286]	[0.705]
Age=15	1.233	0.788	0.777	0.629	0.215**	0.232**	0.465	0.457	0.768	1.457	0.882
	[0.743]	[0.537]	[0.602]	[0.517]	[0.130]	[0.145]	[0.297]	[0.309]	[0.537]	[0.995]	[0.774]
Age=16	0.927	1.011	1.323	1.113	0.687	0.442**	0.582	0.534	0.819	1.062	0.813
	[0.361]	[0.384]	[0.504]	[0.458]	[0.260]	[0.167]	[0.257]	[0.258]	[0.418]	[0.514]	[0.439]
B) Dependent Variable: Ever Given Birth											
Age=16	2.017	1.317	1.077	0.969	0.252*	0.241*	0.383	0.196**	0.210*	0.593	0.621
	[1.523]	[1.175]	[0.977]	[0.873]	[0.178]	[0.182]	[0.292]	[0.162]	[0.191]	[0.536]	[0.579]
Age=17	0.779	0.830	0.862	0.694	0.288**	0.231***	0.462	0.511	0.911	1.496	0.893
	[0.422]	[0.475]	[0.523]	[0.442]	[0.176]	[0.123]	[0.242]	[0.285]	[0.526]	[0.875]	[0.642]

Notes: In each column, a different cut-off value for month-year of birth is taken in defining the policy dummy -- as specified in the column heading. For instance, (-4) means that the policy dummy is one when month-year of birth is greater than the actual value of the cut-off (January 1, 1987) minus 4 months, which is September 1, 1987. In each column, a bubble of 12 months is taken on each side of the cut-off as it is done in the actual analysis. Each cell comes from a separate logistic regression of ever-married status at the specified age in panel (A) and of ever-given-birth status at the specified age in panel (B) on the policy variable and the time trends--which are specified in the headings. Odds ratios and their standard erros are given as the estimated coefficients. The standard errors are clustered at the month-year of birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

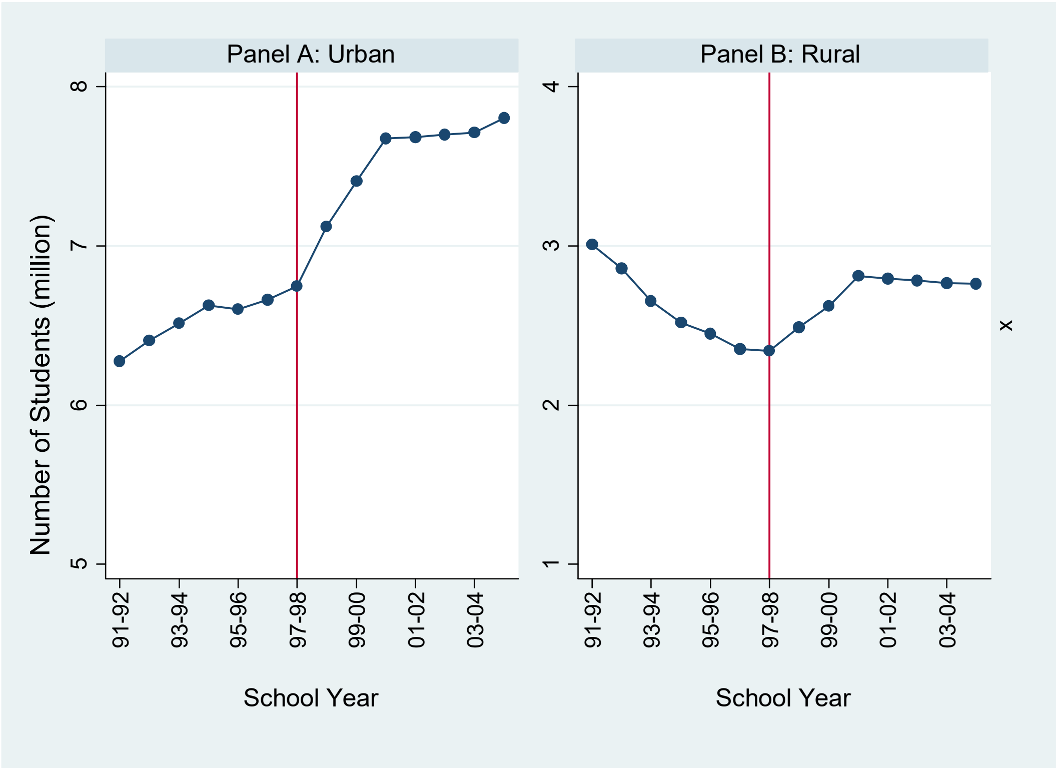
Table 10: Falsification Test of Timing Effects of the Policy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
I) 4-Year Intervals around the Bubble, Split Linear Polynomials											
	Location of Chosen Discontinuity compared to the Actual Discontinuity (in months)										
	-20	-16	-12	-8	-4	0	+4	+8	+12	+16	+20
A) Dependent Variable: Marriage status conditional on not being married until that age											
Age=15	0.352	0.328	0.331	0.650	0.313*	0.308*	0.583	0.719	0.772	1.879	1.672
	(0.307)	(0.274)	(0.331)	(0.571)	(0.211)	(0.220)	(0.468)	(0.595)	(0.622)	(1.489)	(1.669)
Age=17	0.609	1.163	1.713	2.695**	4.042***	5.300***	3.197**	4.545***	4.270***	2.234	0.867
	(0.263)	(0.487)	(0.676)	(1.069)	(1.585)	(2.275)	(1.707)	(2.583)	(2.200)	(1.139)	(0.414)
B) Dependent Variable: Birth status conditional on not giving birth until that age											
Age=15	2.482	0.742	1.474	3.330	0.774	0.053*	0.078	0.143	0.175	0.587	0.537
	(3.007)	(1.190)	(2.804)	(6.018)	(1.336)	(0.081)	(0.121)	(0.183)	(0.219)	(0.676)	(0.714)
Age=16	0.872	0.739	0.199	0.574	0.123**	0.165*	0.385	0.552	0.517	1.057	1.082
	(0.862)	(0.724)	(0.201)	(0.477)	(0.118)	(0.155)	(0.348)	(0.494)	(0.507)	(1.104)	(1.014)
Age=18	0.841	1.180	0.938	1.209	1.623	2.393*	2.587**	3.230***	3.499***	2.427*	1.683
	(0.367)	(0.516)	(0.378)	(0.504)	(0.660)	(1.109)	(1.165)	(1.420)	(1.632)	(1.222)	(0.881)
II) 7-Year Intervals around the Bubble, Split Quadratic Polynomials											
	Location of Chosen Discontinuity compared to the Actual Discontinuity (in months)										
	-20	-16	-12	-8	-4	0	+4	+8	+12	+16	+20
A) Dependent Variable: Marriage status conditional on not being married until that age											
Age=15	0.469	0.310	0.543	0.641	0.222	0.177*	0.694	0.851	1.048	2.433	1.650
	(0.480)	(0.320)	(0.654)	(0.760)	(0.205)	(0.185)	(0.746)	(0.964)	(1.185)	(2.735)	(2.485)
Age=17	0.341*	0.988	1.965	3.305**	4.621***	6.511***	2.249	3.481	3.166*	1.435	0.551
	(0.195)	(0.557)	(1.081)	(1.815)	(2.541)	(4.296)	(1.604)	(2.636)	(2.052)	(0.979)	(0.378)
B) Dependent Variable: Birth status conditional on not giving birth until that age											
Age=16	2.075	1.043	0.336	0.382	0.067**	0.083**	0.177	0.211	0.271	1.381	1.392
	(2.368)	(1.381)	(0.444)	(0.439)	(0.089)	(0.105)	(0.223)	(0.262)	(0.366)	(1.956)	(1.866)
Age=17	0.248	0.493	0.714	0.590	0.306	0.160**	0.439	1.155	3.315	4.119	1.333
	(0.241)	(0.464)	(0.631)	(0.548)	(0.325)	(0.135)	(0.358)	(0.973)	(3.117)	(3.979)	(1.370)
Age=18	0.451	0.518	0.526	1.094	1.978	3.564*	3.518**	2.931*	2.509	1.258	0.699
	(0.266)	(0.309)	(0.301)	(0.643)	(1.140)	(2.318)	(2.138)	(1.849)	(1.692)	(0.934)	(0.550)
II) 7-Year Intervals around the Bubble, Single Quartic Polynomials											
	Location of Chosen Discontinuity compared to the Actual Discontinuity (in months)										
	-20	-16	-12	-8	-4	0	+4	+8	+12	+16	+20
A) Dependent Variable: Marriage status conditional on not being married until that age											
Age=15	0.497	0.336	0.517	0.486	0.232**	0.237*	0.621	0.743	0.877	1.612	1.283
	(0.420)	(0.293)	(0.499)	(0.472)	(0.167)	(0.189)	(0.514)	(0.633)	(0.757)	(1.372)	(1.503)
Age=17	0.481	1.047	1.751	2.524**	3.174***	4.157***	2.255	3.304**	3.229**	1.828	0.913
	(0.220)	(0.478)	(0.770)	(1.091)	(1.348)	(1.974)	(1.194)	(1.859)	(1.647)	(0.983)	(0.513)
B) Dependent Variable: Birth status conditional on not giving birth until that age											
Age=16	1.310	0.710	0.320	0.357	0.143**	0.191*	0.307	0.324	0.400	1.407	1.317
	(1.264)	(0.748)	(0.324)	(0.316)	(0.130)	(0.177)	(0.290)	(0.315)	(0.432)	(1.551)	(1.477)
Age=17	0.317	0.515	0.658	0.538	0.344	0.240**	0.526	1.059	2.449	3.046	1.348
	(0.238)	(0.373)	(0.450)	(0.380)	(0.264)	(0.161)	(0.345)	(0.717)	(1.770)	(2.276)	(1.100)
Age=18	0.621	0.690	0.696	1.177	1.794	2.655*	2.855**	2.502*	2.252	1.341	0.885
	(0.292)	(0.327)	(0.316)	(0.545)	(0.815)	(1.325)	(1.359)	(1.221)	(1.172)	(0.766)	(0.533)

Notes: In each column, a different cut-off value for month-year of birth is taken in defining the policy dummy -- as specified in the column heading. For instance, (-4) means that the policy dummy is one when month-year of birth is greater than the actual value of the cut-off (January 1, 1987) minus 4 months, which is September 1, 1987. In each column, a bubble of 12 months is taken on each side of the cut-off as it is done in the actual analysis. Each cell comes from a separate logistic regression of ever-married status at the specified age in panel (A) and of ever-given-birth status at the specified age in panel (B) on the interactions of the policy variable with each age dummy as well as the interactions of age dummies with the time trends--which are specified in the headings. The coefficients in each cell denote the policy effect for that age, calculated as the linear combination of the baseline policy coefficient and the policy coefficient for that specific age. Odds ratios and their standard erros are given as the estimated coefficients. The standard errors are clustered at the month-year of birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

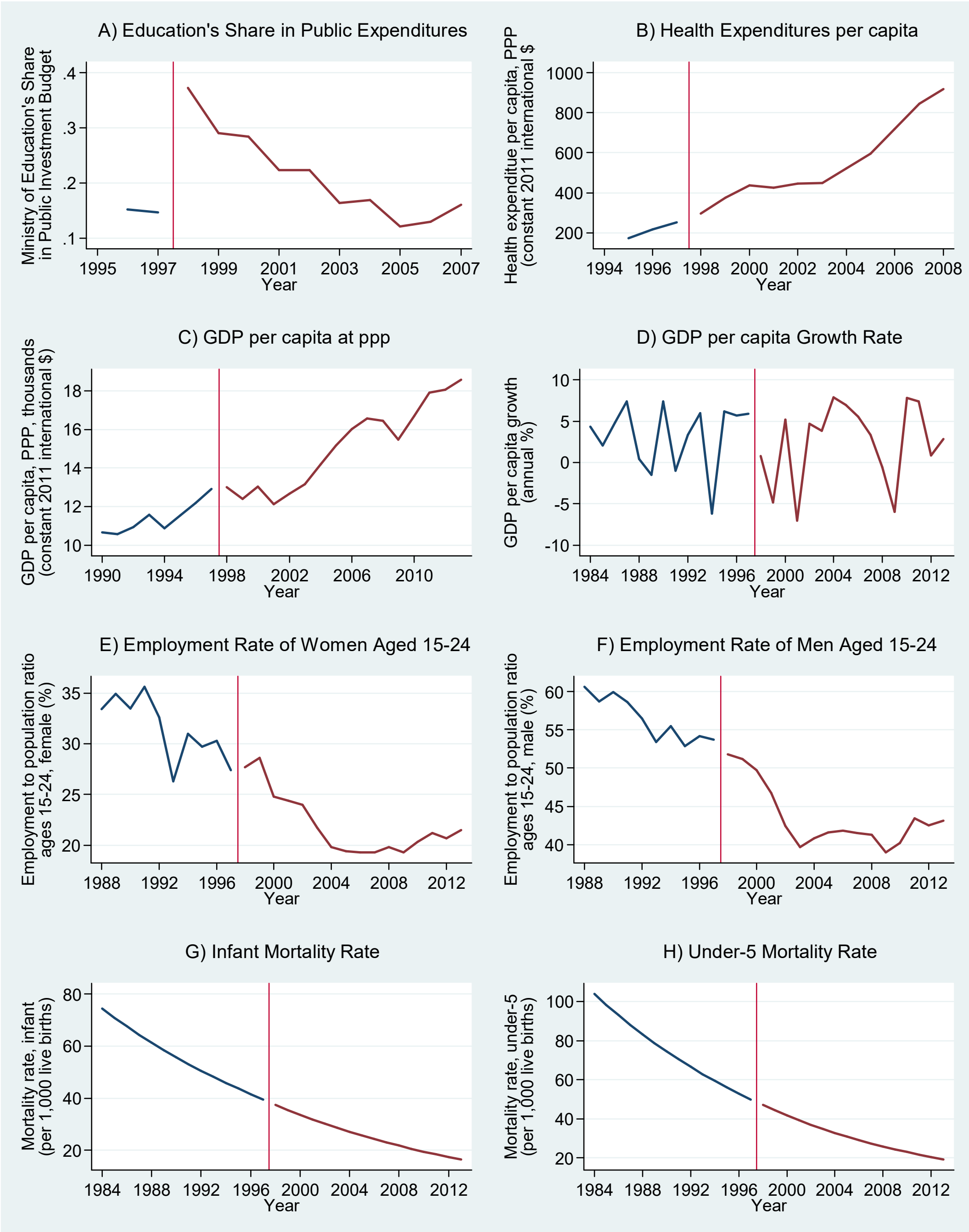
ONLINE APPENDIX (Not for Print Publication)

