'For the Love of the Republic' Education, Secularism, and Empowerment*

Selim Gulesci† **Erik Meyersson**‡

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Abstract

We exploit a change in compulsory schooling laws, as part of a large-scale project of secular modernization in Turkey, to estimate the causal effects of education on religiosity and women's empowerment. A new law implemented in 1998 resulted in individuals born after a specific date to be more likely to complete at least 8 years of schooling while those born earlier could drop out after 5 years. This allows the implementation of a Regression Discontinuity (RD) Design and the estimation of meaningful causal estimates of schooling. We show that the reform resulted in a one-year increase in years of schooling on average among women. Over a period of ten years, this education increase resulted in women reporting lower levels of religiosity, greater decision rights over marriage and contraception, as well as higher household durables consumption. In contrast, we document generally small and insignificant average effects of education on labor force participation, timing of marriage and early fertility. These effects are heterogeneous, depending on measures for the severity of constraints to educational participation. Our findings demonstrate that education may empower women across a wide spectrum of a Muslim society, yet depending on pre-reform constraints to participation, its effects may not be strong enough to fully overcome participation constraints in subsequent voluntary education and the labor force.

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†Department of Economics and IGIER, Bocconi University. Email: selim.gulesci@unibocconi.it.

‡Stockholm Institute of Transition Economics (SITE), Stockholm School of Economics. Email: erik.meyersson@hhs.se.

"Our women are now seen as serving no useful purpose to mankind other than having children; they are considered simply as serving for pleasure, like musical instruments or jewels. But they constitute half and perhaps more than half of our species. Preventing them from contributing to the sustenance and improvement of others by means of their efforts infringes the basic rules of public cooperation to such a degree that our national society is stricken like a human body that is paralyzed on one side. Yet women are not inferior to men in their intellectual and physical capacities... Many evil consequences result from this position of women, the first being that it leads to a bad upbringing for their children." – Namık Kemal, *Tasvir-i Efkâr*, 1867.[1]

1 Introduction

Does expansion of public education empower women? A large literature documents the effects of education on women's economic and social outcomes in developed countries, but we know less about its effects in Muslim societies. Despite the expansion of female education in many majority-Muslim countries, women's rights and female labor force participation remain at low levels (UNDP [70], Doepke et al [22]). The lack of a corresponding upward trend in female labor force participation suggests limits to expanding education as a means of improving women's rights in Muslim societies.[2]

Many factors contribute to the severe gender inequality in Muslim societies, but most posit a nexus of patriarchal culture, strong religious values, and restricting social norms as key factors. Historically, Lewis [49] claims women's status was "probably the most profound single difference" between Muslim and Christian civilizations. In more contemporary cross-country studies, Fish [27] documents a negative cross-country correlation between having an "Islamic religious tradition" and female empowerment, while Barro and McCleary [6] also show that Muslim countries tend to exhibit higher degrees of religious participation and beliefs. Comparing the effects of a business training program on female entrepreneurship among Hindu and Muslim women in India, Field et al [26] find evidence in line with significantly stricter constraints to female labor force participation among Muslim women. To the extent that barriers to entry due to religious values restrain women's rights in Muslim societies, an integral outcome of women's empowerment is therefore a woman's ability to independently assert her own beliefs. Consequently, examining education's effects on religious outcomes is informative of women's rights as long as this is accompanied by more decision rights.

Another distinct aspect of expanding public education in the Muslim world is the political manner in which reforms were implemented. The massive expansions of compulsory schooling under "authoritarian rulers" (Atabaki and Zürcher [5]) like Atatürk in Turkey (Mango [53]), Bourghiba in Tunisia (Allman [2]), Nasser in Egypt (Boktor [11]), and Reza Shah Pahlavi in Iran (Cronin [17]) all shared similar

[1] English translation in B. Lewis, A Middle East Mosaic: Fragments of Life, Letters and History (New York: 2000), p. 192.

[2] The labor market channel has been emphasized as conducive to both development and gender inequality (Goldin [30], Doepke and Tertilt [21], Duflo [23], World Bank [75]) as well as the severe degrees of gender inequality in the Middle East region (World Economic Forum [73]). Education has also been shown to have many important non-pecuniary effects outside the labor market and to the extent that these effects help empower women, they may constitute alternative mechanisms through which education may empower women even in the absence of large effects on their labor. However, most of this research is based on evidence from societies that are not majority-Muslim, where women work to a higher degree (Oreopolous and Salvanes [60]).

traits; the strongly secularizing influences in the new education system, the focus on female educational participation, the authoritarian manner in which many reforms were implemented, and the extent to these elites sought to modernize a largely poor and pious populace. In the case of Nasser and Reza Shah, the education reforms were preceded by military coups, and in all of the reform episodes, nation-building currents were strong.

There are several similarities with these historical episodes and the events that led Turkey to expand its compulsory schooling in the 1990s. In 1997, a military coup ousted a controversial Islamist-led coalition government, and seen explicitly as a way to break the hold of religiously conservative views among the youth, the military then pushed to extend public compulsory schooling for an additional three years from previously only covering primary school to also include junior high school. Among the least likely to attend junior high school were the poor, religious conservatives and ethnic Kurds, and these communities became the main targets of the reform.

In order to examine the causal effects of education on religiosity and women's empowerment, we exploit this extension of compulsory schooling in Turkey. Previous studies have used compulsory schooling laws in Western countries to estimate returns to education in the labor market (Angrist and Krueger [4], Oreopoulos [59]), in terms of health and fertility behaviors (McCrary and Royer [57], Lleras-Muney [50], Black et al [8]) as well as other outcomes. We use an RD strategy similar to McCrary and Royer [57] and Oreopoulos [59]) which provides meaningful causal parameters for the effects of an additional year of schooling on decision rights, religiosity, marriage, fertility, and labor.

When Turkey's parliament passed the new law to increase compulsory schooling from 5 to 8 years, the interaction of this new law and another pertaining to school starting age in Turkey implied that individuals born after September 1986 were more likely to complete 8 years of schooling, whereas those born earlier were more likely to drop out after 5 years. We adopt a regression discontinuity (RD) design assigning treatment based on whether an individual's month and year of birth was before or after September 1986, using this treatment as an instrument for years of education to estimate its effects on empowerment-related outcomes. As such, our identification strategy entails comparing cohorts born one month apart and relies on the assumption that being born on either side of this date is as-good-as random.

We use the sample of ever-married women from the 2008 Turkish Demographic Health Survey (TDHS) to estimate effects on outcomes 10 years after the law was implemented. Analyzing outcomes for ever-married women means focusing the RD treatment effects on a subset of the population that tends to be demonstratively poorer and more socially conservative – the very subpopulation that the reform was aimed at. In a comparison of ever- and never-married women, we find the reform mainly affecting education among the former. And as we demonstrate no average effects on selection into the ever-married sample, excluding non-married women from the analysis effectively implies a greater focus on compliers with the reform. Moreover, even though the extension of compulsory secular schooling meant removing previous religious options for schooling, these were rarely attended by women and we find no effect of the reform reducing religious forms of education, and effects are as prescient even in places where religious schooling options were already scarce. This leads us to interpret the effects for women as the effect of increasing years of public schooling as opposed to a switch in schooling from religious to secular.

In order to estimate the effects of education on religiosity and empowerment, we first test whether the reform led to a significant increase in education. We find that the reform increased female years of schooling by one year, and completion rates for junior high (lower secondary) and high school by 24 and 8 percentage points (ppt) respectively. As we do not find a significant impact of the reform on men's schooling (mainly because the average man's schooling in Turkey around the age threshold was already at a relatively high level), the reform effectively served to reduce Turkey's education gender gap.

Second, our RD estimates reveal that an additional year of schooling had significant secularizing effects, especially with regards to public expressions of religiosity. Ten years after the reform was implemented women were 10 percent (8 ppt) less likely to wear a headscarf, 22 ppt (10 ppt) less likely to have attended a Qur'an study course and 18 percent (7 ppt) less likely to pray regularly. This secularizing effect can also be seen in a weighted index of the different religiosity measures that we have in the dataset.

Third, we find no evidence that this additional year of schooling had any meaningful effects on early fertility, child health, marriage timing, or spouse quality. However, we do find that it affected women's decision rights in terms of marriage and fertility decisions. A reform-induced year of schooling resulted in a 20 percent (10ppt) increase in the likelihood of having a say in the marriage decision and a 12 percent (10ppt) increase in having a say in the use of contraceptives. We further find a reducing effect of schooling on the likelihood that a brideprice was received by women's parents from their husband's family upon their wedding, further suggesting that education had a progressive impact on how marriages were conducted.

Fourth, we document less pronounced and largely imprecise impacts on women's labor market outcomes than standard correlations would indicate. These estimates tend to be imprecisely estimated and sensitive to the specification, although we find positive effects of education on self-employment, suggesting the possibility of part-time forms of labor that interfere less with women's role as homemakers. At the same time, we show significant positive effects of schooling on ownership of household assets, largely driven by appliances related to home production.

Altogether, our results indicate significant empowering effects of public education, albeit with muted effects on the labor market or assortative matching in the marriage market as the main channel of empowerment. However, an examination of heterogenous effects reveal several diverging impacts of the reform depending on the respondents' mothers' education, which we interpret as a proxy for the severity of constraints facing female participation in education. Among these constraints is the combination of coeducational classes and the headscarf ban for women in secular schools, as well as the exclusive teaching in Turkish for ethnic minorities. Mother's education separates individuals not just in terms of human capital accumulation but also in terms of their religiosity levels – making it a useful indicator of the degree to which respondents belong to the relatively more poor and pious communities – as well as their likelihood of being ethnic Kurds, factors correlated with higher constraints to female educational participation. Our results support the existence of heterogeneous effects, not just with regards to education, but also marriage timing and labor force participation. On the one hand, for women from more constrained backgrounds (with less educated mothers), the reform increased their education up to the compulsory minimum but had no effect on subsequent voluntary high school education. For this subsample of women, the increase in schooling led to an earlier marriage with no commensurate effects

on spousal quality, but an overall increase in female labor force participation, and a shift from unpaid labor to self-employment. On the other hand, for women from less constrained backgrounds (with more educated mothers), the reform had considerable effects on completing both junior high school as well as high school, in combination with a positive (albeit imprecise) effect on age at first marriage, husband's education, but without affecting female labor force participation.

We interpret this heterogeneity in the reform's impacts as evidence of the importance of female participation constraints in education and other aspects in a socially conservative Muslim country like Turkey. The reform was not enough to keep heavily constrained women in school beyond the compulsory level, and without continued high school enrollment, this left little room for social advancement except marriage with a limited pool of husbands, possibly sped up by the relatively higher education levels these women held in their segment of the marriage market. Participation in the labor force (through self-employment) may have been the result of a shrinking interspousal education gap as much as economic necessity. For the latter, less constrained group of women, being able to stay in school longer resulted in a delay in marriage, as well as gaining access to a pool of better educated husbands. To the extent that spousal education and wealth are correlated, the wealth effect of forming a better match in the marriage market may explain their decision to remain as homemakers, thus avoiding a highly gender-discriminatory labor market.

With a few exceptions, research on the consequences of female education in Muslim societies remains limited. Lavy and Zablotsky [46] show that an increase in female schooling among the Arab population in Israel led to a fall in fertility, strong assortative matching in the marriage market but without an impact on female labor force participation. Breierova and Duflo [12] exploit a school construction program in Indonesia to estimate the effect of education on age at marriage, fertility and child mortality. Alam *et al* [1] analyze the effects of a female-targeted conditional cash transfer program in Pakistan on girls' schooling, age at first marriage and fertility decisions. Osili and Long [61] estimate the effects of the universal primary education program in Nigeria on fertility.

Specifically related to the 1997 education reform in Turkey, Dayıoğlu, Kırdar, and Koç. [18], Dincer et al [20], and Gunes [33] document reducing effects of education on fertility, delayed age of marriage, and some improvements in infant health using the same DHS survey as we do. These studies employ instrumental variable or difference-in-difference techniques using variation by annual, or more aggregate, birth cohorts without fully implementing the regression discontinuity design. Our own results suggest more muted average effects of education on fertility and marriage timing, and is thus more consistent with recent work by McCracy and Royer [57], who, in an RD design using school-of-entry birth dates, find "generally small, but possibly heterogeneous effects" of education on fertility. We also demonstrate how proper implementation of the RD design switches the signs of many estimates on early fertility-related outcomes compared to standard correlation estimates [3].

[3]Dincer et al [20] examine the reform's effects on fertility in an instrumental variable strategy, using the interaction of the 1986-1990 birth cohort, the number teachers in a region, and whether the survey year was 2008 (instead of 2003) as an instrument for education. With this empirical strategy, the authors find reducing effects on the number of pregnancies but no evidence that it changed gender attitudes. The difference-in-difference setup in this strategy requires relatively strong assumptions that outcomes for younger cohorts in high-teacher regions do not vary between 2003 and 2008 for other reasons than the 1997 education reform. The authors' strategy of interacting with number of teachers also risk picking up average changes in outcomes in the wealthier regions of Turkey. Another set of papers uses variation in birth year to estimate the effect of education on teenage fertility ((Dayıoğlu, Kırdar, and Koç. [18] and Gunes [33])) and political as sell

A growing literature also suggests positive effects of education on views on empowerment and gender. For example, Basu and King [7] find that education tends to have positive effects on Bangladeshi women's participation in attending political meetings. A recent paper by Friedman *et al* [28] examines the educational effects on girls' attitudes related to social issues and finds that schooling has limited impact on girls' pro-democratic, secular, and gender-related attitudes in Kenya. This also links with research on broader non-pecuniary effects of education, recently summarized by Oreopolous and Salvanes [60] and Lochner [51]. Our own results suggest important positive effects of education on decision rights related to spousal and contraceptive use, and to some extent also improvements in views on gender roles and domestic violence.

With regards to education's impact on secularism, modernization theorists have long argued for a secularizing effect (Stark [66], Swatos and Christiano [67]), whereby an increasingly educated population should over time become less dependent on superstitious and supernatural beliefs, and in extension less religious. This may fit the experience in several Western countries but in the Muslim world the association between education and religion seems to have experienced trends in the opposite direction. Women in Muslim countries have made significant gains in educational participation resulting in a narrowing of the gender gap in education (UNDP [70]).[4] Previous empirical research has found mixed results on the relationship between education and religion. Whereas cross-country evidence suggests a positive correlation (Barro and McCleary [6]), others have documented negative ones (Iannaccone [39] and Deaton [19]) (Hout and Fischer [37]). The use of micro-studies without a clear identification strategy is further confounded by the importance of religious institutions as social networks (Sacerdote and Glaeser [65]). Recent work by Hungerman [38] shows negative effects of education on religious affiliation in Canada. Yet, to this date there is little research providing causal identification of the effect of education on broader measures of religious expressions in a Muslim country.

Finally, we contribute to the literature on the role of public education systems as instruments of indoctrination and belief formation (Greif and Tadelis [31], Kremer and Sarychev [44], Lott [52], Pritchett [64]). A common feature of public education systems around the world is the degree to which they are designed not just to teach skills, but also to instill certain beliefs and values. As such, education reform is rarely neutral of the political environment in which it is produced. In many of the large expansions of public education in modern history, state elites have used the school as social cement with which it might mold its youth into a more homogenous community. From this perspective, our study of the 1997 education reform in Turkey is an unprecedented opportunity to study the effects of an education reform motivated by a desire to promote secular modernization, an attempt to rid the

as religious beliefs (Cesur and Mocan [16]). These designs have in common the comparison of larger groups of age cohorts born after 1986 with those born after to generate instruments for education, requiring the assumption that confounding factors can be controlled for by observable controls and common trends in the year of birth. In Cesur and Mocan [16] the authors furthermore exclude the entire 1986 birth cohort, pushing the comparison of treatment and control groups even further away in comparability. Invoking the weaker assumptions of our RD design using month-year of birth leads to results that in several cases fail to confirm those from these other studies and in some are even of the opposite sign, one example being measures of early fertility, another being attitudes towards gender. As such, our analysis suggest some caution in inferring causal effects of education from comparisons across larger birth cohorts. Moreover, the scope of our outcomes, our unique focus on decision rights, as well as the heterogeneity in participation constraints to education, also allows us a deeper examination of the mechanisms involved.

[4] At the same time, an ongoing process commonly referred to as the 'Islamic revival' (Lapidus [45]) has led to an increase in expressions of religiosity both in terms of demand for religious education as well as the practice of wearing a headscarf for women.

youth of unwanted Islamic influences as much as the desire to teach skills. To the extent that one of the purposes of the education reform was to instill the young with more secular values through social engineering, the clear secularizing effects on especially public expressions of religiosity are a testament of its success in this regard.

Overall, our paper adds to the research literature by providing meaningful causal parameters from an RD design for the effect of a year of schooling on both empowerment and religious outcomes for young ever-married women in Turkey. The findings point to a set of returns to schooling that take into context the socially conservative nature of the Turkish society where policies to increase schooling ultimately seem to improve women's status but are also unable to meaningfully break down barriers to some channels which might further improve women's outcomes. Despite the observed improved outcomes, in Turkey and perhaps also other Muslim societies, education may not alone serve as a magic bullet toward full emancipation.

The rest of the paper is organized as follows: Section 2 provides information on the education system in Turkey and the political context within which the policy change took place; Section 3 presents the data used in the study, our empirical design and validity checks on our identifying assumptions; Section 4 presents the empirical results, Section 5 discusses their implications, and Section 6 concludes.

2 The Political Economy of Education in Turkey

In Turkey, instilling certain values in youth is an integral component of the national education system, dating back to reforms implemented by Atatürk in the 1920s. These reforms were part of a broader push to modernize a stagnant Ottoman Empire into a modern Western-oriented republic based on secularism and nationalism. Among these, reforms to the education system brought all educational instruction under government control. Even today, the Ministry of Education describes education as "the process of change in behaviors of individuals."[5]

Previous education reforms in Turkey have had important consequences for women. Even though female schooling existed before Atatürk, such instances provided exceptions to the norm that women did not participate in education (Lewis [49]). A groundbreaking law made primary school compulsory for both girls and boys as well as a new civil code resulting in equal inheritance rights and ending divorce at husbands' discretion (Mango [53]). Yet despite the reforms' importance, given the initial level of women's rights in the early 20th century, their long-running effects have been limited. Albeit significant gains in education over the past decades, labor force participation remains low.[6] The prevalence of forced marriages, often both unofficial and under the legal age, as well as domestic abuse and honor killings, remain acute problems (Human Rights Watch [36]). As a consequence, Turkey systematically scores lower than western countries and similar to other majority-Muslim countries on international gender equality rankings.

In this state of poor women's rights, education could potentially play an important role in facilitating social mobility and improved living conditions, yet participation in voluntary education remains a challenge for many women. Turkey's strictly secular national education system often contrasts with a

[5] "National Education at the Beginning of 2001," Republic of Turkey Ministry of Education

[6] For more on women's participation in Turkey as well as comparisons with other countries, see The Gender Gap, World Economic Forum

broadly shared culture of social and religiously conservative views on the means and ways of female participation. Increasing general schooling among the country's poorer and more pious population, especially for girls, is therefore politically sensitive. A central identification challenge is thus estimating the effect of education on women's empowerment independent of preexisting factors correlated with women's rights.

2.1 Political Islam and the 1997 'Basic Education Law'

Before 1997, Turkey's basic education system consisted of three components; 5 years of primary school (*İlkokul*), 3 years of junior high school (*Ortaokul*), and 3 years of high school (*Lise*). Of these three, primary school was compulsory and the other two voluntary. For both junior high school as well as high school, students had two choices: secular or vocational schools, where the latter included religious (*imam-hatip*) schools. This allowed students after primary school the option of not just dropping out, but also to continue studies focusing on religious instruction. All education is co-educational and exclusively in Turkish. Although the official law stated that women could not wear a headscarf in any public institution (i.e. neither in secular nor religious (*imam-hatip*) schools), in practice this law was less enforced in religious schools where female students could often be seen attending class in their headscarf (Çakır *et al* [15]).

Starting in the early 1990s, an Islamist movement experienced increasing political gains in local and national elections alike, becoming the largest party in the 1995 national elections. The following years would be marred by conflict between the politically Islamic movement and a secular establishment dominated by the military and the judiciary (Yavuz [76]). One of the main points of friction centered around public displays of religiosity, especially women wearing the headscarf as well as attendance in religious instruction centers. These religious instruction centers either consisted of religious formal alternatives to post-primary education, or extracurricular Qur'an study centers (Günay [32]). Despite their being under formal state control, secular critics argued that the state had lost control of these institutions to Islamists who were using them as indoctrination centers to influence Turkish youth at a crucial and impressionable stage in their development.

The ongoing conflict reached a climax on February 28th 1997 when, after a longer than usual meeting, the National Security Council (NSC) announced the adoption of eighteen recommendations designed to stem the spread of Islamism in the country. One of these recommendations was the extension of compulsory secular schooling from 5 to 8 years [7], and it made quite clear what was at stake:

> "*With a view toward rendering the tender minds of young generations inclined foremost toward love of the republic, Atatürk, the homeland, and the nation, and toward the ideal and goal of raising the Turkish nation to the level of modern civilization, and to protect them against the influence of various quarters... an eight-year uninterrupted educational system must be implemented across the country.'*

[7] The other recommendations extended across a wide spectrum of political issues from crimes against Atatürk, the use of Islamist rhetoric in public speeches, state monitoring over private media organizations, the influence of Iran in Turkish society, general "anti-secular activities", to the management of animal sacrifice at religious holidays. All of these may themselves affect preferences and behavior overall but we don't see any reason why they should affect those born just after September 1986 differentially than those born just before. The RD design that we detail more in the next section thus aids to insulate the effect of the education reform from other actions aiming to influence religious and empowerment outcomes.

The February 28th meeting initiated a process, precipitating the resignation of the Islamist-led coalition government as well as the subsequent banning of the Islamist party itself a year later, which would afterwards come to be referred to as the post-modern coup of 1997.[8] Less than five months later after the meeting, Law No. 4306 passed the Turkish parliament by a vote of 277 to 242, although due to the interference of the military, the democratic credentials of the reform remained questionable. The new law stipulated an extension of compulsory schooling to 8 years, effectively merging primary school and junior high school into what is now called primary education (*İlköğretim*). The option to attend religious junior high schools was consequently removed and the traditional diploma that had been awarded at the end of the fifth grade was abolished, replacing it with one for successful completion of the eighth grade.[9] A year later, the law went into effect.

Although the reform served both to expand compulsory schooling as well as terminate the religious junior high school option, for women it was the former that dominated as their participation in religious schools as a form of vocational schooling was already minimal (see the summary statistics in Section 3), and as we show in Section 5, the effects of education are robust regardless of whether there was pre-treatment access to religious schools or not.

According to Turkish Law[10], compulsory schooling begins in the September of the year a child turns 6 years old (completes 5 years old). Even though there may be imperfect compliance with this law, the law implies that the official school start age is determined by the child's age in September. On the other hand, law number 4306 that was adopted on 16 August 1997 made eight years of primary education compulsory[11]. This law was effective starting with the 1997/98 school year, which implied that students who had completed the fifth grade in 1997 were exempt from the law while those who had completed the fourth grade were now required to remain in primary education until they completed the eighth grade.