Figure A1: Number of Students in Basic Education (Grades 1 to 8)



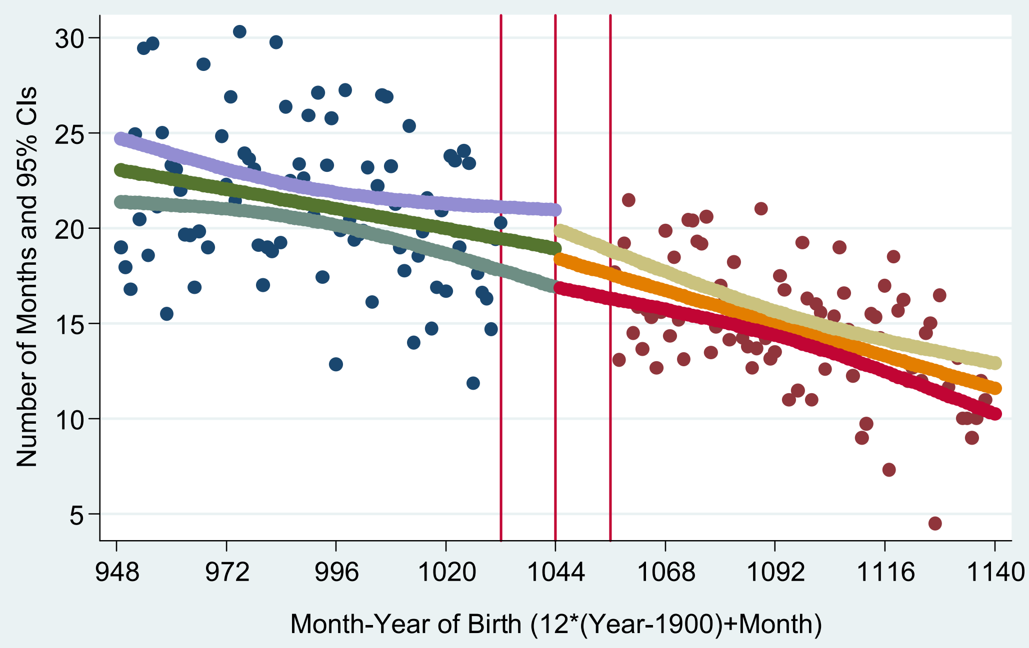
Source: Turkish Statistical Institute (1992-2005)

Figure A2: Testing whether the Cut-off Value for the Running Variable is related to Interventions other than the 1997 Education Reform



Source: Turkish Statistical Institute (2006), World Development Indicators (2015).

Figure A3: Time to First Birth after Marriage



Notes: a) The sample includes women aged 17 and above in the 2013 DHS.
b) 1986 and 1987 birth cohorts are excluded due to the fuzziness of treatment status.
c) Linear polynomials are fit before and after the cut-off.

Table A1: Descriptive Statistics I: Person-Level Characteristics for Sample (A)

	Mean	S.D.	Min.	Max.	No. Obs.
Year of Birth	1979.78	10.32	1959	1998	21,862
Age	30.69	10.07	15	49	21,862
Policy	0.31	0.46	0	1	21,862
Years of Schooling	6.99	4.26	0	21	21,845
Ever Married	0.69	0.46	0	1	21,862
Age at Marriage	21.47	6.52	7	49	15,112
Age at First-Birth	21.51	4.19	10	44	12,644
Time to First-Birth after Marriage (months)	21.11	21.47	0	337	12,644
Location of Birth Characteristics					
Rural	0.442	0.497	0	1	21,837
NUTS-1 Level Region					
Region 1	0.044	0.204	0	1	21,849
Region 2	0.042	0.200	0	1	21,849
Region 3	0.061	0.239	0	1	21,849
Region 4	0.049	0.215	0	1	21,849
Region 5	0.065	0.246	0	1	21,849
Region 6	0.124	0.329	0	1	21,849
Region 7	0.080	0.271	0	1	21,849
Region 8	0.101	0.301	0	1	21,849
Region 9	0.081	0.273	0	1	21,849
Region 10	0.094	0.291	0	1	21,849
Region 11	0.101	0.301	0	1	21,849
Region 12	0.146	0.353	0	1	21,849
Foreign Born	0.015	0.120	0	1	21,849

Notes: The sample is drawn from 15- to 49-year-old individuals in the 2008 and 2013 DHS surveys. The sample excludes the 1986 and 1987 birth cohorts.

Table A2: Descriptive Statistics II: Person-Level Characteristics for Samples (B1) - (B3)

	A) Sample B1					B) Sample B2					C) Sample B3				
	Mean	S.D.	Min.	Max.	Obs.	Mean	S.D.	Min.	Max.	Obs.	Mean	S.D.	Min.	Max.	Obs.
Year of Birth	1986.4	3.20	1982	1991	2,325	1986.3	4.91	1979	1994	4,066	1986.4	6.71	1976	1997	5,946
Age	26.51	3.21	21	31	2,325	26.61	4.91	18	34	4,066	26.50	6.71	15	37	5,946
Policy	0.49	0.50	0	1	2,325	0.48	0.50	0	1	4,066	0.49	0.50	0	1	5,946
Years of Schooling	8.16	4.56	0	21	2,325	8.13	4.39	0	21	4,065	8.08	4.16	0	21	5,945
Ever Married	0.76	0.43	0	1	2,325	0.72	0.45	0	1	4,066	0.65	0.48	0	1	5,946
Age at Marriage	20.11	3.35	10	31	1,775	20.08	3.62	10	34	2,924	20.15	3.84	10	36	3,867
Age at First-Birth	21.28	3.23	12	31	1,514	21.44	3.66	12	34	2,516	21.62	3.95	12	36	3,395
Time to First-Birth after															
Marriage (months)	20.06	16.51	0	227	1,514	20.59	17.29	0	227	2,516	21.16	19.15	0	227	3,395
Location of Birth															
Rural	0.402	0.490	0	1	2324	0.403	0.491	0	1	4061	0.403	0.490	0	1	5,939
NUTS-1 Region															
Region 1	0.052	0.222	0	1	2324	0.050	0.218	0	1	4063	0.051	0.221	0	1	5,942
Region 2	0.032	0.177	0	1	2324	0.030	0.171	0	1	4063	0.034	0.182	0	1	5,942
Region 3	0.063	0.243	0	1	2324	0.065	0.247	0	1	4063	0.063	0.243	0	1	5,942
Region 4	0.043	0.202	0	1	2324	0.044	0.206	0	1	4063	0.048	0.213	0	1	5,942
Region 5	0.064	0.244	0	1	2324	0.063	0.243	0	1	4063	0.064	0.245	0	1	5,942
Region 6	0.121	0.327	0	1	2324	0.120	0.325	0	1	4063	0.121	0.327	0	1	5,942
Region 7	0.077	0.266	0	1	2324	0.082	0.274	0	1	4063	0.076	0.265	0	1	5,942
Region 8	0.094	0.292	0	1	2324	0.093	0.291	0	1	4063	0.093	0.291	0	1	5,942
Region 9	0.080	0.272	0	1	2324	0.083	0.276	0	1	4063	0.085	0.279	0	1	5,942
Region 10	0.100	0.300	0	1	2324	0.101	0.301	0	1	4063	0.097	0.296	0	1	5,942
Region 11	0.100	0.300	0	1	2324	0.102	0.303	0	1	4063	0.102	0.302	0	1	5,942
Region 12	0.157	0.364	0	1	2324	0.149	0.356	0	1	4063	0.148	0.356	0	1	5,942
Foreign Born	0.018	0.133	0	1	2324	0.018	0.134	0	1	4063	0.017	0.130	0	1	5,942
Month of Birth															
January	0.117	0.321	0	1	2325	0.109	0.311	0	1	4066	0.108	0.311	0	1	5,946
February	0.083	0.276	0	1	2325	0.080	0.271	0	1	4066	0.082	0.275	0	1	5,946
March	0.092	0.289	0	1	2325	0.092	0.289	0	1	4066	0.090	0.287	0	1	5,946
April	0.092	0.290	0	1	2325	0.093	0.290	0	1	4066	0.091	0.288	0	1	5,946
May	0.072	0.259	0	1	2325	0.077	0.266	0	1	4066	0.082	0.274	0	1	5,946
June	0.087	0.282	0	1	2325	0.086	0.281	0	1	4066	0.084	0.277	0	1	5,946
July	0.077	0.267	0	1	2325	0.074	0.262	0	1	4066	0.076	0.265	0	1	5,946
August	0.079	0.270	0	1	2325	0.090	0.286	0	1	4066	0.089	0.284	0	1	5,946
September	0.092	0.289	0	1	2325	0.088	0.283	0	1	4066	0.088	0.284	0	1	5,946
October	0.089	0.285	0	1	2325	0.090	0.286	0	1	4066	0.085	0.279	0	1	5,946
November	0.062	0.240	0	1	2325	0.064	0.245	0	1	4066	0.064	0.244	0	1	5,946
December	0.058	0.233	0	1	2325	0.058	0.234	0	1	4066	0.060	0.238	0	1	5,946

Notes: The samples are drawn from 15- to 49-year-old individuals in the 2013 DHS survey. All samples exclude the 1986 and 1987 birth cohorts.

Table A3: Descriptive Statistics III: Person-Age Level Characteristics for Survival Analysis Samples

	A) Marriage Sample						B) First-birth Sample				
	Mean	S.D.	Min.	Max.	No. Obs.		Mean	S.D.	Min.	Max.	No. Obs.
1) Sample B1						1) Sample B1					
Year of Birth	1986.4	3.2	1982	1991	12,533	Year of Birth	1986.4	3.2	1982	1991	11,055
Year	2002.8	3.6	1996	2010	12,533	Year	2003.3	3.5	1997	2010	11,055
Age	16.34	1.68	14	19	12,533	Age	16.93	1.40	15	19	11,055
Policy	0.495	0.500	0	1	12,533	Policy	0.492	0.500	0	1	11,055
Married	0.063	0.243	0	1	12,533	Given Birth	0.043	0.202	0	1	11,055
Trend (in months)	-1.17	38.49	-59.5	59.5	12,533	Trend (in months)	-1.46	38.49	-59.5	59.5	11,055
2) Sample B2						2) Sample B2					
Year of Birth	1986.4	4.9	1979	1994	21,769	Year of Birth	1986.3	4.9	1979	1994	19,221
Year	2002.7	5.200	1993	2013	21,769	Year	2003.3	5.1	1994	2013	19,221
Age	16.33	1.68	14	19	21,769	Age	16.93	1.40	15	19	19,221
Policy	0.490	0.500	0	1	21,769	Policy	0.485	0.500	0	1	19,221
Married	0.063	0.244	0	1	21,769	Given Birth	0.043	0.204	0	1	19,221
Trend (in months)	-1.75	58.88	-95.5	95.5	21,769	Trend (in months)	-2.47	58.86	-95.5	95.5	19,221
3) Sample B3						3) Sample B3					
Year of Birth	1986.1	6.5	1976	1997	30,151	Year of Birth	1985.8	6.4	1976	1997	26,220
Year	2002.3	6.6	1990	2013	30,151	Year	2002.6	6.4	1991	2013	26,220
Age	16.22	1.65	14	19	30,151	Age	16.83	1.39	15	19	26,220
Policy	0.475	0.499	0	1	30,151	Policy	0.460	0.498	0	1	26,220
Married	0.061	0.239	0	1	30,151	Given Birth	0.043	0.203	0	1	26,220
Trend (in months)	-5.68	77.73	-131.5	131.5	30,151	Trend (in months)	-8.77	76.69	-131.5	131.5	26,220

Notes: The samples are drawn from 15- to 49-year-old individuals in the 2008 and 2013 DHS surveys. All samples exclude the 1986 and 1987 birth cohorts.