Together, the law pertaining to the school starting age and the 1997 law making eight years of schooling compulsory for some grades implied that children who were born in September 1986 or before (starting schooling in 1992 or before) would have completed fifth grade in 1997 and thus would have been exempt from the 1997 law; whereas children born before September 1986 (starting schooling in 1993 or after) would have completed at most fourth grade in 1997 and would have been required to stay in primary education until at least they complete grade eight. Naturally, there may have been imperfect compliance with the school starting age, or grade repetition etc, but the important point is that official requirements implied that children born before September 1986 were *more likely to be exempt* from the 1997 law compared to younger cohorts [12]. And as we discuss in the next section, it is

[8] See "Post-modern darbe" (*Turkish: Post-modern coup*), Cengiz Çandar, Sabah, June 28th 1997, http://arsiv.sabah.com.tr/1997/06/28/y12.html

[9] Already enrolled students in religious and other vocational junior high schools were allowed to finish their degrees (See the Ministry of National Education Year 2000 Assessment Report, http://www.unesco.org/education/wef/countryreports/turkey/rapport_1.html A further component of the Law raised the minimum grade requirements of attending Qur'an instruction centers but these were subsequently overturned two years later.

[10] Resmi Gazete 15 October 1983, Number 18192

[11] Resmi Gazete 18 August 1997, Number 23084

[12] Cesur and Mocan [16] criticize our approach, claiming that the threshold date we use in the empirical analysis is wrong because the 1997 reform did not explicitly refer to the September 1986 date as the cutoff determining which cohorts were subject to the new compulsory schooling regime. While it is true that this cutoff was not mentioned in the new law, it is the interaction of the 1997 law and the past laws pretaining to the beginning of compulsory schooling period that enable us to use this strategy. If our strategy was truly flawed, we would not be able to identify a jump in schooling around the

this consequence of the new law that facilitates the use of the RD design. It also allows us to isolate the effect of education from other actions stemming from the other seventeen recommendations in the above mentioned military memorandum of February 28th.

The new law required a massive investment in education. According to a World Bank [71] report annual expenditures for the reform were in the order of 3 billion US dollars. This included expenditure on construction of schools, educational materials, and staff. Within just a few years of the implementation of the reform, around 82,000 new classrooms were built (increasing classroom supply by 30 percent) and 70,000 new teachers were recruited. In order to improve access for children in rural areas, a variety of methods were implemented ranging from extending an already existing bussing scheme, establishing more boarding schools, and consolidating some village schools. Students from low-income families often received free textbooks and school meals.

Despite the name, 'Basic Education Law,' the law was primarily meant to enforce enrollment as opposed to reforming aspects of the main education system, such as the curriculum or other rules, resulting in an extension of the existing secular junior high school curriculum as it was before 1997 (Dülger [24]). Since male schooling was already comparatively high, the legal change had a particularly strong effect on including women, and especially so for women in more socially conservative communities. As we show later in section 4, the law effectively reduced the gender gap in junior high school completion by half.

3 Data and Empirical Design

3.1 Data

The data used in this study comes primarily from the 2008 Turkey Demographic Health Survey (TDHS), with the 2003 wave of the TDHS used for some robustness checks. Within these household surveys, the main module is directed toward the sample of women who have married at least once, and, importantly for our identification strategy, this module includes data on month-and-year of birth of the respondents. Since the cutoff in our RD design occurs roughly mid-year, and due to the absence of month-and-year of birth for the larger DHS household sample, we can therefore only expect to identify precise RD estimates for the sample of ever married women.

In our analysis, we are interested in measuring religious expression and empowerment. The data includes variables measuring religious expression in a broad way such as wearing a headscarf, having attended a Qur'an course, as well as prayer and fasting. The ever-married women module of the TDHS also includes a number of variables related to female empowerment. We specifically look at the timing and characteristics of marriage, decision-making power, labor force participation, characteristics of the spouse and ownership of household durables.

Table 1 provides summary statistics on key characteristics of women in the ever married women sample from the 2008 TDHS survey. Since our identification focuses on young women in their early twenties, we report summary statistics for those between 16 and 26 years of age (most of our estimated

threshold. In a recent working paper, Erten and Keskin [25] adopt the same empirical strategy as ours, using a different micro dataset and find very similar results on the effects of the reform on women's schooling. As such, their paper further corroborate the validity of our approach

bandwidths in the RD analysis will use this age range). As seen in Panel A, the average woman in this age bracket had 6.3 years of schooling. Roughly 81 percent had graduate from primary school, 38 percent had graduated from junior high school while 21 percent of them had completed high school.[13] Around 6 percent had attended a vocational school (including technical as well as religious schools) as their most recently attended school type. As it is unlikely that anyone enrolled in a junior high religious school would go on to a secular higher form of education, this is a useful upper bound measure of the degree of female religious schooling. Clearly, women having religious schooling were an extreme minority even among those women with any form of education, and as such, even if the reform did close down junior high religious schools as an option, this is unlikely to have any bearing on female educational choices. Any effects found of education is thus likely be that of secular schooling versus no schooling, as opposed to secular schooling versus religious schooling.

Panel B of Table 1 provides descriptive statistics for the measures of religiosity we have in the data. 77 percent of women aged 16 to 26 years old in the ever married sample reported wearing a headscarf when they leave the house, consistent with other recent surveys from Turkey[14]. 44 percent had attended a Qur'an course and just below 40 percent reported that they said their prayers regularly. The last two measures in Panel B show an overwhelming majority of women in our sample reporting that they prayed occasionally and fasted regularly. Since all of these variables measure religiosity in different ways, we also construct a weighted religiosity index, where the weights are proportional to one minus the mean of the religiosity variables. Thus, the religiosity index puts more weight on less common measures of religious expression, such as Qur'an study and regular prayer, and less weight on praying occasionally and fasting, which are more common[15]. The mean level of the religiosity index is 0.43 and its standard deviation is 0.24.

Panel C of Table 1 provides descriptive statistics for several empowerment-related outcomes. The average age at first marriage among the ever-married women is 18.8 years.[16] The next variable relates to the decision-making power of the respondent, specifically about selecting her marital partner. In Turkey, arranged marriages are quite common, and these arrangements vary from having individuals' parents negotiate and decide on the match to introducing the two parties of the potential match and letting them decide on the final outcome. The TDHS survey contains a question "Who decided on your marriage with your husband? You and your partner, or your families?" and the options are "our families", "ourselves" or "I eloped to my husband". Just over half of the respondents (55%) reported that they decided on their marriage themselves as opposed to their family making that decision[17]. The

[13] Even though the DHS survey includes its own categories from completion of lower secondary and primary school, these are likely subject to some measurement error. The pre-reform name in English for the five year compulsory education was "primary school", whereas the English name for the post-reform eight-year compulsory schooling was "primary education". In the DHS it is not clear whether completing the category "primary school" refers to the former or the latter, and similarly whether completing secondary school refers to junior high school, i.e primary education, or high school. For this purpose, we use as measures of completion for primary and junior high school dummy variables for whether the respondent had at least 5 and 8 years of schooling respectively.

[14] See for example Çarkoğlu and Toprak [14].

[15] More formally, the weights are $w_i = (1-\mu_i)/\sum_{j\in J}(1-\mu_j)$ where μ_i is the mean of religiosity variable i in the set of religiosity variables J. All religiosity variables used in creating the index are indicator variables taking either a value of 0 or 1.

[16] The relatively low age of marriage is not specific to the young cohorts in the ever married sample. For ever married women of *all* ages, the corresponding average is 19.9. It is however somewhat younger than nation-wide estimates which is around 22.9 (See United Nations Economic Commission for Europe, http://goo.gl/fLQ49A).

[17] Among those who reported that they decided on their marriage themselves, 11% reported that they eloped with their

following row of Panel C is related to the institution of brideprice (*başlık parası*) which, despite losing much of its prominence in modern-day Turkish society, is still in use in rural parts of the country[18]. On average, around 19 percent of the women in the sample reported that their family received a brideprice upon their wedding.

An important mechanism through which education may improve women's rights is the labor market (see for example Goldin [30], Doepke et al [22]) and the 2008 TDHS contains a number of measures on women's labor market outcomes, of which we will primarily use the share of women employed in non-agricultural sectors and the share of self-employed women.[19] The labor force participation in non-agricultural sectors among married women aged 16-26 in our sample was 10 percent, a characteristically low level consistent with Turkey being a country where very few women are active in the labor force outside of agriculture[20]. Our interest in the share of respondents being self-employed comes as it represents a kind of labor force participation that tends to be informal and relatively more acceptable in socially conservative environments (White [74]).[21]. A mere 3 percent of female respondents in the sample reported being self-employed. We also include as an outcome the share of women who are so-called "unpaid family workers", which often means they work through their male relatives as intermediaries. This form of labor is also considered more socially acceptable for women, albeit one where women remain highly vulnerable with little or no control over their earnings (White [74]).

The last two rows of Panel C report summary statistics for outcomes related to the respondent's spouse and her household. On average, respondents' husbands had 1.7 more years of schooling and were just over 5 years older than their wives. The last row in panel C shows an index based on the first principal component across the ownership of 20 different assets, which cover all the relevant household assets included in the TDHS module on household wealth. Although the DHS interprets the ownership of these assets as "household wealth" (TDHS [69]), we follow Young [77] in interpreting the asset ownership variables in the DHS as "consumption". In particular, we generate an asset ownership index calculated as the first principal component of 20 indicator variables on whether the respondent's household owns the following assets/services: fridge, gas/electric oven, microwave oven, blender/mixer, dishwasher, washing machine, iron, vacuum cleaner, air-conditioner, cellphone, computer/laptop, internet, plasma-TV (LCD), cable-TV, satellite antenna, DVD-player, camera, car, taxi/mini-bus, tractor.[22]

husband and the rest decided jointly with their husband, without having to elope. Fore brevity, we code both responses in the same way as "having decided on their partner themselves". The analysis of the three types of responses separately do not change the qualitative findings and the results are available from the authors upon request.

[18] Using data from previous waves of the TDHS, Anderson [3] shows that the proportion of women who reported that their family received a brideprice upon their marriage declined from 46 to 23 percent in rural parts of the country between 1960-75 to 1985-98, while the corresponding rates in urban Turkey went from 34 to 12 percent. In the 2008 TDHS that we use, among the entire sample of married women the rates are 29 percent in rural and 14 percent in urban areas.

[19] Unfortunately, the survey did not include any measure of individuals' labor earnings.

[20] Overall labor force participation (including both agricultural and non-agricultural employment) is only 19 percent for the sampled women.

[21] Studies on labor force participation and occupational choices of women in Turkey have documented the challenges women face in finding wage-jobs in the formal sector, except for highly-skilled women with university or higher degrees (World Bank [72], Özcan *et al* [62])

[22] We exclude three assets that are included in the survey but for which we have very few observations in the sample (less than 1%) who report owning them, and therefore we believe them to be irrelevant for the setting of our study. The excluded assets are: garbage grinder, fitness equipment and washer/dryer. Furthermore, we combine some assets that were asked separately in the survey but we believe to contain similar information. In particular, we combine laptop and computer, video-recorder and camera to have one dummy variable that indicates whether a household owns a laptop or a computer, and another variable for ownership of video-recorder or camera. Ignoring these modifications and constructing

For the religiosity variables, we report results for the full set of variables available in the TDHS. For the other empowerment-related outcomes we report what we consider a focused set of variables and report results for a wider set of outcomes in Appendix B.[23]

Finally, Panel D of Table 1 provides summary statistics on predetermined characteristics of married women in our sample. On average, nearly a third of the young women in the ever-married sample had a mother whose primary language was not Turkish (and in 88 percent of these cases, the mother's primary language was Kurdish). With regards to parental education, three quarters of fathers and forty percent of mothers had completed primary school. For completion of junior high school, the corresponding percentages are 19 and 5. Finally, 26 percent of the respondents had consanguineous parents and 28 percent were from rural areas.

3.2 Identification

As described in Section 2.1, an important consequence of the reform in compulsory schooling law in 1997 (along with the law pertaining to the school starting age) was that individuals born after September 1986 were induced to stay longer in school compared to older cohorts. For older cohorts, any further schooling beyond 5 years remained optional. This allows the use of a Regression Discontinuity (RD) design to estimate the causal effect of schooling on various outcomes. Our empirical design thus relies on a comparison of cohorts born just after or before the discontinuity, in this case September 1986. As long as the treatment and control groups in close proximity to the discontinuity do not differ systematically in any other way than their years of schooling, this allows an as-good-as-random assignment of years of schooling (Imbens and Lemieux [41].

Previous research (McCrary and Royer [57], Oreopolous [59]) using RD designs have often used the birth-date-related discontinuity as an instrument for schooling. As will be clear in Section 4, in our design the first-stage estimates a discontinuous jump of almost exactly one year of schooling, providing meaningful reduced-form results and making the full implementation two-stage least squares somewhat redundant, although we report both, and continue to rely on the exclusion restriction that the reduced-form RD estimate affects the outcome solely through more years of schooling. Throughout the text, we use a basic RD specification of the form:

$$\begin{aligned} y_i &= \alpha + \beta t_i + f(x_i, t_i) + \varepsilon_i \\ \forall x_i &\in (c-h, c+h) \end{aligned} \tag{1}$$

where y_i is the outcome in question, t_i is the treatment, x_i is the forcing variable, in this case months after September 1986, and h is a neighborhood around c, hereby referred to as the bandwidth. The control function $f(x_i, t_i)$ is some continuous function, usually an n-order polynomial in the forcing variable on each side of c. Previous research has used different approaches to RD estimation, but are predominantly variations of equation 1 by choosing different bandwidths and control functions. We use local linear regressions (Hahn *et al.* [34], Imbens and Lemieux [41]). In order to determine the correct bandwidth

the asset using all the assets included in the TDHS separately does not change our findings significantly.

[23] These include age of first birth, number of children, having a say over the use of contraception, employment by sector and type, house ownership, husband's education level completed, as well as husband's labor force participation.

we use the optimal bandwidth routine from Imbens and Kalyanaraman [40]. This allows specifying an optimal bandwidth for each outcome under consideration. Following Card and Lee [13] we cluster the standard errors at the month-year of birth level in order to accommodate for specification error in the forcing variable. In all regressions we control for dummy variables for the level of schooling completed by respondent's mother and father respectively, a dummy variable for whether the respondent's mother's first language was different from Turkish, a dummy variable for being from a rural location, a dummy variable for whether the respondent's mother and father are relatives, month-of-birth fixed effects, as well as region fixed effects.

3.3 Preliminary Checks

In this section we investigate the validity of the RD design and the assumptions underlying our interpretation of the RD estimates as the impact of the education reform on women in Turkey. None of these tests produce any evidence that threatens the validity of the RD design. We also discuss a number of tests related to this specific design and the use of a sample of ever married women in evaluating the consequences of education induced by the reform.

A key assumption in order for the RD estimates to have a causal interpretation is for the assignment of years of schooling around the threshold to be as good as random. A consequence of this assumption is that we should observe no commensurate jumps at the threshold for any variables that were predetermined at the time of the treatment. Figure 1 illustrates this by plotting monthly binned averages of pre-determined characteristics against the forcing variable. In these graphs, we show parents' completion of primary and junior high school, whether respondent's mother spoke a non-Turkish language as a mother tongue or were in a consanguineous marriage, a dummy variable for being from a rural location. In addition, we also report two covariates on whether the respondent migrated from one location to another during her childhood, and whether she was born in any of the southeast provinces of Turkey.[24]

The graphs A through D of Figure 1 demonstrate two main points. First, one can easily see the severe disparity in educational attainment between respondents' mothers' and fathers' educational attainment – approximately 40 percent of the respondents reported that their mother had completed primary school or above, while the corresponding figure was 75 percent for their fathers. For the women in our sample, attaining an education level higher than primary school thus represents a significant change from their mother's generation. Second, the relationship between respondents' parents' education and their birth cohort appears smooth across the threshold.[25] The rest of the graphs in Figure 1 show that all of the other pre-determined covariates appear similarly balanced around the threshold.

We also test for evidence of jumps using more parsimonious, regression-based tests of all variables that we use as controls in the main analysis in Section 4, following Lee [47]. Of a total of 22 control variables – including dummies for parental education types, rural location, whether their mother had a non-Turkish language as mother tongue, whether the respondent's parents were consanguineous – none

[24]The latter two outcomes are included to show that there is no evidence of selection into treatment related to events surrounding the conflict between the Kurdistan's Worker's Party (PKK) and the Turkish state that was still ongoing in the late 1990s, and had large-scale migration away from affected areas in the southeastern region of the country.

[25]In the graph for father's completed junior high school completion, the endpoint to the right is slightly higher than the endpoint to the left. The jump, however, is not statistically significant in this graph nor in the regression analysis in Table 11 in Appendix B

(except one, and then only at the 10 percent level) of these are individually statistically significant at conventional levels and, using seemingly unrelated regressions (SUR), we can reject joint significance of treatment on the control variables with a p-value of 0.75 (see Table 11 in Appendix B).

A second source of concern might be manipulation of the respondent's age by her parents. In the 1980s, it was not uncommon for Turkish parents to delay registering their children until later, sometimes as much as three years later. If they could also change the registered age of the respondent around the time of the education reform, this would imply that our assumption of local randomness may be violated. We do not, however, expect parents to be able to affect the birth dates of their 11-12 year old children after the announcement of the law change and any seasonality in the birth-month cohorts should be picked up by our month-of-birth fixed effects, although their inclusion has no bearing on the estimates. Furthermore, we run a McCrary [56] density test on the density of the forcing variable yielding an insignificant estimate, as can be seen in Figure 5.

As we will be using the ever married women's module, another concern is to what extent there is a sample selection bias – whether treatment had any effect on current or past marital status as of 2008 and thus on the inclusion into the main sample of investigation. In order to test for the possibility of a selection effect into the ever married women's sample, we use the household module of the TDHS which contains observations from *all* household members living in the sampled households, regardless of their marital status at the time of the survey. This module contains information on the highest education attained and the year of birth of the individual household members. Not knowing the month of birth, we can at best expect to estimate imprecise and somewhat noisy effects for this larger group including both married and single women. Our more noisy cutoff is in this case between 21 and 22 years of age, recorded at the time of the interview between October and December of 2008. As can be seen in the lower right graph in Figure 2, there is no evidence of a significant jump in marital status at the discontinuity, and there is therefore little reason to expect that the treatment affected likelihood of inclusion into the sample restricted to married women we will focus on for the rest of the paper.[26] As such, our approach identifies an education effect unconfounded by selection into marriage. [27]

The upper two graphs of Figure 2 further illustrate the degree to which the reform affected women and men differentially. The upper left graph in Figure 2 shows completion of junior high school for *all* women in the sampled households, in annual age bins at interview date. Here, average completion of junior high school is clearly higher (to the order of one and a half year) just to the right of the cutoff compared to that just to the left, illustrated also by the difference in endpoints of local linear smoothers on each side of the discontinuity. This confirms the expected increase in educational attainment for all women regardless of marital status. The upper right graph in Figure 2 plots the same relationship for men in the sampled households and reveals no clear jump at the discontinuity. Not only is the difference at the threshold much smaller than for women, but it is also negative and within the corresponding confidence intervals. Consequently, the reform seems to have had a much more muted effect for men, which is perhaps not so surprising given that almost 90 percent of the men in the sample were already completing junior high school even before the reform.

[26] We also report regression-based tests in Table 8 in Appendix B.

[27] See McCrary and Royer [**?**] for a discussion along similar lines for selection into motherhood. Also, even in the existence of a selection effect, the approach of Lee [48] could be used to bound the treatment effect.

In order to demonstrate the effect that the reform had on the gender gap in schooling, in the lower left panel of Figure 2, we plot the ratios for the average female-to-male completion rates in junior high school against the annual birth cohort. To the left-hand-side of the threshold, the ratio of female-to-male junior high school completion rate fluctuates around 0.6, to the right-hand-side of the threshold the ratio is around 0.8. The jump at the threshold is around 0.16, a relative increase of roughly a third compared to the pre-reform ratio. To put the magnitude in broader context, the ratio for individuals in their forties is 0.45, compared to 0.6 for the 22-year-olds, the gender gap closed by 0.15 points over a generation. The size of the jump at the discontinuity in Figure 2 implies that the reform accomplished this magnitude in just a few years.

One consequence of our focusing on ever-married women is that the estimated RD treatment effects are likely to be less informative for unmarried women at the time of the survey. If unmarried women are of significant interest in evaluating the 1997 reform, then our results would be of limited value. These two groups differ in marked ways and in order to illustrate these differences we tabulate the means by marriage status for a number of variables related to parental backgrounds in Table 9 in Appendix B among women in the age group 16-25. The household sample does not include many variables on parental background, so in order to build the corresponding variables for unmarried women, we used the household roster to identify their parents, which was possible for women whose parents were still alive and who live in the same household as them. This way, we were able to identify parental education for a majority of unmarried women aged 16 to 25 in the sample, as most of them still lived with their mother (86 %), father (79 %) or both (77 %) at the time of the survey.

The findings in Table 9 clearly show the sample of ever-married women as coming from poorer, less-educated, and likely more socially conservative backgrounds. Estimating RD treatment effects for this sample will thus yield estimates more representative of the poor and pious. The question then is, by leaving out unmarried women, i.e. those from richer and likely more socially progressive backgrounds, what implications does this have for our design?

Our design rests on inference, at the discontinuity, from compliers with the reform among the ever-married women sample. If there are plenty of compliers among the unmarried women, our results on outcomes such as religiosity and empowerment will not capture effects for this subgroup. In order to see whether the reform also affected education among the unmarried sample, we estimate RD treatment effects on years of schooling and the completion of junior high school in Table 10 in Appendix B separately for ever-married and unmarried women. Table 10 shows that, in contrast to the positive effects on education for ever-married sample, similar impacts for the unmarried women are non-existent in terms of years of schooling, and much smaller in terms of completion of junior high school. Hence, although our main analysis will exclude the unmarried women sample in later sections, doing so is unlikely to have a significant bearing on the evaluation of the reform itself. As discussed in Section 3 the reform was specifically targeted to a subpopulation that would have, in the absence of the reform, not completed junior high school, which to an overwhelming degree meant women from poor and pious backgrounds. The absence of a significant first-stage relationship among a group of women who would likely finish junior high school, regardless of the reform, is therefore unsurprising. Focusing on the ever-married women thus allows the study of a group of women closer to the policy-relevant subpopulation.

Finally, to verify that the discontinuity around September 1986 is at least somewhat unique, and

following Meyersson [58], we run RD regressions with years of schooling as the outcome, allowing placebo discontinuities at every month within one and half years away from the true discontinuity, and plot the resulting absolute t-statistics in Figure 7 of Appendix Appendix B; for one specification using the IK bandwidth and one specification using a constant bandwidth of 30 months. In both specifications, the plotted t-statistics clearly show the month and year of September 1986 as an "extreme" discontinuity in years of schooling compared to birth dates farther from this point.