Table A4: Policy Effect on Marriage and First-Birth by Age – Sample (A) and Year of Birth as the Running Variable

	Single Time Trend				Split Time Trends		
	Linear (1)	Quadratic (2)	Cubic (3)	Quartic (4)	Linear (5)	Quadratic (6)	Obs.
A) Dependent Variable: Ever Married							
Age=12	0.249***	0.597	0.488	0.415	0.333*	0.690	20,552
	[0.118]	[0.366]	[0.309]	[0.288]	[0.199]	[0.492]	
Age=13	0.329***	0.590	0.604	0.487*	0.572*	0.791	20,552
	[0.083]	[0.247]	[0.241]	[0.213]	[0.183]	[0.356]	
Age=14	0.305***	0.438***	0.601**	0.546**	0.572**	0.387***	20,552
	[0.056]	[0.108]	[0.137]	[0.130]	[0.129]	[0.108]	
Age=15	0.389***	0.528***	0.701**	0.611***	0.729*	0.494**	20,552
	[0.061]	[0.120]	[0.123]	[0.098]	[0.120]	[0.148]	
Age=16	0.556***	0.744**	0.828	0.777**	0.785*	0.610***	19,732
	[0.056]	[0.098]	[0.102]	[0.086]	[0.098]	[0.105]	
Age=17	0.711***	0.979	1.073	1.127	0.932	0.775	18,879
	[0.072]	[0.115]	[0.124]	[0.148]	[0.094]	[0.150]	
Age=18	0.749***	0.922	0.985	1.027	0.874	0.655***	18,043
	[0.050]	[0.087]	[0.128]	[0.120]	[0.109]	[0.072]	
Age=19	0.800***	0.929	0.976	1.042	0.859***	0.871**	17,174
	[0.040]	[0.069]	[0.077]	[0.067]	[0.039]	[0.056]	
B) Dependent Variable: Ever Given Birth							
Age=13	0.282**	0.885	1.385	1.520	1.217	0.137	20,552
	[0.158]	[0.736]	[1.129]	[1.196]	[0.598]	[0.229]	
Age=14	0.255***	0.417	0.702	0.669	0.652	0.335	20,552
	[0.105]	[0.267]	[0.455]	[0.428]	[0.338]	[0.374]	
Age=15	0.353***	0.446**	0.486**	0.411**	0.444***	0.209**	20,552
	[0.077]	[0.153]	[0.160]	[0.153]	[0.130]	[0.128]	
Age=16	0.425***	0.587***	0.678**	0.610***	0.639***	0.454***	19,732
	[0.054]	[0.118]	[0.129]	[0.114]	[0.104]	[0.121]	
Age=17	0.567***	0.651***	0.710**	0.773*	0.638***	0.500**	18,873
	[0.067]	[0.091]	[0.107]	[0.112]	[0.093]	[0.142]	
Age=18	0.677***	0.873	1.003	1.047	0.850	0.600**	18,033
	[0.056]	[0.085]	[0.127]	[0.117]	[0.100]	[0.127]	
Age=19	0.697***	0.829**	0.904	0.968	0.757***	0.746***	17,159
	[0.039]	[0.065]	[0.078]	[0.057]	[0.038]	[0.062]	

Notes: The sample includes observations from both 2008 and 2013 DHS. In the 2008 survey, it includes 1959 to 1993 birth cohorts in all rows of age values above; in the 2013 survey, it includes 1964 to 1998 birth cohorts for ages 12 to 15, 1964 to 1997 birth cohorts for age 16, 1964 to 1996 birth cohorts for age 17, 1964 to 1995 birth cohorts for age 18, and 1964 to 1994 birth cohorts for age 19. 1986 and 1987 birth cohorts are excluded in all cases. The policy dummy is one when year of birth is greater than 1987. Each cell comes from a separate logistic regression of ever-married status at the specified age in panel (A) and of ever-given-birth status at the specified age in panel (B) on the policy variable as well as the specified time trends. Odds ratios and their standard errors are given as estimated parameters. In the first 4 columns, single time trends up to quartic polynomials are fitted, whereas separate polynomials are fitted on either side of the discontinuity in columns (5) and (6). The standard errors are clustered at the year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A5: Policy Effect on Marriage and First-Birth by Age – 10-year Birth Intervals on Both Sides of the Bubble; Year of Birth as the Running Variable

	Single Time Trend				Split Time Trends		
	Linear (1)	Quadratic (2)	Cubic (3)	Quartic (4)	Linear (5)	Quadratic (6)	Obs.
				A) Dependent Variable: Ever Married			
Age=12	0.729	0.630	1.435	2.567	0.560	2,216	12,863
	[0.365]	[0.448]	[1.301]	[2.153]	[0.367]	[2.397]	
Age=13	0.482*	0.593	0.643	0.919	0.610	0.888	12,863
	[0.180]	[0.269]	[0.446]	[0.585]	[0.238]	[0.599]	
Age=14	0.486***	0.629*	0.557*	0.555*	0.625**	0.485**	12,863
	[0.110]	[0.149]	[0.185]	[0.173]	[0.141]	[0.177]	
Age=15	0.510***	0.702**	0.695	0.656	0.682**	0.666	12,863
	[0.106]	[0.115]	[0.163]	[0.179]	[0.105]	[0.192]	
Age=16	0.707**	0.839	0.736**	0.657***	0.818	0.649**	12,417
	[0.100]	[0.107]	[0.108]	[0.101]	[0.107]	[0.125]	
Age=17	1.019	1.137	1.183	1.012	1.093	1.033	11,564
	[0.120]	[0.133]	[0.212]	[0.171]	[0.131]	[0.203]	
Age=18	0.955	1,015	0.939	0.774*	0.990	0.732**	10,728
	[0.075]	[0.137]	[0.145]	[0.113]	[0.125]	[0.089]	
Age=19	0.984	0.993	1.048	1.036	0.978	0.998	9,859
	[0.059]	[0.058]	[0.087]	[0.102]	[0.050]	[0.066]	
				B) Dependent Variable: Ever Given Birth			
Age=13	1.077	2.228	1.263	0.792	2.180	0.125	12,863
	[0.913]	[2.055]	[1.412]	[1.154]	[1.446]	[0.241]	
Age=14	0.470	0.778	0.574	0.299	0.781	0.351	12,863
	[0.326]	[0.572]	[0.568]	[0.397]	[0.501]	[0.456]	
Age=15	0.558	0.498**	0.378**	0.303***	0.502*	0.254**	12,863
	[0.212]	[0.177]	[0.166]	[0.140]	[0.179]	[0.157]	
Age=16	0.583***	0.711*	0.523**	0.430***	0.708*	0.406***	12,417
	[0.115]	[0.147]	[0.133]	[0.123]	[0.130]	[0.128]	
Age=17	0.695***	0.725**	0.696**	0.554***	0.712**	0.555**	11,558
	[0.087]	[0.111]	[0.107]	[0.105]	[0.105]	[0.166]	
Age=18	0.898	1.027	1.033	0.735*	0.974	0.751	10,718
	[0.070]	[0.134]	[0.145]	[0.123]	[0.118]	[0.164]	
Age=19	0.888**	0.901*	0.946	0.901	0.882**	0.867	9,844
	[0.049]	[0.052]	[0.081]	[0.089]	[0.044]	[0.075]	

Notes: The sample includes observations from both 2008 and 2013 DHS. The sample is restricted to 10-year intervals around the bubble; i.e., it includes 1976-1985 and 1988-97 birth cohorts. The policy dummy is one when year of birth is greater than 1987. Each cell comes from a separate logistic regression of ever-married status at the specified age in panel (A) and of ever-given-birth status at the specified age in panel (B) on the policy variable as well as the specified time trends. Odds ratios and their standard errors are given as estimated parameters. In the first 4 columns, single time trends up to quartic polynomials are fitted, whereas separate polynomials are fitted on either side of the discontinuity in columns (5) and (6). The standard errors are clustered at the year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A6: Policy Effect on Marriage and First-Birth by Age – 5-year Birth Intervals on Both Sides of the Bubble; Year of Birth as the Running Variable

	Single Time Trend		Split Time Trends		Single Time Trend		Split Time Trends	
	Linear	Quadratic	Linear	Obs.	Linear	Quadratic	Linear	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	A) Dependent Variable: Ever Married				B) Dependent Variable: Ever Given Birth			
Age=12	1.472	0.800	0.855	7,310	--	--	--	--
	[0.844]	[0.468]	[0.525]		--	--	--	
Age=13	0.518	0.603	0.655	7,310	1.640	1.435	1.590	7,310
	[0.381]	[0.323]	[0.309]		[2.544]	[2.196]	[2.261]	
Age=14	0.397***	0.446***	0.455***	7,310	0.327	0.414	0.522	7,310
	[0.087]	[0.085]	[0.089]		[0.403]	[0.470]	[0.458]	
Age=15	0.430***	0.483***	0.484***	7,310	0.431*	0.326**	0.359*	7,310
	[0.114]	[0.113]	[0.106]		[0.191]	[0.180]	[0.192]	
Age=16	0.698**	0.756**	0.747**	7,310	0.400***	0.457***	0.460***	7,310
	[0.110]	[0.094]	[0.095]		[0.111]	[0.087]	[0.076]	
Age=17	1.099	1.092	1.084	6,867	0.580***	0.604**	0.599**	6,862
	[0.200]	[0.199]	[0.187]		[0.073]	[0.137]	[0.129]	
Age=18	0.936	0.917	0.913	6,438	0.855	0.844	0.839	6,432
	[0.125]	[0.111]	[0.098]		[0.110]	[0.095]	[0.091]	
Age=19	1.011	1.001	0.997	5,969	0.897	0.881*	0.877**	5,964
	[0.099]	[0.080]	[0.071]		[0.082]	[0.063]	[0.058]	

Notes: The sample includes observations from both 2008 and 2013 DHS. The sample is restricted to 5-year intervals around the bubble; i.e., it includes 1981-1985 and 1988-92 birth cohorts. The policy dummy is one when year of birth is greater than 1987. Each cell comes from a separate logistic regression of ever-married status at the specified age in panel (A) and of ever-given-birth status at the specified age in panel (B) on the policy variable as well as the specified time trends. Odds ratios and their standard errors are given as estimated parameters. The standard errors are clustered at the year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A7: Policy Effect on Number of Children – Sample (B3) and Month-Year of Birth as Running Variable

	Single Time Trend						Split Time Trends		
Degree of Polynomial	One (1)	Two (2)	Three (3)	Four (4)	Five (5)	Six (6)	One (7)	Two (8)	Three (9)
A) Dependent Variable: Ever Married									
Age=15	0.002	0.002	0.008	0.008	0.004	0.004	0.002	0.009	-0.003
	[0.004]	[0.004]	[0.006]	[0.006]	[0.009]	[0.009]	[0.004]	[0.008]	[0.014]
Age=16	-0.005	-0.005	-0.005	-0.006	0.000	-0.001	-0.005	-0.006	-0.005
	[0.007]	[0.007]	[0.011]	[0.011]	[0.015]	[0.014]	[0.007]	[0.013]	[0.024]
Age=17	-0.015	-0.017	-0.007	-0.016	-0.019	-0.032	-0.017	-0.015	-0.046
	[0.014]	[0.013]	[0.019]	[0.017]	[0.025]	[0.023]	[0.013]	[0.021]	[0.040]
Age=18	-0.034	-0.038*	-0.043	-0.092***	-0.076*	-0.103**	-0.040*	-0.097***	-0.121*
	[0.021]	[0.021]	[0.031]	[0.028]	[0.039]	[0.040]	[0.021]	[0.033]	[0.063]
Age=19	-0.003	-0.021	0.024	-0.031	-0.007	0.005	-0.024	-0.021	-0.031
	[0.031]	[0.032]	[0.043]	[0.048]	[0.057]	[0.076]	[0.031]	[0.054]	[0.108]
Age=20	0.008	-0.015	0.057	0.028	0.018	0.026	-0.023	0.043	-0.234
	[0.038]	[0.044]	[0.053]	[0.071]	[0.074]	[0.112]	[0.042]	[0.079]	[0.172]

Notes: The data come from the 2013 DHS. The sample includes 10 years on the left hand side of the bubble; on the right hand side of the bubble, the sample includes 10 years for age 15, 9 years for age 16, 8 years for age 17, 7 years for age 18, 6 years for age 19, and 5 years for age 20. The number of observations is 5,946 in all cells for age 15, 5,921 for age 16, 5,589 for age 17, 5,273 for age 18, 4,970 for age 19, and 4,685 for age 20. The policy dummy is one when year of birth is greater than 1987. Each cell comes from a separate OLS regression of the number of children born by the specified age on the policy variable and the specified time trends in month and year of birth. The standard errors are clustered at the month-year of birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A8: Policy Effect on Marriage and First-Birth Hazard Rates by Age – Sample (A) and Year of Birth as Running Variable