4 Results

4.1 Schooling

> *"The government's most creative and significant duty is education." – Turkish proverb widely attributed to Mustafa Kemal Atatürk*

The first set of outcomes on which we test for the impact of the compulsory schooling reform are those related to education itself. The upper two graphs in Figure 3 provide a graphical illustration of the RD design: the upper left graph plots (using observations from the 2008 TDHS) the average years of schooling in monthly bins against month-and-year of birth where the cutoff is September 1986.[28] The reform implied (as described in Section 2.1) that those born before this date were more likely to be exempt from the new law and, as a result, more likely to be required to attend a minimum of *five* years of schooling. On the other hand, those born after September 1986 were more likely to be required to attend a minimum of *eight* years of schooling. Overlaid in the graph is a local linear smoother on each side of the discontinuity, and these demonstrate a noticeable jump at the threshold of approximately one additional year of schooling. In order to further illustrate the RD design, we also use data from the 2003 DHS survey (ever-married women module) to conduct a placebo check. The upper right graph in Figure 3 shows the corresponding relationship for observations from the 2003 DHS survey. The age threshold is defined to be at the exact same age as before, comparing women 21 and 22 years of age – or equivalently, being born before or after September 1981. As the upper right graph of Figure 3 shows, we observe no jump in the same-aged women in the earlier 2003 survey. This implies that the jump we observe for the 2008 observations is most likely due to the reform and not some underlying relationship between age and women's schooling in Turkey[29].

In order to establish these relationships more thoroughly, we estimate specification 1 on the sample of ever-married women from the 2008 Turkey DHS. The regression estimates for the impact of the policy change on years of schooling as well as the type of schooling completed by women are reported in Table 2. Column 1 of Table 2 estimates the RD treatment effect on years of schooling, using the Imbens and Kalyanaraman [40] method to calculate the optimal bandwidth $\hat{h}$ of 69 months around the threshold.

[28] Graphs using alternative bin sizes are provided in Figure 6 in Appendix B.

[29] The fact that the September cutoff comes from laws related to school-starting age may raise an additional concern that initial maturity differences may influence schooling as well as other, non-cognitive outcomes. Bedard and Dhuey [10] demonstrate the existence of such *age-for-grade* effects in various OECD countries (excluding Turkey). If initial maturity differences coming from school starting age were the main mechanism driving our findings, we would expect to find similar effects for the 2003 sample where we use the same September 1st cutoff. Instead, our placebo tests using the 2003 sample (and the same September cutoff) demonstrate that the effects we identify in the 2008 sample are driven mainly by the reform in compulsory schooling laws, and not by initial maturity differences related to school-starting age.

This yields an estimated impact of one year of schooling, which is significant at the 1 percent level. This corresponds to roughly 16 percent increase relative to the mean level of schooling (reported in the first row of Table 2). We test the robustness of our estimates following Imbens and Lemieux [41] by using half, a third, and twice the optimal bandwidth respectively in Panels B, C, and D. We also allow the control function to have quadratic and cubic orders in columns 2 and 3. Only in column 1 of Panel C. with the largest bandwidth and least restrictive control function, does the estimate vary meaningfully vary away from an estimate of one but is still statistically significant. Otherwise, the estimates hover closely around that from the main specification in column 1. To sum up, the results imply that mostly irrespective of the control function and the bandwidth used, the reform led to an increase of approximately one year of schooling. This finding has the consequence that in using the reform to estimate the impact of an additional year of schooling on other outcomes, the reduced-form (i.e. *sharp* RD) and the IV estimates (i.e. *fuzzy* RD) will be largely the same. Therefore, although we report IV estimates throughout this paper, we will focus on the reduced-form regressions using the local linear method.

Columns 4-6 of Table 2 report estimates for the impact of the reform on various types of educational attainment. In column 4 of Panel A, the dependent variable is a dummy variable equal to one if the respondent completed more than 5 years of schooling, corresponding to primary school or above. The mean completion rate of 80 percent is high, but unsurprising as it was compulsory even before the education reform. Using the linear control function, the RD estimate for the effect of the reform is insignificant both statistically and economically, at 2.6 ppts (or 3 percent relative to the mean), and in other specifications the estimate remains small. In contrast, for completion of junior high school in column 5 of Panel A, the education type most affected by the reform, we show a large positive and statistically significant effect of 22 ppt (55 percent relative to the mean). The estimate is somewhat smaller in the other specifications but remain roughly of the same magnitude and precisely estimated.

Column 6 shows that the reform had a positive, significant and robust impact on the probability that women in Turkey graduated from high school (which corresponds to 11 grades of completed schooling). The local linear estimates in panel A show a statistically significant impact of 8 ppt (or 35 percent relative to the mean) and estimate in the other specifications are of similar magnitude and precision. This implies that the reform's average impact was not just limited to increasing women's schooling up to the legal minimum of 8 years in school, but it caused some women to stay longer in school to complete high school or above.

The last outcome in the table, vocational schooling, is important in order to understand the mechanisms through which the schooling reform may have influenced women's outcomes. As described in Section 2 above, the reform not only extended the minimum number of years of schooling but also removed the option of choosing a vocational junior high school. Thus any treatment effect could manifest itself either through more years of schooling or through affecting the type of schooling. In the dataset we have no information on the years of different types of vocational schooling respondents attained but we know if the last school they attended was a vocational school (which includes religious (junior high or high) schools). The dependent variable in Column 8 of Table 2 is a dummy variable equal to one if the last school that the respondent attended was a vocational school. As students of a vocational junior high school are unlikely to move on to a general high school, we can to a large extent infer that this is a

measure for the completion of either vocational junior high school as the highest education completed or subsequent vocational school completion (high school or above). A large negative impact on attending a vocational school would be consistent with a treatment effect being driven by a switch from religious (or any other form of vocational schooling) to secular schooling. Yet the results in column 7 show that the reform had an insignificant effect, both statistically and economically, on the probability of having last attended a vocational (including religious) school. And in most specifications, the estimate is positive, not negative. In other words, even though the reform removed the possibility of attending a vocational *junior high school*, this didn't lead to a real reduction in attending vocational school overall. As women to such a small extent participated in vocational schooling of any form (the mean is just 6 percent), this is not surprising.[30]

4.2 Education and Secularism

> *"In human life, you will find players of religion until the knowledge and proficiency in religion will be cleansed from all superstitions, and will be purified and perfected by the enlightenment of real science." – Mustafa Kemal Atatürk*[31]

In this section, we test if the additional year of schooling caused by the reform affected outcomes related to religiosity of women in Turkey. As a first step, in Figure 3 we plot the relationship between the propensity to wear a headscarf and the forcing variable in monthly bins with a local linear smoother on each side of the threshold.[32] In the lower left graph we do this for the sample of individuals born before or after September 1986 from the 2008 survey, and in the lower right graph we plot the same relationship for individuals born before or after September 1981 from the 2003 survey. Whereas the left-hand-side graph illustrates the treatment, the figure on the right represents a placebo test of same-aged individuals from the earlier survey wave. We observe a negative jump in wearing the headscarf at the discontinuity for the 2008 survey, but no meaningful jump for individuals of the same age in the 2003 survey.

The 2008 survey contained questions on religiosity including headscarf use, having attended a Qur'an study course, fasting during Ramadan and praying. For the purpose of illustration we construct a mean-weighted index of these variables combined in the upper right graph in Figure 4. This index shows evidence of a negative jump at the discontinuity, similar to the graph for wearing a headscarf in Figure 3.

Table 3 provides the results of estimating specification 1 on multiple measures of religious expression and practices we have in the data. As a point of reference, in Panel A, we report basic ordinary least squares (OLS) regressions of our religiosity measures on years of schooling within the optimal bandwidth $\hat{h}$, allowing the bandwidth to vary by outcome. For all the religiosity-related variables except for having attended a Qur'an course, we observe a negative correlation with years of schooling. For example, for the religiosity index in column 1, an additional year of schooling is associated with a 1.1 ppt lower value

[30] This doesn't mean the closure of religious schools could not have had an effect for men, but as the relevant empowerment outcomes in the DHS only exist for the ever-married women sample, the potential effects for men are outside the scope of this paper.

[31] Speech (October 1927); quoted in Atatürk'ten Dusunceler by E. Z. Karal, p .59

[32] The use of the headscarf variable at this point is because it is the only religiosity-related variable for which we have data in the 2003 survey.

of the religiosity index and in column 2 a 3.5 ppt lower likelihood to wear a headscarf. The rest of the columns include outcomes for whether the respondent has ever attended an extracurricular Qur'an course, whether the respondent prays five times a day, whether the respondent ever prays, and whether the respondent fasts. In all but the Qu'ran course outcome (where the point estimate for years of schooling is 0.001 and insignificant), schooling is negatively correlated with these measures of religious expression.

Panel B of Table 3 presents the RD estimates using the local linear control function with optimal IK bandwidths for each specific outcome. The results imply that the reform led to a 5.8 ppt lower religiosity index (13 percent relative to the sample mean), a 7.6 ppt (10 percent) lower likelihood of wearing a headscarf, a 9.6 ppt (22 percent) decrease in having attended a Qur'an course, and a 7.3 ppt (19 percent) lower incidence of regular prayer. For the religiosity index, Qur'an course, headscarf, and regular prayer outcomes, the negative RD estimates in Panel B are larger in magnitude than the OLS estimates in Panel A, with all but the last being statistically significant. These estimates are robust to variations in specification as demonstrated in panels C through E where we include a quadratic control function in the former and combine a linear control function with half and twice the optimal bandwidth in the two latter panels respectively. Estimates using two-stage-least-squares in Panel E are nearly identical to the estimates in Panel B, an expected outcome given our first-stage estimates of around one. In Panel F, we use completion of junior high school (a minimum of 8 years of schooling) as the endogenous variable Taken together, our results imply that the increase in compulsory schooling led to a significant decrease along multiple measures of religiosity.

In explaining why we find results for the first three but less so for the last two outcomes related to religiosity, it is important to remember the context in which the education law was applied. Many in the secular establishment were particularly concerned over *public* displays of religiosity as well as alternative instructional facilities. Given Turkey's long-running controversial debate over the headscarf as a symbol of Islamism, and the explicit mentioning of Qur'an study centers as potentially threatening to country's secular values, it is perhaps not so surprising that estimates are particularly pronounced for these outcomes. In contrast, occasional prayer and fasting could be deemed as measuring religiosity at a less controversial, or less public, level which could explain the smaller effects on these outcomes.

Moreover, as discussed in Section 2, to the extent that the law imposed girls to stay in secular education for longer, it also imposed them to not wear a headscarf regularly until an older age and this could have led to a change in their preferences and affected their headscarf use in later life. This in turn could mean that schooling may not necessarily affect deeper religious preferences per se, but rather public expressions of them. If this is simply a matter of indoctrination towards restricting public expression of religiosity, this could necessitate an interpretation away from a secularization-as-enlightenment story. The negative effect on having attended a Qur'an study course is also in line with this view, as the spread of extracurricular religious courses was one of the main sticking points for the military at the time of the education reform.

Furthermore, these results do not necessarily imply that women independently *chose* their level of religious expression. One possibility is that schooling may have changed the type of husband the women eventually married, and that the differences in religious expressions simply represent different commandments of different husband types; for example, a religious husband for a woman with less

schooling versus a secular husband for a woman with more schooling. In order to investigate whether the effects on public expressions of religiosity came from women making these decisions themselves, and whether this may come through some sort of empowerment, in the next sections we examine the effects on empowerment-related outcomes.

4.3 Education and Empowerment

In this section we investigate the reform's effects on women's empowerment. Our findings are reported in Table 4 where we focus on eight key outcomes related to women's empowerment. Results for a broader set of outcomes are available in Appendix B.

We start by testing the effects of schooling on age at first marriage. In our sample, women marry young, with mean age at first marriage at 18.75 years (standard deviation of 2.8). This implies that a fair share of women got married around the minimum age of marriage which in Turkey is 16, and some even younger than that. Panel A of Table 4 shows that schooling is positively correlated with age at first marriage: an additional year of schooling is associated with getting married 2.5 months later. On the other hand, the RD estimates reported in Panel B are insignificant and the point estimates have the opposite signs compared to the OLS correlations reported in Panel A. In some of the alternative specifications, the magnitude of the negative estimate is somewhat larger and in Panel F it is marginally significant at the 10 percent level.

Of significant interest is to test whether education improved decision rights of women in Turkey. The ever-married women module in the 2008 DHS included a question on whether women had a say in the choice of their husband. An illustrating measure of the state of women's rights in Turkey is that only half (54 percent) of the women in our sample reported that they chose their husbands themselves, as opposed to having a match arranged by the families (as reported in the first row of Table 4). It is therefore noteworthy that the RD treatment effect in Panel B shows a precisely estimated 11 ppt (21 percent relative to the sample mean) increase in the proportion of women who decided on their husband themselves (as opposed to their families). This is a much larger estimate than the basic OLS correlations reported in Panel A, and the estimate is robust to alternative specifications as can be seen in Panels C through F.[33]

Next, we report effects of education on the incidence of whether a brideprice was paid by the respondent's husband's family to her parents. Column 3 of Table 4 shows that brideprice, although a declining phenomenon in Turkey, still occurred for 20 percent of the cases in our sample, and as can be expected, schooling is negatively correlated with its occurrence (as demonstrated in Panel A). In the RD framework, the estimates are negative and larger, reducing the likelihood that brideprice was received by the respondent's family (for her marriage) by a third to a half in relative terms. As such, although we do not find clear effects on timing of marriage, we do observe concrete and sizeable effects on the manner in which marriages took place. The evidence implies that as women became more educated due to the reform, the manner in which their marriages took place became more modern and more socially progressive.

In Turkey, female labor force participation is very low by international standards, especially when

[33]The only other question about decision-making rights in TDHS 2008 was one related to the type of contraceptive method(s) used by the couple. We find similar positive effects on this outcome (reported in Table 5

excluding the agricultural sectors. In our sample only 14 percent of the respondents reported being currently employed in any non-agricultural sector, and as can be seen in column 4 of Panel A, a year of schooling is associated with a 2.3 ppt increase in the employment rate in non-agricultural sector(s). The RD results reveal positive estimates on probability of employment in the non-agricultural sector(s) but this effect is not robust across the different specifications (the point estimate is negative and imprecise for the $2\hat{h}$ bandwidth specification in Panel E). Moreover, the magnitude of the estimates are highly sensitive to the specification. Overall, we cannot say that the increase in education caused by the reform led to a clear increase in women's participation in the non-agricultural labor market.[34] In the following two columns, we test for the effects of education on type of occupation that women in the sample were employed in. In particular, we test for effects on the probability of being self-employed or being an unpaid family worker. The results for self-employment are positive regardless of the specification, and magnitudes appear less sensitive to the specification, but are again not always precisely estimated. Estimates on unpaid family worker shows negative estimates larger than those from the OLS regressions, but none are statistically significant. What is clear, however, is that any increase in employment appears to be largely driven by an increase in self-employment among women in the sample.

Next, we test whether the increase in respondents' education enabled them to marry more educated husbands through assortative matching in the marriage market (Becker [9]). As we discussed earlier in section 4, we find no evidence that the reform had a direct effect on men's schooling.[35] Column 7 of Table 4 reports our findings on the effects of the respondent's education on her spouse's schooling. Although OLS estimates in Panel A reveal higher-educated women marrying higher-educated husbands, the RD estimates in subsequent panels exhibit smaller and statistically insignificant estimates (and a negative estimate in Panel E). Thus, we do not find that the reform, on average, enabled women from treated cohorts to marry more educated husbands.

Lastly, in the final column of Table 4 we report effects on the asset ownership index, described in section 3. As can be seen in the OLS estimates of Panel A, schooling is positively correlated with the index, with one extra year of schooling corresponding roughly to 1.4 higher points of the index, a small relative effect of roughly 3 percent compared to the mean. The RD estimate in Panel B reveals a treatment effect of 2.5 ppt, corresponding to a 5 percent increase relative to the sample mean. A more detailed examination of the breakdown of the index into its specific components [36] shows that this positive impact is predominantly driven by an increase in the ownership of household assets related to domestic tasks and chores, such as washing machines, vacuum cleaners, and dish washers.[37] Overall, these findings imply that, ten years after the compulsory schooling laws were changed in Turkey, women who attained a higher level of education due to the reform are more likely to live in households owning assets that make it easier to carry out domestic tasks – a task typically carried out by the women in

[34]Similarly, we find a positive but imprecisely estimated effect on overall labor force participation, as reported in Table 12 in Appendix B.

[35]Moreover, as we show in Table 13 of Appendix B the average husband in our sample is aged 28 (or equivalently an interspousal gap of roughly five years) and we find no meaningful RD effects of female education on husbands' age, even though the OLS estimate shows a negative correlation.

[36]Tables 14 and 15 in Appendix B

[37]In particular, women in cohorts affected by the reform are 6 ppt more likely to own washing machines, 5 ppt more likely to own dishwashers, and 8 ppt more likely to own vacuum cleaners relative to women who were born before the age cutoff.

Turkish households.

Our result on age of marriage contrasts somewhat with those of Dayıoğlu, Kırdar, and Koç. [18], Dincer et al [20], and Gunes [33] showing delaying effects on marriage end reduced fertility. To pursue this further, we expand the set of outcomes in Table 5 to include age at first birth, birth before 18, the number of pregnancies, living children, incidences of low birthweight and child death respectively, as well as whether the respondent had a say in the use of contraceptives. For all except the last of these outcomes, the RD results in this table confirm the absence of clear reducing effects on early fertility, and estimates for infant health tend to be small and of varying sign depending on the specification. More striking, however, is that for pregnancies and number of children alive, the estimate is positive, and in the case of pregnancies in Panel C, of both relevant magnitude and precision.

As is clear from the results in this table, the negative sign of the RD estimate on age at first marriage previously estimated is consistent with broader absence of early fertility-reducing effects of education. For example, our RD estimates are of opposite signs as the instrumental variable estimates of Dincer et al [20] on number of pregnancies, and as for most other fertility-related outcomes our estimates tend to be small, insignificant and of varying sign. Furthermore, we do not find any evidence suggesting that education induced by the reform reduced child mortality, despite the apparent negative correlation between education and child mortality. However, we do find a clear positive effect on whether the respondent had a say in the use of contraceptives, consistent with our earlier result on having a say in spousal choice that education affects relevant dimensions of female decision rights.

A common feature of related research on the fertility effects of the 1997 reform is the comparison of wider year-of-birth cohorts eligible for attending junior high school before and after the reform came into place. Our own results suggest that such a comparison risks confounding the effect of the reform – and in extension, the effect of education – with factors potentially affecting both school attendance and the outcome in question. To see this, note for example that the OLS estimates for the number of pregnancies in the optimal-bandwidth sample in Panel A (which includes individuals born 4 years before and after September 1986) have the expected sign, in the sense that more years of schooling is associated with lower levels of fertility. Estimating the reduced form linear RD estimate with the widest bandwidth in Panel D, the estimate is smaller and of the same sign but as we reduce the bandwidth closer to the discontinuity (Panels B and C) the estimates switch sign. This illustrates how comparisons further away from the threshold tend to bias the estimates toward the OLS estimates.[38] We will return

[38] One reason that makes this bias particularly acute in this setting is that many fertility-related outcomes such as number of children, pregnancies etc, rise very steeply in age for the larger subpopulation of interest, namely women in their early twenties. Comparing younger cohorts as treated units with older cohorts means comparing units who are likely to make different fertility decisions related to their age. The strength of RD in this setting lies in removing this source of bias by comparing units of roughly the same age. In principle, and potential bias aside, one explanation for the divergence between our RD estimates and those using instrumental variables with larger cohort groups could be differences in local treatment effects resulting from variation in the subpopulation studied. In our case our subpopulation is the individuals close to being born on September 1986 who would be of roughly the same age when surveyed in the 2008. Other research relying on comparing young cohorts constituted of several years of birth cohorts with older cohorts are intrinsically comparing groups with a lower propensity to have children with those with a higher propensity to have children because of age. As our RD design passes the standard validity tests, this means our subpopulation close to the discontinuity would – compared to the individuals farther from *both* sides of the discontinuity – need to exhibit some characteristic making them less likely to have education affect their fertility less than any of the other outcomes we do find effects on. As we include month-of-birth dummy variables, and observe OLS estimates of similar signs as other studies we find the possibility of differences in the localness of treatment effects an unlikely explanation for the divergence.

to possible explanations for the signs of our estimates in the next section.

5 Discussion

The findings described in the previous section reveal broad empowerment effects of female education in Turkey across multiple outcomes. Our interpretation is that this is the result of expanding compulsory schooling. And as we showed in Table 4, there are no negative effects on having attended vocational schooling, a natural consequence as very few women report having attended vocational schooling in the first place. Another way to show evidence against a "switching" effect is to compare the RD estimates in places where there was better access to vocational schooling with places that had less access. Vocational schools, especially the religious imam-hatip ones, tend to be more common in urban environments. As such, if our estimates were mostly driven by women growing up in urban areas at the time this could lead to another interpretation than our preferred one. In Table 18 we compare the effects of the reform by whether the respondent spent their childhood (up until 12 years old or longer) in a rural or urban location. As the estimates for Rural (Panel B) and Urban (Panel C) show, the effects of the reform on education are nearly identical, but more importantly, there are clear secularizing effects in the childhood-in-rural sample, i.e. the places where access to religious schooling alternatives would have been the lowest. Moreover, the estimates on having a say in the choice of husband, albeit statistically insignificant in itself, is not statistically different from the RD estimate in the urban sample. Meanwhile, neither in the childhood-in-rural nor -in-urban location we find any meaningful effects on having attended vocational schooling. This provides further evidence that our effects on ever-married women's education and empowerment outcomes are unlikely to be driven by switching effects from vocational schooling (including religious schooling) to secular schooling.

Whereas we find that the reform-induced increase in female schooling in the country had large effects on women's religiosity, decision rights and ownership of household durables, we fail to find systematic evidence that these effects are driven by improvements in women's labor market outcomes, the timing of their marriage (and early fertility), or on the 'quality' of their spouse. Among these latter outcomes, our estimates tend to vary nontrivially with the specification. As none of the validity tests conducted in Section 3.3 give cause for concern over the RD design, one possible reason for why some of the results are sensitive to the specification may be because treatment effects are heterogeneous across the studied sample.

In particular, we expect the reform to have heterogeneous effects due to disparities in constraints facing female human capital accumulation in Turkey. Some of these constraints may have to do with market imperfections while others may have to do more with religious or social beliefs making female participation in co-ed forms of education under a headscarf ban controversial. Turkey's education system is deeply ingrained with secularism but a fair share of the population exhibit religiously conservative beliefs. As religious conservatism tends to overlap with economic status, this has resulted in a situation where those women who might benefit the most from education, the poor and pious, also tend to be the ones facing the toughest constraints to it (Meyersson [58]).