	Single Time Trend				Split Time Trends	
	Linear	Quadratic	Cubic	Quartic	Linear	Quadratic
	(1)	(2)	(3)	(4)	(5)	(6)
A) Dependent Variable: Marriage status conditional on not being married until that age						
Age=12	0.226**	0.533	0.234	0.172	0.164	0.763
	[0.138]	[0.436]	[0.267]	[0.195]	[0.195]	[1.234]
Age=13	0.381***	0.583	0.662	0.517	0.742	0.805
	[0.143]	[0.360]	[0.382]	[0.330]	[0.280]	[0.520]
Age=14	0.289***	0.357***	0.613	0.597	0.573*	0.215***
	[0.065]	[0.109]	[0.183]	[0.188]	[0.175]	[0.069]
Age=15	0.459***	0.599*	0.780	0.660*	0.866	0.576
	[0.089]	[0.184]	[0.184]	[0.155]	[0.190]	[0.264]
Age=16	0.739**	1.096	1.124	1.097	0.964	0.759
	[0.091]	[0.193]	[0.215]	[0.218]	[0.170]	[0.223]
Age=17	0.901	1.512*	1.675**	1.993**	1.292	1.119
	[0.155]	[0.356]	[0.371]	[0.566]	[0.204]	[0.334]
Age=18	0.722***	0.825	0.861	0.864	0.825	0.644
	[0.088]	[0.160]	[0.198]	[0.188]	[0.184]	[0.189]
Age=19	0.788*	1.029	1.273	1.284	1.109	1.006
	[0.106]	[0.194]	[0.240]	[0.231]	[0.193]	[0.341]
B) Dependent Variable: Birth status conditional on not giving birth until that age						
Age=14	0.243***	0.301*	0.524	0.515	0.495	0.241
	[0.118]	[0.212]	[0.356]	[0.364]	[0.293]	[0.363]
Age=15	0.417***	0.454**	0.443**	0.339**	0.404***	0.137***
	[0.110]	[0.181]	[0.163]	[0.149]	[0.132]	[0.068]
Age=16	0.455***	0.721	0.876	0.799	0.818	0.598**
	[0.071]	[0.172]	[0.186]	[0.170]	[0.141]	[0.153]
Age=17	0.689**	0.734	0.808	1.044	0.716	0.512
	[0.110]	[0.156]	[0.189]	[0.237]	[0.155]	[0.220]
Age=18	0.763*	1.198	1.442**	1.444*	1.169	1.044
	[0.116]	[0.237]	[0.264]	[0.287]	[0.170]	[0.342]
Age=19	0.659***	0.800**	0.902	0.957	0.767***	0.817
	[0.048]	[0.074]	[0.063]	[0.066]	[0.048]	[0.103]

Notes: The sample includes observations from both 2008 and 2013 DHS. It includes 1959 to 1993 birth cohorts in the 2008 survey and 1964 to 1998 birth cohorts in the 2013 survey. 1986 and 1987 birth cohorts are excluded in all cases. The data are in person-age format. In panel (A), individuals who are never married by age 12 enter the risk set at this age and stay in the risk set until they get married or until the last age they are observed in the sample. Similarly, in panel (B), individuals who have never given birth by age 14 enter the risk set at this age and stay in the risk set until they give birth or until the last age they are observed in the sample. The number of observations is 141,622 in panel (A) and 108,361 in panel (B). The policy dummy is one when year of birth is greater than 1987. The estimates in each column come from a separate logistic regression of marriage hazard status in panel (A) and first-birth hazard status in panel (B) on the interactions of the policy dummy with each age dummy as well as the interactions of the specified time trends with each age dummy. Odds ratios and their standard errors are given as estimated parameters. In the first 4 columns, single time trends up to quartic polynomials are fitted, whereas separate polynomials are fitted on either side of the discontinuity in columns (5) and (6). The standard errors are clustered at the year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A9: Check of Continuity in the Relationships between the Outcome Variables and the Running Variable I – Cut-off at January 1978

	I) Levels						II) Timing					
	Single Time Trend				Split Time Trends		Single Time Trend				Split Time Trends	
Degree of Polynomial	One (1)	Two (2)	Three (3)	Four (4)	One (5)	Two (6)	One (7)	Two (8)	Three (9)	Four (10)	One (11)	Two (12)
	I-A) Dependent Variable: Ever Married						II-A) Dependent Variable: Marriage status conditional on not being married until that age					
Age=13	0.419	0.488	0.295	0.301	0.486	0.234	--	--	--	--	--	--
	[0.281]	[0.319]	[0.273]	[0.285]	[0.314]	[0.276]	--	--	--	--	--	--
Age=14	0.812	0.862	0.653	0.699	0.858	0.613	1.345	1.309	1.191	1.169	1.280	0.987
	[0.354]	[0.349]	[0.430]	[0.434]	[0.335]	[0.469]	(0.854)	(0.761)	(1.124)	(0.987)	(0.712)	(0.996)
Age=15	0.953	0.957	0.980	0.979	0.953	0.956	1.093	1.096	1.437	1.511	1.100	1.634
	[0.286]	[0.277]	[0.511]	[0.515]	[0.273]	[0.630]	(0.400)	(0.405)	(0.962)	(1.108)	(0.412)	(1.545)
Age=16	0.982	0.984	1.030	1.018	0.982	1.010	1.012	0.990	1.097	1.046	0.986	1.044
	[0.229]	[0.229]	[0.408]	[0.411]	[0.229]	[0.531]	(0.307)	(0.302)	(0.543)	(0.539)	(0.305)	(0.719)
Age=17	1.125	1.132	0.828	0.813	1.127	0.736	1.371	1.409	0.588	0.547	1.387	0.427
	[0.232]	[0.232]	[0.297]	[0.298]	[0.231]	[0.349]	(0.390)	(0.384)	(0.280)	(0.274)	(0.377)	(0.271)
Age=18	1.028	1.028	0.973	0.971	1.026	0.955	0.856	0.853	1.335	1.328	0.852	1.522
	[0.189]	[0.189]	[0.306]	[0.306]	[0.188]	[0.386]	(0.217)	(0.212)	(0.570)	(0.559)	(0.211)	(0.804)
Age=19	0.938	0.933	0.793	0.794	0.930	0.781	0.773	0.765	0.542	0.541	0.763	0.539
	[0.158]	[0.156]	[0.220]	[0.221]	[0.155]	[0.278]	(0.229)	(0.218)	(0.265)	(0.258)	(0.213)	(0.314)
	I-B) Dependent Variable: Ever Given Birth						II-B) Dependent Variable: Birth status conditional on not giving birth until that age					
Age=15	0.841	0.887	0.304	0.357	0.889	0.204	1.070	1.068	0.165	0.210	1.077	0.098
	[0.494]	[0.480]	[0.282]	[0.302]	[0.460]	[0.206]	(0.840)	(0.840)	(0.228)	(0.257)	(0.818)	(0.142)
Age=16	0.853	0.883	0.783	0.784	0.876	0.696	0.863	0.882	1.573	1.598	0.874	2.161
	[0.300]	[0.289]	[0.466]	[0.468]	[0.280]	[0.521]	(0.363)	(0.346)	(1.060)	(1.156)	(0.341)	(2.001)
Age=17	0.633*	0.657*	0.780	0.750	0.653*	0.739	0.485**	0.499**	0.764	0.682	0.490**	0.718
	[0.163]	[0.162]	[0.344]	[0.342]	[0.161]	[0.438]	(0.164)	(0.163)	(0.443)	(0.416)	(0.161)	(0.576)
Age=18	0.871	0.881	0.834	0.798	0.876	0.782	1.398	1.396	1.003	0.956	1.401	0.974
	[0.199]	[0.197]	[0.333]	[0.326]	[0.197]	[0.417]	(0.454)	(0.454)	(0.530)	(0.536)	(0.465)	(0.699)
Age=19	0.954	0.958	0.836	0.831	0.955	0.778	1.109	1.108	0.876	0.885	1.102	0.770
	[0.181]	[0.179]	[0.282]	[0.283]	[0.178]	[0.345]	(0.329)	(0.319)	(0.421)	(0.396)	(0.311)	(0.422)

Notes: The data come from the 2013 DHS. The data are restricted to cohorts born before 1985 (i.e. those that are not affected by the 1997 education reform). In this range of birth cohorts, we take the 7-year interval with a bubble (as in the actual analysis) right before 1985. Hence, the sample includes the 1976 to 1985 birth cohorts, excluding the 1980 and 1981 birth cohorts. The counterfactual cut-off value of month-year of birth for the policy variable is January 1981. The number of observations is 4,013 in panels (I-A) and (I-B), 20,638 in panel (II-A), and 18,552 in panel (II-B). Each cell comes from a separate logistic regression of ever-married status in panel (I-A) and of ever-given-birth status in panel (I-B) at the specified age on the policy variable and the specified time trends in month and year of birth. The estimates in each column come from a separate logistic regression of marriage hazard status in panel (II-A) and of birth hazard status in panel (II-B) on the interactions of the policy variable with each age dummy and the interactions of the specified time trends in month and year of birth with each age dummy. Odds ratios and their standard errors are given as estimated parameters. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A10: Check of Continuity in the Relationships between the Outcome Variables and the Running Variable II – Cut-off at January 1976

	I) Levels						II) Timing					
	Single Time Trend				Split Time Trends		Single Time Trend				Split Time Trends	
Degree of Polynomial	One (1)	Two (2)	Three (3)	Four (4)	One (5)	Two (6)	One (7)	Two (8)	Three (9)	Four (10)	One (11)	Two (12)
	I-A) Dependent Variable: Ever Married						II-A) Dependent Variable: Marriage status conditional on not being married until that age					
Age=13	0.585	0.541	0.144*	0.151*	0.549	0.093*	--	--	--	--	--	--
	[0.386]	[0.362]	[0.155]	[0.165]	[0.367]	[0.128]	--	--	--	--	--	--
Age=14	1.071	1.066	0.564	0.558	1.070	0.444	1.698	1.526	1.384	2.175	1.591	2.774
	[0.414]	[0.408]	[0.345]	[0.366]	[0.413]	[0.389]	(0.800)	(0.672)	(0.996)	(1.892)	(0.719)	(3.284)
Age=15	1.231	1.223	0.808	0.815	1.225	0.770	1.360	1.352	1.084	1.078	1.349	1.146
	[0.350]	[0.346]	[0.388]	[0.399]	[0.347]	[0.489]	(0.507)	(0.502)	(0.678)	(0.668)	(0.499)	(0.908)
Age=16	0.940	0.939	0.852	0.851	0.938	0.862	0.712	0.713	0.909	0.928	0.716	1.035
	[0.217]	[0.215]	[0.335]	[0.334]	[0.215]	[0.424]	(0.191)	(0.191)	(0.409)	(0.398)	(0.189)	(0.542)
Age=17	1.047	1.041	1.253	1.246	1.039	1.346	1.227	1.209	2.162*	2.146*	1.204	2.537*
	[0.198]	[0.196]	[0.382]	[0.382]	[0.196]	[0.513]	(0.324)	(0.312)	(0.865)	(0.861)	(0.309)	(1.279)
Age=18	1.086	1.073	1.134	1.118	1.068	1.180	1.141	1.124	0.899	0.886	1.117	0.877
	[0.189]	[0.186]	[0.312]	[0.311]	[0.186]	[0.407]	(0.271)	(0.261)	(0.304)	(0.295)	(0.258)	(0.362)
Age=19	1.048	1.045	1.170	1.128	1.041	1.146	0.945	0.976	1.178	1.097	0.970	1.005
	[0.187]	[0.190]	[0.337]	[0.332]	[0.191]	[0.417]	(0.294)	(0.314)	(0.590)	(0.529)	(0.314)	(0.591)
	I-B) Dependent Variable: Ever Given Birth						II-B) Dependent Variable: Birth status conditional on not giving birth until that age					
Age=15	1.311	1.311	0.724	0.768	1.350	0.722	1.511	1.520	1.669	3.013	1.605	6.599
	[0.595]	[0.595]	[0.518]	[0.642]	[0.636]	[0.842]	(0.872)	(0.874)	(1.567)	(3.782)	(0.972)	(12.045)
Age=16	1.047	1.041	0.619	0.620	1.043	0.523	0.907	0.904	0.575	0.578	0.900	0.489
	[0.308]	[0.303]	[0.280]	[0.288]	[0.305]	[0.320]	(0.367)	(0.357)	(0.351)	(0.338)	(0.351)	(0.358)
Age=17	0.824	0.824	0.592	0.593	0.821	0.548	0.668	0.683	0.599	0.623	0.684	0.636
	[0.203]	[0.198]	[0.231]	[0.230]	[0.196]	[0.268]	(0.248)	(0.242)	(0.351)	(0.340)	(0.236)	(0.430)
Age=18	0.913	0.911	0.881	0.874	0.909	0.889	1.057	1.064	1.557	1.521	1.067	1.803
	[0.188]	[0.186]	[0.293]	[0.289]	[0.186]	[0.366]	(0.300)	(0.308)	(0.737)	(0.709)	(0.310)	(1.061)
Age=19	1.120	1.110	1.012	0.997	1.106	0.984	1.535	1.501	1.247	1.228	1.489	1.137
	[0.191]	[0.189]	[0.275]	[0.274]	[0.189]	[0.335]	(0.409)	(0.392)	(0.544)	(0.523)	(0.385)	(0.591)

Notes: The data come from the 2013 DHS. The data are restricted to cohorts born before 1985 (i.e. those that are not affected by the 1997 education reform). In this range of birth cohorts, we take the 1968 to 1983 birth cohorts excluding the 1975 and 1976 birth cohorts (7-year intervals around the bubble). The counterfactual cut-off value of month-year of birth for the policy variable is January 1983. The number of observations is 3,994 in panels (I-A) and (I-B), 20,354 in panel (II-A), and 18,395 in panel (II-B). Each cell comes from a separate logistic regression of ever-married status in panel (I-A) and of ever-given-birth status in panel (I-B) at the specified age on the policy variable and the specified time trends in month and year of birth. The estimates in each column come from a separate logistic regression of marriage hazard status in panel (II-A) and of birth hazard status in panel (II-B) on the interactions of the policy variable with each age dummy and the interactions of the specified time trends in month and year of birth with each age dummy. Odds ratios and their standard errors are given as estimated parameters. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A11: Check of Continuity in the Relationships between the Outcome Variables and the Running Variable III – Cut-off at January 1974

	I) Levels						II) Timing					
	Single Time Trend				Split Time Trends		Single Time Trend				Split Time Trends	
Degree of Polynomial	One (1)	Two (2)	Three (3)	Four (4)	One (5)	Two (6)	One (7)	Two (8)	Three (9)	Four (10)	One (11)	Two (12)
	I-A) Dependent Variable: Ever Married						II-A) Dependent Variable: Marriage status conditional on not being married until that age					
Age=13	2.544	2.560	12.131**	12.057**	2.568	17.943**	--	--	--	--	--	--
	[1.499]	[1.530]	[12.153]	[12.381]	[1.510]	[22.587]	--	--	--	--	--	--
Age=14	1.718	1.752	3.665**	3.607**	1.743	4.513**	1.272	1.258	1.510	1.439	1.251	1.611
	[0.608]	[0.626]	[2.078]	[2.049]	[0.618]	[3.182]	(0.635)	(0.613)	(1.185)	(1.075)	(0.607)	(1.513)
Age=15	1.120	1.124	1.450	1.424	1.121	1.477	0.763	0.762	0.638	0.637	0.762	0.544
	[0.286]	[0.285]	[0.602]	[0.587]	[0.285]	[0.758]	(0.284)	(0.282)	(0.378)	(0.371)	(0.282)	(0.389)
Age=16	0.792	0.790	0.876	0.874	0.789	0.846	0.541**	0.552**	0.510	0.507	0.552**	0.442
	[0.176]	[0.174]	[0.314]	[0.313]	[0.174]	[0.381]	(0.161)	(0.158)	(0.230)	(0.226)	(0.157)	(0.248)
Age=17	0.937	0.925	0.915	0.909	0.924	0.898	1.228	1.185	0.975	0.962	1.182	0.994
	[0.184]	[0.181]	[0.298]	[0.297]	[0.181]	[0.376]	(0.341)	(0.325)	(0.435)	(0.429)	(0.324)	(0.577)
Age=18	0.993	0.989	0.884	0.874	0.987	0.774	1.104	1.121	0.861	0.853	1.122	0.636
	[0.171]	[0.171]	[0.265]	[0.263]	[0.171]	[0.295]	(0.293)	(0.303)	(0.426)	(0.415)	(0.306)	(0.382)
Age=19	1.173	1.161	1.029	1.021	1.159	0.927	1.669*	1.627*	1.426	1.461	1.623*	1.465
	[0.199]	[0.198]	[0.316]	[0.318]	[0.199]	[0.371]	(0.455)	(0.436)	(0.649)	(0.669)	(0.435)	(0.856)
	I-B) Dependent Variable: Ever Given Birth						II-B) Dependent Variable: Birth status conditional on not giving birth until that age					
Age=15	1.006	1.002	2.209	1.989	0.991	2.211	0.580	0.562	1.395	1.241	0.563	1.320
	[0.525]	[0.518]	[1.816]	[1.542]	[0.507]	[2.089]	(0.415)	(0.383)	(1.483)	(1.200)	(0.378)	(1.643)
Age=16	1.026	1.026	1.549	1.520	1.022	1.550	1.042	1.043	1.159	1.178	1.044	1.124
	[0.322]	[0.321]	[0.778]	[0.756]	[0.320]	[0.932]	(0.458)	(0.466)	(0.861)	(0.903)	(0.469)	(1.044)
Age=17	1.038	1.022	1.038	1.015	1.017	0.899	1.048	1.037	0.689	0.691	1.028	0.533
	[0.277]	[0.266]	[0.460]	[0.438]	[0.262]	[0.478]	(0.425)	(0.397)	(0.436)	(0.421)	(0.386)	(0.396)
Age=18	0.869	0.860	0.788	0.784	0.857	0.696	0.684	0.685	0.542	0.540	0.685	0.483
	[0.204]	[0.198]	[0.306]	[0.302]	[0.197]	[0.336]	(0.245)	(0.240)	(0.323)	(0.328)	(0.239)	(0.367)
Age=19	1.017	1.013	0.905	0.905	1.013	0.795	1.296	1.323	1.185	1.214	1.337	1.119
	[0.174]	[0.174]	[0.263]	[0.265]	[0.174]	[0.297]	(0.359)	(0.361)	(0.551)	(0.569)	(0.365)	(0.680)

Notes: The data come from the 2013 DHS. The data are restricted to cohorts born before 1985 (i.e. those that are not affected by the 1997 education reform). In this range of birth cohorts, we take the 1966 to 1981 birth cohorts excluding the 1973 and 1974 birth cohorts (7-year intervals around the bubble). The counterfactual cut-off value of month-year of birth for the policy variable is January 1974. The number of observations is 3,792 in panels (I-A) and (I-B), 19,220 in panel (II-A), and 17,397 in panel (II-B). Each cell comes from a separate logistic regression of ever-married status in panel (I-A) and of ever-given-birth status in panel (I-B) at the specified age on the policy variable and the specified time trends in month and year of birth. The estimates in each column come from a separate logistic regression of marriage hazard status in panel (II-A) and of birth hazard status in panel (II-B) on the interactions of the policy variable with each age dummy and the interactions of the specified time trends in month and year of birth with each age dummy. Odds ratios and their standard errors are given as estimated parameters. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A12: Check of Continuity in the Relationships between the Outcome Variables and the Running Variable IV – Cut-off at January 1972

	I) Levels						II) Timing					
	Single Time Trend				Split Time Trends		Single Time Trend				Split Time Trends	
Degree of Polynomial	One (1)	Two (2)	Three (3)	Four (4)	One (5)	Two (6)	One (7)	Two (8)	Three (9)	Four (10)	One (11)	Two (12)
	I-A) Dependent Variable: Ever Married						II-A) Dependent Variable: Marriage status conditional on not being married until that age					
Age=13	2.188	2.148	4.021	4.066	2.109	5.548	--	--	--	--	--	--
	[1.279]	[1.182]	[3.435]	[3.556]	[1.152]	[6.665]	--	--	--	--	--	--
Age=14	1.553	1.600	1.821	1.768	1.611	1.779	1.133	1.286	0.751	0.710	1.292	0.526
	[0.577]	[0.622]	[1.122]	[1.069]	[0.636]	[1.379]	(0.551)	(0.707)	(0.671)	(0.598)	(0.733)	(0.544)
Age=15	1.425	1.495	1.375	1.342	1.513	1.350	1.293	1.366	1.024	1.009	1.388	1.015
	[0.391]	[0.430]	[0.631]	[0.601]	[0.445]	[0.798]	(0.470)	(0.536)	(0.626)	(0.602)	(0.558)	(0.780)
Age=16	1.203	1.205	0.912	0.889	1.202	0.834	0.973	0.945	0.611	0.608	0.936	0.552
	[0.303]	[0.310]	[0.372]	[0.349]	[0.310]	[0.418]	(0.327)	(0.300)	(0.310)	(0.295)	(0.292)	(0.329)
Age=17	1.206	1.210	0.788	0.775	1.207	0.690	1.175	1.178	0.646	0.660	1.180	0.547
	[0.236]	[0.239]	[0.245]	[0.238]	[0.239]	[0.260]	(0.308)	(0.316)	(0.286)	(0.300)	(0.317)	(0.303)
Age=18	1.120	1.106	1.037	1.052	1.106	1.106	0.942	0.901	1.768	2.079*	0.907	2.999**
	[0.208]	[0.202]	[0.289]	[0.295]	[0.202]	[0.381]	(0.256)	(0.233)	(0.657)	(0.812)	(0.234)	(1.462)
Age=19	1.041	1.040	0.971	0.974	1.042	0.980	0.863	0.889	0.821	0.802	0.892	0.711
	[0.191]	[0.190]	[0.263]	[0.263]	[0.190]	[0.322]	(0.296)	(0.310)	(0.470)	(0.457)	(0.313)	(0.514)
	I-B) Dependent Variable: Ever Given Birth						II-B) Dependent Variable: Birth status conditional on not giving birth until that age					
Age=15	1.154	1.194	0.743	0.744	1.220	0.640	0.617	0.595	0.143	0.135	0.594	0.071
	[0.523]	[0.587]	[0.628]	[0.649]	[0.623]	[0.753]	(0.392)	(0.420)	(0.169)	(0.169)	(0.436)	(0.121)
Age=16	1.235	1.278	1.072	1.051	1.289	1.004	1.288	1.306	1.357	1.271	1.302	1.298
	[0.337]	[0.356]	[0.479]	[0.458]	[0.363]	[0.576]	(0.501)	(0.513)	(0.894)	(0.801)	(0.511)	(1.067)
Age=17	1.534*	1.537*	1.152	1.125	1.536*	1.065	1.848*	1.728	1.174	1.154	1.712	1.083
	[0.368]	[0.378]	[0.432]	[0.417]	[0.380]	[0.516]	(0.629)	(0.584)	(0.625)	(0.622)	(0.576)	(0.743)
Age=18	1.252	1.254	0.933	0.910	1.250	0.867	0.905	0.905	0.688	0.680	0.902	0.674
	[0.268]	[0.271]	[0.325]	[0.308]	[0.270]	[0.364]	(0.311)	(0.314)	(0.389)	(0.370)	(0.310)	(0.440)
Age=19	1.068	1.061	0.972	0.984	1.061	1.026	0.818	0.803	1.057	1.192	0.809	1.413
	[0.186]	[0.184]	[0.253]	[0.260]	[0.185]	[0.332]	(0.210)	(0.200)	(0.433)	(0.507)	(0.202)	(0.755)

Notes: The data come from the 2013 DHS. The data are restricted to cohorts born before 1985 (i.e. those that are not affected by the 1997 education reform). In this range of birth cohorts, we take the 1964 to 1979 birth cohorts excluding the 1971 and 1972 birth cohorts (7-year intervals around the bubble). The counterfactual cut-off value of month-year of birth for the policy variable is January 1972. The number of observations is 3,606 in panels (I-A) and (I-B), 18,127 in panel (II-A), and 16,476 in panel (II-B). Each cell comes from a separate logistic regression of ever-married status in panel (I-A) and of ever-given-birth status in panel (I-B) at the specified age on the policy variable and the specified time trends in month and year of birth. The estimates in each column come from a separate logistic regression of marriage hazard status in panel (II-A) and of birth hazard status in panel (II-B) on the interactions of the policy variable with each age dummy and the interactions of the specified time trends in month and year of birth with each age dummy. Odds ratios and their standard errors are given as estimated parameters. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A13: Check of a Policy Effect on Other Covariates at the Cut-off

	A) 4-year Intervals			B) 7-year Intervals					
	Single Time Trend		Split Time Trends	Single Time Trend				Split Time Trends	
Degree of Polynomial	One (1)	Two (2)	One (3)	One (4)	Two (5)	Three (6)	Four (7)	One (8)	Two (9)
Rural	0.965	0.965	0.965	1.145	1.146	1.077	1.067	1.144	1.040
	[0.202]	[0.201]	[0.202]	[0.173]	[0.171]	[0.244]	[0.243]	[0.171]	[0.295]
NUTS1 Region 1	0.756	0.777	0.771	0.628	0.618	0.663	0.662	0.621	0.633
	[0.415]	[0.400]	[0.397]	[0.228]	[0.213]	[0.376]	[0.376]	[0.212]	[0.437]
NUTS1 Region 2	0.469	0.456	0.449	0.774	0.808	0.457	0.432	0.800	0.349
	[0.272]	[0.273]	[0.271]	[0.358]	[0.354]	[0.300]	[0.285]	[0.351]	[0.300]
NUTS1 Region 3	0.468*	0.464*	0.463*	0.808	0.801	0.426*	0.415*	0.802	0.318*
	[0.187]	[0.192]	[0.192]	[0.239]	[0.241]	[0.202]	[0.207]	[0.242]	[0.206]
NUTS1 Region 4	2.428	2.448	2.437	0.819	0.809	2.306	2.303	0.810	3.580
	[1.602]	[1.563]	[1.567]	[0.323]	[0.333]	[1.623]	[1.621]	[0.335]	[3.150]
NUTS1 Region 5	1.398	1.392	1.386	0.790	0.790	1.421	1.419	0.789	1.906
	[0.678]	[0.691]	[0.694]	[0.256]	[0.258]	[0.742]	[0.752]	[0.259]	[1.286]
NUTS1 Region 6	0.997	0.963	0.973	0.804	0.801	0.822	0.830	0.801	0.850
	[0.375]	[0.360]	[0.368]	[0.182]	[0.183]	[0.318]	[0.314]	[0.184]	[0.405]
NUTS1 Region 7	1.206	1.206	1.203	1.299	1.293	1.197	1.193	1.299	1.068
	[0.548]	[0.545]	[0.545]	[0.362]	[0.368]	[0.571]	[0.569]	[0.373]	[0.639]
NUTS1 Region 8	0.670	0.688	0.691	1.081	1.132	0.825	0.800	1.125	0.738
	[0.330]	[0.332]	[0.327]	[0.333]	[0.330]	[0.421]	[0.416]	[0.323]	[0.463]
NUTS1 Region 9	1.205	1.183	1.166	1.650*	1.651*	1.445	1.493	1.652*	1.280
	[0.526]	[0.476]	[0.463]	[0.466]	[0.462]	[0.700]	[0.677]	[0.456]	[0.689]
NUTS1 Region 10	0.724	0.719	0.721	0.830	0.830	0.796	0.806	0.833	0.787
	[0.257]	[0.263]	[0.268]	[0.206]	[0.207]	[0.327]	[0.349]	[0.210]	[0.446]
NUTS1 Region 11	1.521	1.460	1.445	1.352	1.350	1.609	1.566	1.344	1.656
	[0.612]	[0.545]	[0.538]	[0.379]	[0.380]	[0.715]	[0.673]	[0.376]	[0.869]
NUTS1 Region 12	1.075	1.082	1.086	0.969	0.954	0.945	0.941	0.954	1.019
	[0.398]	[0.412]	[0.413]	[0.245]	[0.235]	[0.396]	[0.388]	[0.233]	[0.530]