To examine the effect of the reform depending on varying constraints to education, we separate individuals by their mother's education level. Absent specific data on parental levels of religiosity or

income, we use the respondent's mother's education level as a proxy measure of female constraints to education as well as a significant predictor of religious conservatism and poverty of the respondent's parents. Specifically, we split the sample by whether or not the respondent's mother had any formal education. This creates two groups of roughly the same size since around half of the women in our sample had mothers with no formal education. As we demonstrate below, this measure of mother's education creates a strong separation in educational attainment as well as religiosity and decision rights. Moreover, among the quarter of the sample that reported Kurdish as their mother's main language, more than 80 percent fall into the low mother's education group, and thus make up around half of the individuals in this group. As such the higher constraints to education among this group could come from factors related to not just poverty and piety, but also that of being an ethnic minority.[39]

To ease the comparison of estimates in the full data set as well as the subgroups, we focus on specifications using a constant bandwidth across outcomes (namely the optimal bandwidth from the first-stage regression in column 1 of Table 2) and estimate the RD treatment effects on outcomes for the two subsamples: women whose mothers had no formal schooling (low mother's education sample) and women whose mothers had some formal schooling (high mother's education sample). Table 6 provides the results. For ease of comparison, in panel A of Table 6, we report RD results near-identical to those in Table 2. In panels B and C of Table 6, we provide the estimated effects of the reform on low and high mother's education subsamples respectively. The first rows in panels B and C of Table 6, showing the outcome means, illustrate the severe disparity in education levels between these two groups. Women whose mothers had any formal education had on average 8 years of schooling, whereas women whose mothers had no formal education had only 4.6 years of schooling. Similar differences can be seen in completion rates for primary, junior high, and high (as well as vocational) school in rows 2 through 5.

The results in Table 6 demonstrate that, once separated into subsamples based on the respondents' mothers' education, the reform's effects on education differ substantially. The first column of the table shows that the reform led to an additional 0.7 year of schooling for respondents with low mother's education, while it led to a 1.3 year increase for those with high mother's education. The difference in estimates between these two groups are economically significant (0.6 years), but not statistically significant, as the SUR test in Panel D fails to reject the hypothesis that the difference is equal to zero with a p-value of 0.12. In the rest of the table, we examine the effects of the reform on completion rates at different levels of education and we find important differences that are both economically and statistically significant. Women whose mothers had no formal schooling (who were likely to face greater social and economic constraints to access education) did get more education, but these effects were concentrated in junior high school (a 15ppt increase) and, to a lesser extent, in primary education (a 7ppt increase). We observe no significant effect on high school completion for this subsample. On the

[39]There are several reasons to expect particularly high constraints to participation for ethnic Kurds. There exists substantial discrimination against women in Kurdish communities (Kirdar [43]), the constitution's restriction on Turkish as the exclusive language of instruction makes Kurdish students less likely to succeed in school and more likely to drop-out (McClure [55]), and Kurds tend to be poorer, hence more likely to drop out for economic reasons (Kaya [42]. The ongoing conflict between the Turkish state and the Kurdistan's Worker's Party (PKK) has over time also resulted in increased security risks for teachers and pupils alike, higher student-to-teacher ratios and a dearth of schooling equipment. At the same time, state interference is widely prevalent in the Kurdish-speaking regions, both in terms of cultural restrictions as well as outright security restrictions, creating significant hostilities toward the Turkish state and its public schools (Marcus [54]).

other hand, for women whose mothers had some formal schooling (who were likely to be less constrained), the effects are larger in terms of junior high school completion (26ppt increase) as well as high school completion (13ppt increase). Therefore, a clear difference in terms of how the reform affected women's education across the two subsamples was that it had more persistent effects on education for women whose mothers had some formal education. The reform enabled these women to remain in school, beyond the duration required by law. In contrast, for the relatively more constrained, the reform induced a substantial number of women to complete the law-mandated years of schooling, but failed to have effects beyond that.

Next, we test whether the reform had heterogenous effects on empowerment-related outcomes across the two subsamples. In Table 7, we allow the treatment effects on our key set of empowerment-related outcomes to differ by the respondents' mothers' education. Once again, for ease of comparison, we report the full-sample average effects in Panel A. These are nearly identical to RD estimates reported in Table 4, except that we now use a constant bandwidth around the threshold. Panels B and C separate the sample into low and high mother's education subsamples respectively and estimate the RD treatment effects for the two subsamples using the same bandwidth. The outcome means reported in Panels B and C confirm our view that mother's education is a suitable proxy for constraints to education – in nearly all the outcomes respondents with low mother's education exhibit levels consistent with less empowerment (higher religiosity and higher likelihood to receive a brideprice for marriage; lower age at first marriage and lower decision rights over marriage) compared to respondents with high(er) mother's education. When we turn to the RD treatment effects, for most of these empowerment-related outcomes, we find that the reform had similar effects in the two subsamples. Specifically, the treatment effects reported in columns 1, 3 and 4 show that the additional years of schooling induced by the reform led to lower religiosity, higher decision rights and a lower likelihood to receive a brideprice among women in both subsamples. While the magnitudes and the precision of the point estimates vary across the two subsamples, p-values reported in Panel D show that none of these effects are statistically different across the two subsamples. On the other hand, for the outcomes related to marriage timing, employment, and spousal characteristics, we find that the reform had different effects on the two groups. For women whose mothers had no formal education (Panel B), the reform lowered the age at first marriage (by as much as between two-thirds to a whole year depending on the specification) while increasing the likelihood of being employed outside of the agricultural sector, the likelihood of being self-employed, and reducing the likelihood of being an unpaid family worker. On the other hand, the reform had no precise effects on spousal quality (husband's education) or asset ownership. These effects are in stark contrast to women with higher mother's schooling (Panel C), for whom the reform appears to have had an imprecisely estimated delaying effect on marriage, no meaningful effect on employment, while increasing spousal education and ownership of household durables.

These findings are in line with the increase in compulsory schooling (and the higher education level that it brings) empowering women from different backgrounds in Turkey through different channels. For women who were likely to face significant constraints in accessing education (i.e. those with lower mother's education) the reform led to an increase in their likelihood to graduate from (compulsory) junior high school but failed to have a corresponding increase in (voluntary) high school graduation. For them, the key mechanism of empowerment seems to have been greater participation and higher returns

in the labor market, as demonstrated by the increase in their participation in the non-agricultural labor market, higher incidence of self-employment as well as a reduction in the occurrence of unpaid family work. Unpaid family labor is typically associated with working in the family farm or non-farm business, with no formal rights. This is often seen as an inferior occupational form for women, as it is indicative of a high degree of dependence on male relatives (White [74]) and having limited, if any, control over the business earnings. In contrast to this, self-employment likely represents a step up in terms of earnings and women's control over these earnings. Self-employment is still far from regular full-time wage employment, both in terms of earnings and hours worked.[40] Nonetheless, the increase in non-agricultural labor force participation as well as the shift away from unpaid labor suggests a relative improvement in labor market opportunities.

In contrast, for women who were likely to be less constrained in accessing education, the reform did not only induce them to complete junior high school, but also enabled them to go on to complete high school, allowing this to further delay marriage. These women fared better on the marriage market, getting married to men with higher schooling and potentially higher wealth as evidenced by the increase in household durables. While this wealth effect may have enabled them to own more household assets, it did little to facilitate entry into the labor market, which is among the most gender-discriminating labor markets in the world.[41] Moreover, as described above, our analysis of the asset index suggests most of the components driving the positive effect of the reform on wealth are assets related to home production. Thus, it is possible that many women who – through the reform were able to extract additional resources to reduce time spent on household chores – may have chosen to remain homemakers. As such, increased education may have resulted in an improved bargaining position for many of these women, albeit within an institutional context where women rarely work.

Although we lack direct measures of a woman's bargaining power vis-a-vis her husband, the DHS includes several attitude variables that are relevant for our purpose. In Table 16 in the Appendix we show that there is a significant treatment effect on agreeing with the statement that "women should work if they wish to". Effects on other attitude-related outcomes reveal estimates in line with a relative empowerment. The same can be said when investigating attitudes towards domestic violence (Table 17 in the Appendix). Of particular interest is a negative treatment effect on the respondents saying that they find domestic violence is justified if a woman "speaks up to her husband". In addition, although we do not find empowering effects of education for all questions asked on gender-related attitudes, we do test for joint significance for the effect of the reform across all these outcomes and are able to reject the null hypothesis of zero joint effects at a 10 percent level. [42]

One finding that is somewhat surprising is the negative effect of schooling on the marriage age of women from more constrained (with lower mother's schooling) backgrounds. Here, it is important to understand not just the reform but also the nature of some of the constraints themselves. The reform clearly occurred with an explicit goal of raising education rates among the poorer and socially

[40] In one study by Tansel [68], summary statistics showed self-employed women earning an hourly wage ten percent lower than wage-earning women, whereas the former worked thirty percent less hours per week.

[41] In 2014, Turkey's ranked at the bottom of the World Economic Forum's gender rankings for labor force participation, as number 128 out of 140 countries http://reports.weforum.org/global-gender-gap-report-2014/economies/.

[42] As such, this is another group of outcomes for which our results differ from those of Dincer et al [20], who find "no evidence that it changed attitudes toward gender inequality".

conservative who, for voluntary education, either opted out entirely or opted for types of education more amenable for conservative families, such as religious schools. As discussed in Meyersson [58], an important source of conservative parents' unwillingness to send their daughters to school was the ban on wearing a headscarf (as well as co-ed classes). While the reform itself mandated three additional years of junior high school by law, it remained silent on the subsequent, and still voluntary, high school education. As such, after the compulsory eight years were completed, constraints on further female education persisted, preventing women among the poor and pious to continue their education. Without higher education to further delay marriage, women with no more than eight years of schooling may have had little available options for social advancement except for marriage, especially if they could now have a say in whom to marry. [43] Furthermore, among the pool of women without high school, those with at least junior high school would likely stand out as above-average matches within their category, resulting in a faster match in the marriage market. A faster match in the marriage market, and given the usual high correlation between age at first marriage and age at first birth (which is circa 0.9 among the 15-26-year-olds in our sample), further means it is not surprising that we also do not find any meaningful reducing effects of education on early fertility.

6 Concluding Remarks

> *"Teachers are the one and only people who save nations" – Turkish proverb widely attributed to Mustafa Kemal Atatürk*

We implement an RD design to exploit exogenous variation in years of schooling generated from a reform increasing compulsory schooling in Turkey. The empirical design employed in the paper provides a solution to an identification problem where women's years of schooling are endogenous to individual characteristics. In this case, women staying longer in school likely exhibit preexisting traits more conducive to female participation in education confounding inference from basic correlations. This will be particularly problematic in socially conservative Turkey. The main contribution of this paper is thus to evaluate the effect of education in isolation from those factors in a majority-Muslim setting with low levels of women's rights.

We find that the reform led to an increase of one additional year of schooling for the average woman in our sample, and this had significant secularizing as well as empowering effects. Women in treated cohorts exhibit lower religiosity, higher decision rights (over their marriage) and have higher consumption of household durables. Furthermore, education seems to lead to empowerment through different channels depending on the degree of constraints facing educational participation. Women with less severe constraints in accessing education are more likely to stay on beyond the compulsory duration of schooling and fair better on the marriage market (marrying more educated husbands), but there is no impact on their labor force participation. On the other hand, women with greater constraints to access education only attain the compulsory level of schooling, do not marry better-educated husbands, but experience improved labor market outcomes, particularly through self-employment.

[43] Also, as long as women have a say in spousal choice, marriage may serve as a way to leave a socially conservative and patriarchal family home.

Our findings suggest that broad expansions of compulsory schooling, even in the presence of significant restrictions to women's rights, may have important empowering effects. Moreover, to the extent that low women's rights adhere from religiously conservative values, education's secularizing effect is consistent with this. Yet, the diverging constraints facing female participation also result in different channels through which education operates, and each of these appear to be unable to single-handedly bridge the gender gap – women facing higher constraints do not continue onto subsequent voluntary forms of education, while less constrained women study further on but they stay out of the labor market. As such, this underlines the limit in what education may accomplish in socially conservative communities, at least in the short to medium term. Having said that, our heterogeneity analysis highlights the importance of mother's education in alleviating constraints on their daughter's education and life choices. Hence, the empowering effects of higher education induced by the reform are likely to remain relevant even in the long run, for the following generations.