Notes: The sample includes observations from the 2013 DHS. In panel (A), with 4-year intervals around the bubble, there are 2,324 observations in all regressions. In panel (B), with 7-year intervals around the bubble, there are 4,061 observations when the dependent variable is rural and 4,063 observations in the other regressions. The policy dummy is one when month-year of birth is greater than 1987 January. Each cell comes from a separate logistic regresion of the specified outcome on the policy dummy and the specified time trends. Odds ratios and their standard errors are given. Standard errors are clustered at the month-year of birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A14: Falsification Test with Sample (A)

A) Dependent Variable: Ever Married

		Year of Discontinuity							
		1983	1984	1985	1986	Actual	1988	1989	1990
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age=14		0.932	0.986	0.998	0.624*	0.546**	0.553**	0.750	0.853
		[0.283]	[0.309]	[0.386]	[0.151]	[0.130]	[0.138]	[0.274]	[0.447]
	No obs.	20,588	20,616	20,625	20,527	20,552	20,542	20,502	20,470
Age=15		1.023	0.886	0.751	0.649***	0.611***	0.656*	0.945	0.975
		[0.257]	[0.221]	[0.133]	[0.094]	[0.098]	[0.152]	[0.203]	[0.285]
	No obs.	20,588	20,616	20,625	20,527	20,552	20,542	20,502	20,470
Age=16		1.094	0.957	0.888	0.838	0.777**	0.868	0.933	0.891
		[0.165]	[0.124]	[0.100]	[0.111]	[0.086]	[0.137]	[0.169]	[0.199]
	No obs.	19,768	19,796	19,805	19,707	19,732	19,722	19,682	19,650

B) Dependent Variable: Ever Given Birth

		Year of Discontinuity							
		1983	1984	1985	1986	Actual	1988	1989	1990
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age=15		0.771	0.764	0.923	0.526	0.411**	0.350***	0.624	0.879
		[0.254]	[0.297]	[0.559]	[0.225]	[0.153]	[0.139]	[0.244]	[0.516]
	No obs.	20,588	20,616	20,625	20,527	20,552	20,542	20,502	20,470
Age=16		1.153	1.185	1.004	0.731**	0.610***	0.568***	0.822	0.929
		[0.294]	[0.297]	[0.267]	[0.107]	[0.114]	[0.117]	[0.185]	[0.265]
	No obs.	19,768	19,796	19,805	19,707	19,732	19,722	19,682	19,650
Age=17		1.067	1.018	0.934	0.875	0.773*	0.893	0.969	0.915
		[0.181]	[0.162]	[0.119]	[0.116]	[0.112]	[0.163]	[0.270]	[0.431]
	No obs.	18,909	18,937	18,946	18,848	18,873	18,863	18,823	18,791

Notes: In each column, a separate cut-off value of year of birth is taken in defining the policy dummy -- as specified in the column heading. For instance, in column (1), the policy dummy is one when year of birth is equal to 1983 or above, and zero otherwise. In each column, two birth cohorts around the cut-off are dropped; for instance, the 1982 and 1983 birth cohorts are dropped in column (1). Each cell comes from a separate logistic regression of ever-married status at the specified age in panel (A) and of ever-given-birth status at the specified age in panel (B) on the policy variable and the time trends--which are single time trends in the form of quartic polynomials. Odds ratios and their standard erros are given as the estimated coefficients. The standard errors are clustered at the year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A15: Policy Effect on Marriage and First-Birth by Age with Dummies for Month of Birth – 4-Year and 7-Year Intervals around the Bubble (Samples B1 and B2)

	I) 4-Year Intervals			II) 7-Year Intervals					
	Single Time Trend		Split Trends	Single Time Trend				Split Time Trends	
Degree of Polynomial	One (1)	Two (2)	One (3)	One (4)	Two (5)	Three (6)	Four (7)	One (8)	Two (9)
A) Dependent Variable: Ever Married									
Age=13	0.825	0.746	0.699	1.175	1.416	0.989	0.424	1.309	0.244
	[0.793]	[0.764]	[0.750]	[0.878]	[1.003]	[1.084]	[0.575]	[0.933]	[0.480]
Age=14	0.378	0.319	0.309	0.861	0.850	0.478	0.200	0.814	0.124
	[0.323]	[0.293]	[0.290]	[0.512]	[0.520]	[0.411]	[0.214]	[0.501]	[0.172]
Age=15	0.330**	0.313**	0.293**	0.472**	0.429**	0.328*	0.248**	0.416**	0.171**
	[0.164]	[0.169]	[0.160]	[0.168]	[0.171]	[0.191]	[0.158]	[0.169]	[0.139]
Age=16	0.553*	0.518**	0.507**	0.559**	0.517***	0.548*	0.466**	0.508***	0.445*
	[0.169]	[0.169]	[0.163]	[0.127]	[0.124]	[0.194]	[0.165]	[0.123]	[0.190]
Age=17	1.416	1.364	1.370	1.058	1.040	1.297	1.194	1.034	1.344
	[0.420]	[0.401]	[0.401]	[0.203]	[0.197]	[0.419]	[0.383]	[0.195]	[0.559]
Age=18	1.134	1.123	1.125	1.085	1.075	1.246	1.221	1.075	1.253
	[0.277]	[0.272]	[0.270]	[0.174]	[0.171]	[0.329]	[0.319]	[0.170]	[0.412]
Age=19	1.207	1.197	1.198	1.154	1.156	1.044	1.009	1.152	1.022
	[0.296]	[0.292]	[0.290]	[0.183]	[0.184]	[0.273]	[0.262]	[0.182]	[0.337]
B) Dependent Variable: Ever Given Birth									
Age=15	0.385	0.254	0.297	0.578	0.362	0.628	0.368	0.341	0.464
	[0.397]	[0.336]	[0.378]	[0.332]	[0.253]	[0.713]	[0.515]	[0.255]	[0.802]
Age=16	0.335**	0.223**	0.212**	0.561	0.423*	0.382	0.257*	0.402*	0.171*
	[0.183]	[0.146]	[0.143]	[0.217]	[0.191]	[0.244]	[0.181]	[0.190]	[0.162]
Age=17	0.414**	0.326***	0.311***	0.502**	0.444***	0.358**	0.247***	0.425***	0.167***
	[0.159]	[0.138]	[0.133]	[0.142]	[0.140]	[0.162]	[0.122]	[0.138]	[0.102]
Age=18	1.086	1.047	1.044	0.958	0.956	1.034	0.941	0.946	0.987
	[0.365]	[0.352]	[0.350]	[0.212]	[0.212]	[0.377]	[0.344]	[0.211]	[0.458]
Age=19	1.314	1.310	1.309	1.213	1.218	1.267	1.199	1.212	1.247
	[0.377]	[0.376]	[0.374]	[0.223]	[0.224]	[0.390]	[0.369]	[0.223]	[0.487]

Notes: The data come from the 2013 DHS. The sample includes 48 months on each side of the bubble in panel (I) and 84 months on each side of the bubble in panel (II). The number of observations is 2,325 in all cells in part (I), while it is 4,066 in all cells for ages up to 18 and 4040 in all cells for age 19 in panel (II). The policy dummy is one when year of birth is greater than 1987. Each cell comes from a separate logistic regression of ever-married status in panel (A) and of ever-given-birth status in panel (B) at the specified age on the policy variable and the specified time trends in month and year of birth; additional control variables include dummies for month of birth. In marriage estimations at age 13 in part (I), two months (February and March) are combined to generate a single dummy; and in fertility esimations at age 15, February and March as well as November and December are combined. Odds ratios and their standard errors are given as estimated parameters. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A16: Policy Effect on Marriage and First-Birth by Age with Dummies for Month of Birth – 10-Year Intervals around the Bubble (Sample B3)

	Single Time Trend						Split Time Trends		
Degree of Polynomial	One (1)	Two (2)	Three (3)	Four (4)	Five (5)	Six (6)	One (7)	Two (8)	Three (9)
A) Dependent Variable: Ever Married									
Age=13	0.832	1.140	1.212	1.283	1.146	0.727	1.077	0.954	0.162
	[0.539]	[0.735]	[1.074]	[1.149]	[1.370]	[0.963]	[0.672]	[1.180]	[0.414]
Age=14	0.622	0.850	0.783	0.568	0.703	0.360	0.780	0.386	0.155
	[0.317]	[0.424]	[0.543]	[0.436]	[0.689]	[0.399]	[0.384]	[0.388]	[0.282]
Age=15	0.465**	0.584*	0.496*	0.343**	0.301*	0.214**	0.550*	0.240**	0.097**
	[0.139]	[0.185]	[0.211]	[0.175]	[0.185]	[0.150]	[0.174]	[0.152]	[0.112]
Age=16	0.737	0.840	0.540**	0.362***	0.451**	0.448**	0.804	0.279***	0.485
	[0.143]	[0.168]	[0.156]	[0.111]	[0.180]	[0.183]	[0.162]	[0.103]	[0.306]
Age=17	1.204	1.335*	1.210	0.843	1.144	1.155	1.269	0.817	1.545
	[0.191]	[0.214]	[0.300]	[0.215]	[0.404]	[0.424]	[0.202]	[0.255]	[0.906]
Age=18	1.228	1.325**	1.183	0.794	1.012	1.520	1.274*	0.789	1.382
	[0.163]	[0.188]	[0.249]	[0.177]	[0.291]	[0.504]	[0.177]	[0.207]	[0.691]
Age=19	1.138	1.146	1.118	0.930	1.092	1.459	1.126	0.894	1.444
	[0.147]	[0.169]	[0.223]	[0.233]	[0.306]	[0.524]	[0.157]	[0.253]	[0.774]
B) Dependent Variable: Ever Given Birth									
Age=15	0.747	0.749	0.531	0.274	0.880	0.342	0.697	0.159	1.245
	[0.386]	[0.385]	[0.402]	[0.256]	[1.125]	[0.554]	[0.370]	[0.231]	[3.067]
Age=16	0.674	0.755	0.547	0.265**	0.418	0.255*	0.697	0.146**	0.156
	[0.230]	[0.264]	[0.251]	[0.147]	[0.289]	[0.196]	[0.250]	[0.111]	[0.217]
Age=17	0.763	0.731	0.472**	0.234***	0.299**	0.210***	0.700	0.137***	0.095**
	[0.173]	[0.192]	[0.158]	[0.099]	[0.143]	[0.117]	[0.186]	[0.071]	[0.088]
Age=18	1.104	1.245	1.062	0.615	0.928	1.304	1.170	0.557	1.544
	[0.206]	[0.247]	[0.297]	[0.196]	[0.370]	[0.624]	[0.228]	[0.209]	[1.105]
Age=19	1.200	1.238	1.373	1.067	1.125	1.873	1.196	1.133	1.315
	[0.180]	[0.211]	[0.307]	[0.310]	[0.364]	[0.816]	[0.197]	[0.369]	[0.855]

Notes: The data come from the 2013 DHS. The sample includes 10 years on each side of the bubble. The number of observations is 5,946 in all cells for all ages up to 15, 5,921 for age 16, 5,589 for age 17, 5,273 for age 18, and 4,970 for age 19. The policy dummy is one when year of birth is greater than 1987. Each cell comes from a separate logistic regression of ever-married status in panel (A) and of ever-given-birth status in panel (B) at the specified age on the policy variable and the specified time trends in month and year of birth; additional control variables include dummies for month of birth. Odds ratios and their standard errors are given as estimated parameters. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A17: Policy Effect on Marriage and First-Birth Hazard Rates with Dummies for Month of Birth – 4-Year and 7-Year Intervals around the Bubble (Samples B1 and B2)

	I) 4-Year Intervals			II) 7-Year Intervals					
	Single Time Trend		Split Trends	Single Time Trend				Split Time Trends	
Degree of Polynomial	One (1)	Two (2)	One (3)	One (4)	Two (5)	Three (6)	Four (7)	One (8)	Two (9)
A) Dependent Variable: Marriage status conditional on not being married until that age									
Age=14	0.308	0.215	0.227	0.703	0.561	0.283	0.119	0.563	0.086
	[0.395]	[0.297]	[0.311]	[0.576]	[0.494]	[0.377]	[0.192]	[0.490]	[0.170]
Age=15	0.319*	0.360	0.318	0.316***	0.272***	0.263*	0.244*	0.263**	0.183*
	[0.191]	[0.250]	[0.226]	[0.130]	[0.138]	[0.186]	[0.193]	[0.138]	[0.189]
Age=16	0.862	0.828	0.827	0.652	0.636	0.897	0.850	0.631	0.941
	[0.388]	[0.385]	[0.386]	[0.193]	[0.194]	[0.444]	[0.448]	[0.195]	[0.631]
Age=17	4.433***	5.087***	5.437***	2.479***	2.494***	3.691***	4.213***	2.546***	6.639***
	[1.751]	[2.081]	[2.323]	[0.682]	[0.696]	[1.552]	[1.914]	[0.725]	[4.193]
Age=18	0.836	0.836	0.834	1.116	1.114	1.138	1.148	1.115	1.021
	[0.290]	[0.290]	[0.289]	[0.278]	[0.279]	[0.438]	[0.432]	[0.279]	[0.480]
Age=19	1.135	1.127	1.124	1.214	1.276	0.672	0.621	1.255	0.602
	[0.475]	[0.484]	[0.483]	[0.355]	[0.356]	[0.311]	[0.306]	[0.349]	[0.388]
B) Dependent Variable: Birth status conditional on not giving birth until that age									
Age=15	0.209	0.055*	0.056*	0.284*	0.143**	0.199	0.074	0.118**	0.036
	[0.250]	[0.086]	[0.086]	[0.198]	[0.120]	[0.281]	[0.119]	[0.110]	[0.075]
Age=16	0.316*	0.204*	0.175*	0.529	0.434	0.291	0.199*	0.412	0.087**
	[0.209]	[0.174]	[0.159]	[0.273]	[0.275]	[0.220]	[0.179]	[0.270]	[0.107]
Age=17	0.546	0.477	0.454	0.470**	0.453**	0.327*	0.248**	0.440**	0.167**
	[0.302]	[0.276]	[0.267]	[0.173]	[0.176]	[0.193]	[0.162]	[0.177]	[0.136]
Age=18	2.476**	2.503**	2.545**	2.007**	1.995**	2.701**	2.742**	1.973**	3.735**
	[1.123]	[1.146]	[1.186]	[0.604]	[0.574]	[1.282]	[1.341]	[0.572]	[2.377]
Age=19	1.753	1.759	1.740	1.754**	1.786**	1.721	1.743	1.776**	1.705
	[0.715]	[0.702]	[0.689]	[0.486]	[0.479]	[0.774]	[0.772]	[0.473]	[0.935]

Notes: The data come from the 2013 DHS. The sample includes 48 months on each side of the bubble in panel (I) and 84 months on each side of the bubble in panel (II). The samples in panel (A) are restricted to individuals aged 14 and above, while the samples in panel (B) are restricted to individuals aged 15 and above. The number of observations is 12,533 in all columns in panel (A) of part (I), while it is 11,055 in all columns in panel (B) of part (I). In part (II), the number of observations is 21,769 in all columns in panel (A) and 19,221 in all columns in panel (B). The policy dummy is one when year of birth is greater than 1987. The estimates in each column come from a separate logistic regression of marriage hazard status in panel (A) and of birth hazard status in panel (B) on the interactions of the policy variable with each age dummy and the interactions of the specified time trends in month and year of birth with each age dummy. Other covariates included are month-of-birth dummies. Odds ratios and their standard errors are given as estimated parameters. The standard errors are clustered at the month-year of birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A18: Policy Effect on Marriage and First-Birth Hazard Rates with Dummies for Month of Birth – 10-Year Intervals around the Bubble (Sample B3)

	Single Time Trend						Split Time Trends		
Degree of Polynomial	One (1)	Two (2)	Three (3)	Four (4)	Five (5)	Six (6)	One (7)	Two (8)	Three (9)
A) Dependent Variable: Marriage status conditional on not being married until that age									
Age=14	0.495	0.675	0.590	0.253	0.420	0.224	0.601	0.169	0.278
	[0.332]	[0.447]	[0.574]	[0.299]	[0.650]	[0.404]	[0.388]	[0.262]	[0.687]
Age=15	0.370***	0.452**	0.363**	0.236**	0.162**	0.140**	0.430**	0.166**	0.063*
	[0.134]	[0.177]	[0.178]	[0.154]	[0.121]	[0.124]	[0.170]	[0.137]	[0.094]
Age=16	1.133	1.206	0.573	0.404**	0.728	0.887	1.173	0.324**	1.411
	[0.290]	[0.298]	[0.212]	[0.165]	[0.426]	[0.493]	[0.290]	[0.169]	[1.265]
Age=17	2.614***	2.827***	3.043***	2.320**	4.336***	4.477***	2.679***	2.865**	10.833***
	[0.635]	[0.655]	[1.040]	[0.843]	[2.100]	[2.196]	[0.626]	[1.327]	[9.509]
Age=18	1.329	1.427	1.048	0.843	0.815	1.275	1.403	0.786	0.733
	[0.273]	[0.311]	[0.305]	[0.285]	[0.316]	[0.636]	[0.295]	[0.307]	[0.528]
Age=19	1.084	1.330	0.881	0.657	1.007	2.097	1.277	0.553	7.408**
	[0.265]	[0.354]	[0.302]	[0.307]	[0.514]	[1.462]	[0.322]	[0.324]	[6.800]
B) Dependent Variable: Birth status conditional on not giving birth until that age									
Age=15	0.618	0.492	0.182*	0.072**	0.231	0.042*	0.466	0.013**	0.013
	[0.379]	[0.301]	[0.161]	[0.076]	[0.345]	[0.075]	[0.296]	[0.024]	[0.035]
Age=16	0.564	0.698	0.557	0.243*	0.218*	0.184*	0.631	0.132**	0.029**
	[0.257]	[0.337]	[0.328]	[0.187]	[0.186]	[0.168]	[0.311]	[0.132]	[0.049]
Age=17	0.894	0.861	0.388**	0.198***	0.235**	0.261*	0.844	0.114***	0.105*
	[0.285]	[0.294]	[0.169]	[0.111]	[0.154]	[0.185]	[0.289]	[0.079]	[0.122]
Age=18	1.845**	2.407***	2.318**	1.729	2.848*	4.950***	2.158***	1.919	9.260**
	[0.502]	[0.605]	[0.892]	[0.734]	[1.625]	[3.092]	[0.529]	[0.992]	[8.659]
Age=19	1.513*	1.782**	1.970**	1.765	1.473	2.712	1.704**	2.101	1.566
	[0.365]	[0.433]	[0.675]	[0.742]	[0.696]	[1.669]	[0.399]	[0.965]	[1.309]

Notes: The data come from the 2013 DHS. The sample includes 10 years on each side of the bubble. The samples in panel (A) are restricted to individuals aged 14 and above, while the samples in panel (B) are restricted to individuals aged 15 and above. The number of observations is 30,151 in all columns in panel (A), while it is 26,220 in all columns in panel (B). The policy dummy is one when year of birth is greater than 1987. The estimates in each column come from a separate logistic regression of marriage hazard status in panel (A) and of birth hazard status in panel (B) on the interactions of the policy variable with each age dummy and the interactions of the specified time trends in month and year of birth with each age dummy. Other covariates included are month-of-birth dummies. Odds ratios and their standard errors are given as estimated parameters. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A19: Policy Effect on Marriage and First-Birth by Age with Dummies for Birth-Month, NUTS-1 Regions, and Rural Area – Samples (B1) and (B2)

	I) 4-Year Intervals			II) 7-Year Intervals					
	Single Time Trend		Split Trends	Single Time Trend				Split Time Trends	
Degree of Polynomial	One (1)	Two (2)	One (3)	One (4)	Two (5)	Three (6)	Four (7)	One (8)	Two (9)
A) Dependent Variable: Ever Married									
Age=13	0.774	0.699	0.662	1.180	1.449	0.938	0.450	1.345	0.271
	[0.836]	[0.793]	[0.780]	[0.909]	[1.067]	[1.062]	[0.626]	[0.993]	[0.537]
Age=14	0.389	0.305	0.297	0.872	0.856	0.463	0.208	0.822	0.130
	[0.357]	[0.296]	[0.291]	[0.513]	[0.525]	[0.397]	[0.223]	[0.508]	[0.179]
Age=15	0.299**	0.284**	0.268**	0.461**	0.419**	0.305**	0.244**	0.409**	0.169**
	[0.150]	[0.157]	[0.149]	[0.165]	[0.169]	[0.177]	[0.153]	[0.167]	[0.135]
Age=16	0.533**	0.507**	0.497**	0.528***	0.491***	0.520*	0.453**	0.483***	0.437**
	[0.159]	[0.164]	[0.158]	[0.120]	[0.119]	[0.175]	[0.154]	[0.118]	[0.179]
Age=17	1.457	1.412	1.417	1.045	1.027	1.295	1.204	1.021	1.368
	[0.430]	[0.416]	[0.415]	[0.198]	[0.195]	[0.408]	[0.378]	[0.193]	[0.555]
Age=18	1.170	1.162	1.162	1.069	1.058	1.253	1.235	1.059	1.279
	[0.276]	[0.272]	[0.270]	[0.170]	[0.166]	[0.323]	[0.315]	[0.166]	[0.412]
Age=19	1.265	1.256	1.256	1.142	1.143	1.047	1.020	1.140	1.044
	[0.308]	[0.305]	[0.303]	[0.183]	[0.184]	[0.273]	[0.266]	[0.182]	[0.347]
B) Dependent Variable: Ever Given Birth									
Age=15	0.343	0.221	0.265	0.592	0.364	0.605	0.368	0.345	0.466
	[0.363]	[0.304]	[0.348]	[0.352]	[0.261]	[0.701]	[0.518]	[0.263]	[0.802]
Age=16	0.312**	0.214**	0.206**	0.556	0.414*	0.363	0.260*	0.396*	0.178*
	[0.173]	[0.143]	[0.140]	[0.215]	[0.190]	[0.233]	[0.182]	[0.190]	[0.166]
Age=17	0.382**	0.308***	0.295***	0.476***	0.421***	0.327**	0.234***	0.404***	0.159***
	[0.148]	[0.134]	[0.128]	[0.136]	[0.134]	[0.146]	[0.114]	[0.134]	[0.095]
Age=18	1.096	1.062	1.058	0.938	0.936	1.005	0.925	0.927	0.972
	[0.371]	[0.363]	[0.361]	[0.210]	[0.212]	[0.368]	[0.341]	[0.211]	[0.453]
Age=19	1.360	1.361	1.358	1.204	1.208	1.259	1.205	1.203	1.255
	[0.386]	[0.389]	[0.386]	[0.223]	[0.225]	[0.385]	[0.370]	[0.224]	[0.488]

Notes: The data come from the 2013 DHS. The sample includes 48 months on each side of the bubble in panel (I) and 84 months on each side of the bubble in panel (II). The number of observations is 2,325 all cells in part (I), while it is 4,066 in all cells for ages up to 18 and 4040 in all cells for age 19 in panel (II). The policy dummy is one when year of birth is greater than 1987. Each cell comes from a separate logistic regression of ever-married status in panel (A) and of ever-given-birth status in panel (B) at the specified age on the policy variable and the specified time trends in month and year of birth; additional control variables include month and place of birth dummies (NUTS-1 level region dummies and an urban/rural dummy). With 48-month intervals, certain months and regions are combined in panel (A) regressions at ages 13 and 14; and with 84-year intervals, certain regions are combined in panel (A) regressions for age 13. With 48-month intervals, certain months and regions are combined in panel (B) regressions at age 15 and certain regions are combined in panel (B) regressions at age 16; with 84-intervals, certain regions are combined in panel (B) regressions at age 15. Odds ratios and their standard errors are given as estimated parameters. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A20: Policy Effect on Marriage and First-Birth by Age with Dummies for Birth-Month, NUTS-1 Regions, and Rural Area – Sample (B3)

	Single Time Trend						Split Time Trends		
Degree of Polynomial	One (1)	Two (2)	Three (3)	Four (4)	Five (5)	Six (6)	One (7)	Two (8)	Three (9)
A) Dependent Variable: Ever Married									
Age=13	0.777	1.080	1.162	1.314	1.250	0.833	1.029	1.024	0.217
	[0.515]	[0.716]	[1.058]	[1.197]	[1.544]	[1.131]	[0.658]	[1.267]	[0.553]
Age=14	0.610	0.815	0.761	0.576	0.705	0.386	0.753	0.407	0.173
	[0.311]	[0.410]	[0.522]	[0.437]	[0.682]	[0.422]	[0.373]	[0.400]	[0.307]
Age=15	0.441***	0.543*	0.466*	0.335**	0.293**	0.219**	0.514**	0.243**	0.104**
	[0.133]	[0.174]	[0.198]	[0.170]	[0.180]	[0.153]	[0.165]	[0.151]	[0.118]
Age=16	0.702*	0.791	0.506**	0.347***	0.438**	0.452**	0.760	0.272***	0.499
	[0.137]	[0.159]	[0.145]	[0.105]	[0.171]	[0.182]	[0.153]	[0.099]	[0.312]
Age=17	1.163	1.276	1.194	0.853	1.145	1.186	1.217	0.840	1.547
	[0.184]	[0.205]	[0.293]	[0.216]	[0.399]	[0.436]	[0.193]	[0.259]	[0.915]
Age=18	1.194	1.274*	1.170	0.814	1.000	1.527	1.229	0.816	1.294
	[0.159]	[0.180]	[0.245]	[0.180]	[0.286]	[0.515]	[0.171]	[0.213]	[0.663]
Age=19	1.112	1.124	1.098	0.947	1.096	1.522	1.105	0.908	1.401
	[0.146]	[0.169]	[0.221]	[0.241]	[0.311]	[0.566]	[0.158]	[0.265]	[0.799]
B) Dependent Variable: Ever Given Birth									
Age=15	0.728	0.706	0.512	0.275	0.869	0.371	0.660	0.171	1.315
	[0.385]	[0.372]	[0.395]	[0.261]	[1.131]	[0.603]	[0.359]	[0.245]	[3.174]
Age=16	0.648	0.710	0.527	0.265**	0.411	0.268*	0.657	0.153**	0.169
	[0.225]	[0.253]	[0.244]	[0.149]	[0.288]	[0.207]	[0.240]	[0.117]	[0.231]
Age=17	0.726	0.679	0.445**	0.230***	0.281***	0.209***	0.654	0.140***	0.093**
	[0.168]	[0.183]	[0.149]	[0.098]	[0.136]	[0.117]	[0.178]	[0.073]	[0.086]
Age=18	1.061	1.185	1.037	0.624	0.906	1.274	1.117	0.574	1.402
	[0.202]	[0.241]	[0.293]	[0.205]	[0.367]	[0.629]	[0.223]	[0.220]	[1.036]
Age=19	1.181	1.218	1.362	1.081	1.103	1.905	1.179	1.157	1.224
	[0.180]	[0.212]	[0.307]	[0.320]	[0.363]	[0.848]	[0.198]	[0.387]	[0.823]

Notes: The data come from the 2013 DHS. The sample includes 10 years on each side of the bubble. The number of observations is 5,946 in all cells for all ages up to 15, 5,921 for age 16, 5,589 for age 17, 5,273 for age 18, and 4,970 for age 19. The policy dummy is one when year of birth is greater than 1987. Each cell comes from a separate logistic regression of ever-married status in panel (A) and of ever-given-birth status in panel (B) at the specified age on the policy variable and the specified time trends in month and year of birth; additional control variables include month and place of birth dummies (NUTS-1 level region dummies and a rural/urban dummy). In marriage estimations at age 13, some regions are combined. Odds ratios and their standard errors are given as estimated parameters. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A21: Policy Effect on Marriage and First-Birth Hazard Rates with Birth-Month, NUTS-1 Region, and Rural Dummies – Samples (B1) and (B2)

	I) 4-Year Intervals			II) 7-Year Intervals					
	Single Time Trend		Split Trends	Single Time Trend				Split Time Trends	
Degree of Polynomial	One (1)	Two (2)	One (3)	One (4)	Two (5)	Three (6)	Four (7)	One (8)	Two (9)
A) Dependent Variable: Marriage status conditional on not being married until that age									
Age=14	0.316	0.224	0.236	0.700	0.556	0.273	0.122	0.561	0.087
	[0.402]	[0.309]	[0.322]	[0.574]	[0.493]	[0.360]	[0.195]	[0.491]	[0.170]
Age=15	0.324*	0.372	0.327	0.311***	0.267***	0.254*	0.244*	0.260***	0.184*
	[0.195]	[0.257]	[0.231]	[0.129]	[0.136]	[0.180]	[0.191]	[0.136]	[0.187]
Age=16	0.880	0.849	0.847	0.629	0.614	0.876	0.838	0.610	0.938
	[0.390]	[0.390]	[0.390]	[0.185]	[0.186]	[0.425]	[0.432]	[0.188]	[0.614]
Age=17	4.625***	5.273***	5.638***	2.483***	2.500***	3.710***	4.217***	2.552***	6.654***
	[1.823]	[2.143]	[2.386]	[0.685]	[0.700]	[1.566]	[1.910]	[0.727]	[4.159]
Age=18	0.867	0.867	0.865	1.106	1.105	1.131	1.140	1.106	1.021
	[0.297]	[0.297]	[0.296]	[0.277]	[0.279]	[0.435]	[0.429]	[0.278]	[0.484]
Age=19	1.195	1.188	1.183	1.210	1.271	0.669	0.629	1.251	0.615
	[0.504]	[0.513]	[0.512]	[0.360]	[0.360]	[0.313]	[0.310]	[0.354]	[0.396]
B) Dependent Variable: Birth status conditional on not giving birth until that age									
Age=15	0.207	0.056*	0.058**	0.277*	0.138*	0.188	0.072	0.115**	0.036
	[0.247]	[0.088]	[0.088]	[0.195]	[0.117]	[0.264]	[0.116]	[0.108]	[0.073]
Age=16	0.313*	0.206*	0.178**	0.518	0.423	0.277*	0.196*	0.403	0.086**
	[0.209]	[0.177]	[0.162]	[0.267]	[0.270]	[0.210]	[0.177]	[0.266]	[0.106]
Age=17	0.543	0.480	0.457	0.457**	0.441**	0.313**	0.241**	0.429**	0.163**
	[0.297]	[0.276]	[0.266]	[0.168]	[0.172]	[0.182]	[0.155]	[0.172]	[0.131]
Age=18	2.529**	2.543**	2.584**	1.982**	1.968**	2.628**	2.665**	1.947**	3.599**
	[1.156]	[1.171]	[1.210]	[0.605]	[0.575]	[1.263]	[1.315]	[0.573]	[2.305]
Age=19	1.778	1.782	1.760	1.726*	1.756**	1.649	1.684	1.748**	1.632
	[0.722]	[0.706]	[0.692]	[0.483]	[0.477]	[0.737]	[0.738]	[0.470]	[0.883]

Notes: The data come from the 2013 DHS. The sample includes 48 months on each side of the bubble in panel (I) and 84 months on each side of the bubble in panel (II). The samples in panel (A) are restricted to individuals aged 14 and above, while the samples in panel (B) are restricted to individuals aged 15 and above. The number of observations is 12,533 in all columns in panel (A) of part (I), while it is 11,055 in all columns in panel (B) of part (I). In part (II), the number of observations is 21,769 in all columns in panel (A) and 19,221 in all columns in panel (B). The policy dummy is one when year of birth is greater than 1987. The estimates in each column come from a separate logistic regression of marriage hazard status in panel (A) and of birth hazard status in panel (B) on the interactions of the policy variable with each age dummy and the interactions of the specified time trends in month and year of birth with each age dummy. Other covariates include month dummies, dummies for 13 regions, and a rural/urban dummy. Odds ratios and their standard errors are given as estimated parameters. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A22: Policy Effect on Marriage and First-Birth Hazard Rates with Birth-Month, NUTS-1 Region, and Rural Dummies – Sample (B3)

	Single Time Trend						Split Time Trends		
Degree of Polynomial	One (1)	Two (2)	Three (3)	Four (4)	Five (5)	Six (6)	One (7)	Two (8)	Three (9)
A) Dependent Variable: Marriage status conditional on not being married until that age									
Age=14	0.482	0.648	0.575	0.253	0.418	0.234	0.578	0.173	0.288
	[0.323]	[0.432]	[0.558]	[0.298]	[0.642]	[0.416]	[0.376]	[0.266]	[0.697]
Age=15	0.357***	0.430**	0.354**	0.234**	0.161**	0.144**	0.408**	0.168**	0.066*
	[0.128]	[0.169]	[0.175]	[0.153]	[0.120]	[0.126]	[0.163]	[0.139]	[0.097]
Age=16	1.079	1.146	0.553	0.394**	0.709	0.885	1.115	0.321**	1.408
	[0.275]	[0.283]	[0.203]	[0.160]	[0.410]	[0.483]	[0.276]	[0.165]	[1.240]
Age=17	2.511***	2.720***	3.091***	2.368**	4.313***	4.521***	2.577***	2.978**	10.572***
	[0.608]	[0.628]	[1.057]	[0.858]	[2.079]	[2.217]	[0.601]	[1.371]	[9.204]
Age=18	1.287	1.376	1.054	0.859	0.809	1.283	1.354	0.811	0.702
	[0.266]	[0.302]	[0.310]	[0.291]	[0.317]	[0.643]	[0.287]	[0.317]	[0.515]
Age=19	1.061	1.309	0.878	0.671	1.019	2.265	1.255	0.567	7.297**
	[0.262]	[0.354]	[0.304]	[0.314]	[0.519]	[1.567]	[0.321]	[0.331]	[6.881]
B) Dependent Variable: Birth status conditional on not giving birth until that age									
Age=15	0.601	0.469	0.176**	0.070**	0.220	0.042*	0.444	0.013**	0.013
	[0.371]	[0.289]	[0.156]	[0.075]	[0.328]	[0.076]	[0.285]	[0.024]	[0.037]
Age=16	0.546	0.666	0.542	0.239*	0.210*	0.185*	0.604	0.133**	0.030**
	[0.248]	[0.323]	[0.320]	[0.185]	[0.180]	[0.169]	[0.299]	[0.133]	[0.050]
Age=17	0.861	0.818	0.379**	0.197***	0.224**	0.258*	0.804	0.116***	0.101**
	[0.274]	[0.281]	[0.166]	[0.110]	[0.146]	[0.182]	[0.276]	[0.080]	[0.116]
Age=18	1.796**	2.336***	2.278**	1.731	2.723*	4.760**	2.099***	1.921	8.311**
	[0.491]	[0.595]	[0.886]	[0.743]	[1.563]	[2.998]	[0.520]	[0.999]	[7.839]
Age=19	1.490	1.748**	1.929*	1.721	1.388	2.610	1.675**	2.052	1.369
	[0.362]	[0.429]	[0.665]	[0.723]	[0.655]	[1.603]	[0.395]	[0.948]	[1.155]

Notes: The data come from the 2013 DHS. The sample includes 10 years on each side of the bubble. The samples in panel (A) are restricted to individuals aged 14 and above, while the samples in panel (B) are restricted to individuals aged 15 and above. The number of observations is 30,151 in all columns in panel (A), while it is 26,220 in all columns in panel (B). The policy dummy is one when year of birth is greater than 1987. The estimates in each column come from a separate logistic regression of marriage hazard status in panel (A) and of birth hazard status in panel (B) on the interactions of the policy variable with each age dummy and the interactions of the specified time trends in month and year of birth with each age dummy. Other covariates include month dummies, dummies for 13 regions, and a rural/urban dummy. Odds ratios and their standard errors are given as estimated parameters. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.

Table A23: Effect of the New Civil Code on Marriage Hazard Rates by Age

A) Dependent Variable: Marriage status conditional on not being married until that age						
	Single Time Trend				Split Time Trends	
Degree of Polynomial	One (1)	Two (2)	Three (3)	Four (4)	One (5)	Two (6)
II) 2-Year Intervals on both sides of the cut-off (1983-84 vs. 1985-86 Birth Cohorts)						
Age=14	0.966	0.759	0.698	0.751	29.905	3.111
	(0.100)	(0.221)	(0.213)	(0.389)	(74.238)	(8.361)
Age=15	0.887***	0.950	0.943	1.293	1.376	3.141
	(0.031)	(0.128)	(0.125)	(0.313)	(1.031)	(3.099)
Age=16	1.035	1.162	1.166	1.176	2.183	2.241
	(0.035)	(0.165)	(0.169)	(0.318)	(2.171)	(3.555)
Age=17	1.100***	1.298*	1.294*	1.106	0.880	2.301
	(0.034)	(0.182)	(0.183)	(0.304)	(0.879)	(3.443)
Age=18	1.003	1.159	1.195*	1.348*	0.549	1.340
	(0.030)	(0.104)	(0.113)	(0.203)	(0.408)	(1.260)
Age=19	1.009	1.096	1.096	0.938	0.594	0.684
	(0.022)	(0.113)	(0.113)	(0.193)	(0.390)	(0.594)
II) 4-Year Interval before the cut-off, 2-Year after (1981-84 vs. 1985-86 Birth Cohorts)						
Age=14	1.013	0.905	0.966	0.942	23.130	18.042
	(0.053)	(0.120)	(0.243)	(0.256)	(44.209)	(47.796)
Age=15	0.927***	0.891**	0.857	0.858	2.375	2.316
	(0.025)	(0.045)	(0.098)	(0.100)	(1.629)	(2.074)
Age=16	0.991	1.040	1.206	1.232	0.970	1.038
	(0.021)	(0.056)	(0.154)	(0.158)	(0.808)	(1.392)
Age=17	1.018	1.176***	1.267**	1.312**	0.252	0.781
	(0.020)	(0.059)	(0.140)	(0.155)	(0.220)	(0.895)
Age=18	1.018	0.984	1.022	1.022	0.933	0.462
	(0.016)	(0.038)	(0.076)	(0.077)	(0.550)	(0.365)
Age=19	0.973*	1.061	1.091	1.118	0.384	0.578
	(0.015)	(0.042)	(0.084)	(0.094)	(0.231)	(0.414)

Notes: The data come from the 2013 DHS. The sample includes 24 months on each side of the cut-off. The number of observations is 6,327 in all columns in panel (A) and 9,641 in all columns in panel (B). The estimates in each column come from a separate logistic regression of marriage hazard status on the interactions of the intensity of the exposure to the new civil code with each age dummy and the interactions of the specified time trends in month and year of birth with each age dummy, dummies for each birth-month, dummies for the NUTS-1 level region of birth, and a dummy for rural region of birth. Odds ratios and their standard errors are given as estimated parameters. The standard errors are clustered at the month-and-year-of-birth level. Statistical significance is *** at 1 percent level, ** at 5 percent level, * at 10 percent level.