In terms of external validity, as with most RD designs our estimates are, in their narrowest interpretation, relevant for the set of ever married women, born during a narrow time frame around the discontinuity date, who complied with the reform. As we discuss in the text, there are reasons to believe these are disproportionally represented among the nation's poor and pious who, despite the possibility of a divergence between the RD treatment effect and the average treatment effect, were the main target group of the reform and therefore of significant importance. As such, it is our view that any loss in generality in terms of evaluating the effect of education on the average Turkish woman is made up by our focus on those who needed education the most.

With regards to the institutional setting, it is impossible to separate the choice to change the schooling law from the political events at the time of the reform, as the new law became implemented in a broader push by Turkey's secular elite, especially the military, in what is today referred to as the "February 28 process", to halt the spread of public displays of Islamic preferences. By expanding the compulsory years of schooling, without affecting the curriculum, it hoped that the already secularized education system would do its part to mold students along a more secular ideal. Generalizations to other institutional settings where the secular component of public education is less salient should be done with utmost care. Nonetheless, the history of large-scale expansions in public education in the Muslim world is full of often-authoritarian rulers seeing secular education as a key instrument for modernization. Our study sheds light not just on the empowering effects of education, but also its importance as an instrument of youth indoctrination.

Recently, the 1997 reform's role in Turkey's political economy has become even more important in light of education reforms implemented under the religiously conservative government led by Recep Tayyip Erdoğan. In a speech in 2012, Erdoğan made clear his desire to raise a "religious youth"', criticizing the secular influences in the education system, proclaiming that "education is no longer the guinea pig of certain segments in society".[44]

And given our main results on education's effect on measures of religiosity, they are consistent with the government's claim that the country's education system could have social engineering consequences. Yet it has also had important empowering effects for women, especially through stronger decision rights

[44] "Turkish school reforms raise debate on Islamism", Reuters, Mar 20th, 2012 http://uk.reuters.com/article/2012/03/20/uk-turkey-education-idUKBRE82J0GB20120320

on marriage and fertility choices.

Shortly after Erdoğan's 2012 speech, the Turkish government pushed through parliament a new education bill which partly reversed the 1997 reform by anew making enrollment in religious junior high schools optional. The new law also extended compulsory schooling to twelve, up from the previous eight, years. In 2014, it lifted the ban on headscarfs in public high schools.[45]

To many observers, these reforms bear the hallmark of an Islamist government trying to remold the education system to create new pious generations in place of previous secular ones.[46] And to the extent that religious beliefs are correlated with voting for religious political parties, and given our documented negative effects of education on religiosity, it would be in Erdoğan's interest to attempt to instill the education system in opposite ways than what his secular predecessors have done. Yet some of the most recent reforms could also end up lowering constraints to female participation, which arguably originate not just in religiously conservative parents, but also its combination with voluntary forms of post-primary education under strict secular restrictions to participation. As we document, those women from the relatively more constrained backgrounds had severe difficulty continuing through high school after their compulsory education was done (likely a result of the noted secular restrictions), limiting avenues for social advancement. So just as recent education reforms in Turkey may be a shift in the social engineering motives in the education system, they could also end up lowering participation constraints. The tradeoff between having a secularly influenced education system versus lowering participation constraints for those who need it the most is beyond the scope of this paper, but is nonetheless an exciting future area of research.

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Table 1: Summary Statistics of Ever Married Women 16-26 years of age

	Mean	SD	Obs
Panel A: Education			
Yrs. of Schooling	6.29	3.80	1557
Primary School	0.81	0.39	1557
Jr. High School	0.39	0.49	1557
High School	0.21	0.41	1557
Vocational School	0.06	0.23	1557
Panel B: Religiosity			
Religiosity Index	0.43	0.24	1554
Wears headscarf	0.77	0.42	1555
Attended Qur'an course	0.44	0.50	1557
Regular Prayer	0.39	0.49	1555
Irregular Prayer	0.71	0.46	1555
Fasting	0.89	0.31	1554
Panel C: Other outcomes			
Age of First Marriage	18.75	2.77	1557
Own marriage decision	0.55	0.50	1554
Bridesmoney paid	0.19	0.39	1557
Employed in non-agricultural sector	0.10	0.30	1557
Self-employed	0.03	0.16	1555
Unpaid family worker	0.08	0.27	1555
Husband's Years of Schooling	7.97	3.55	1546
Asset ownership index	0.42	0.16	1539
Panel D: Covariates			
Non-Turkish-speaking mother	0.31	0.46	1557
Father completed Primary Sch.	0.75	0.43	1557
Mother completed Primary Sch.	0.40	0.49	1557
Father completed Jr. High. Sch.	0.19	0.40	1557
Mother completed Jr. High. Sch.	0.05	0.22	1557
Consanguineous Parents	0.26	0.44	1557
Rural	0.28	0.45	1557

Notes: The table shows the mean, standard deviation, and number of observations from the 2008 Turkish Demographic Health Survey ever-married women module. The sample includes married women born within 60 months before or after September 1986. Detailed variable descriptions are provided in Appendix A.

Table 2: RD Treatment Effects on Schooling

Outcome	Years of Schooling			Min. Yrs. of Schooling			Last Attended Vocational
				5 Primary	8 (Jr. High)	11 (High)	
Polynomial	Linear	Quadratic	Cubic	Linear	Linear	Linear	Linear
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Bandwidth $\hat{h}$							
Mean	6.32	6.32	6.32	0.80	0.38	0.23	0.06
Treatment	1.018***	1.004***	1.004***	0.026	0.218***	0.079***	0.005
	(0.213)	(0.295)	(0.357)	(0.028)	(0.035)	(0.028)	(0.021)
Bandwidth	69	69	69	52	77	95	71
Obs	1777	1777	1777	1343	2001	2412	1849
Panel B: Bandwidth $\hat{h}/2$							
Treatment	1.087***	1.015***	1.124*	0.064*	0.167***	0.084**	0.012
	(0.246)	(0.356)	(0.625)	(0.038)	(0.040)	(0.033)	(0.028)
Bandwidth	34	34	34	26	39	48	36
Obs	923	923	923	690	1027	1267	949
Panel C: Bandwidth $\hat{h}/3$							
Treatment	0.901***	0.917**	1.186*	0.057	0.128**	0.104***	0.037
	(0.290)	(0.381)	(0.611)	(0.042)	(0.049)	(0.034)	(0.032)
Bandwidth	23	23	23	17	26	32	24
Obs	607	607	607	467	690	844	642
Panel C: Bandwidth $2\hat{h}$							
Treatment	0.573**	1.227***	0.858**	0.011	0.168***	0.007	-0.014
	(0.226)	(0.315)	(0.394)	(0.026)	(0.039)	(0.027)	(0.020)
Bandwidth	137	137	137	104	155	191	143
Obs	3279	3279	3279	2577	3676	4411	3391

Notes: Data is from the *Ever Married Module* of the *2008 Turkey Demographic and Health Survey*. Panel A, B, and C report local RD regressions with linear, quadratic, and cubic polynomials respectively in the month-year-of-birth. The optimal bandwidth is determined using the Imbens and Kalyanaraman [40] algorithm. The dependent variable in columns (1)-(4) is number of completed years in school and the four columns include observations within the optimal bandwidth $\hat{h}$, $\hat{h}/2$, $\hat{h}/3$, and $2\hat{h}$ respectively. The dependent variables in columns (5)-(7) are dummy variables equal to one if the respondent completed junior high school (or above), high school (or above), primary school (or above) respectively. The dependent variable in column (8) is a dummy variable equal to one if the final school respondent attended was a vocational school (which includes religious schools). All specifications control for a set of dummy variables for the type of education respondent's father/mother has completed (no schooling, completed primary, junior-high or higher-level of school), a dummy variable equal to one if the respondent's mother's primary language was different from Turkish, a dummy variable equal to one if the respondent lives in a rural location, a dummy for whether the respondent's parents were related by blood, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered at the month-year cohort level.

Table 3: RD Treatment Effect of Education on Measures of Religiosity

	Religiosity Index	Wears Headscarf	Attended Quran study	Prays Regularly (5/day)	Prays At All	Fasts Regularly
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: OLS with optimal bandwidth $\hat{h}$						
Mean	0.43	0.77	0.44	0.39	0.72	0.88
Yrs. of Schooling	-0.011***	-0.035***	0.001	-0.011**	-0.015***	-0.009***
	(0.002)	(0.003)	(0.004)	(0.004)	(0.003)	(0.002)
Bandwidth	66	71	62	65	90	101
Obs	1679	1847	1591	1680	2294	2499
Panel B: Linear RD with optimal bandwidth $\hat{h}$						
Treatment	-0.058***	-0.076**	-0.096**	-0.073	0.006	-0.014
	(0.022)	(0.037)	(0.045)	(0.045)	(0.036)	(0.027)
Bandwidth	66	71	62	65	90	101
Obs	1679	1847	1591	1680	2294	2499
Panel C: Quadratic RD with optimal bandwidth $\hat{h}$						
Treatment	-0.077**	-0.058	-0.096	-0.054	-0.032	-0.001
	(0.033)	(0.053)	(0.060)	(0.071)	(0.052)	(0.036)
Bandwidth	66	71	62	65	90	101
Obs	1679	1847	1591	1680	2294	2499
Panel D: Linear RD with bandwidth $\hat{h}/2$						
Treatment	-0.071**	-0.078	-0.086	-0.065	0.015	-0.031
	(0.029)	(0.051)	(0.057)	(0.060)	(0.045)	(0.028)
Bandwidth	33	36	31	33	45	50
Obs	882	948	826	883	1218	1322
Panel E: Linear RD with bandwidth $2\hat{h}$						
Treatment	-0.038**	-0.047	-0.106***	-0.024	0.008	-0.004
	(0.018)	(0.032)	(0.039)	(0.039)	(0.031)	(0.026)
Bandwidth	132	142	124	131	180	201
Obs	3133	3389	2979	3123	4202	4648
Panel F: Linear RD-2SLS with bandwidth $\hat{h}$, Yrs. of Schooling						
Yrs. of Schooling	-0.061**	-0.073**	-0.093*	-0.079	0.007	-0.017
	(0.027)	(0.031)	(0.048)	(0.055)	(0.040)	(0.033)
F-stat	18.3	24.2	18.6	17.9	18.1	14.5
Bandwidth	66	71	62	65	90	101
Obs	1679	1847	1591	1680	2294	2499

Notes: Data is from the *Ever Married Module* of the *2008 Turkey Demographic and Health Survey*. Panel A reports OLS results with years of schooling as the independent variable for an optimal bandwidth $\hat{h}$ determined by the Imbens and Kalyanaraman [40] algorithm. Panel B, using the same bandwidth, reports reduced-form RD treatment effects of being born after September 1986 with a linear control function in month-year-of-birth on each side of the discontinuity. Panel C reports results using the same bandwidth, but controlling for a quadratic function of the running variable (birth month). Panels D and E report results from the same specification as in Panel B, but using half and double of the optimal bandwidth $\hat{h}$ respectively. Panel E reports results from an instrumental variable estimation, using the treatment as an instrument for years of schooling and the same optimal bandwidth $\hat{h}$ as in Panels A through C. The dependent variable in column (1) is a weighted average of five indicator variables on religiosity (wearing a headscarf, attended Qur'an course, regular prayer, ever praying, and regularly fasting) where the weights are defined as $w_i = (1-\mu_i)/\sum_J (1-\mu_j)$ where μ_i is the mean of religiosity variable i. The dependent variable in column (2) is a dummy variable equal to one if the respondent reports that she regularly wears a headscarf when going out on the street. The dependent variable in column (3) is a dummy variable equal to one if the respondent reported that she has ever attended a course to study the Qur'an. The dependent variable in column (4) is a dummy variable equal to one if the respondent reported that she regularly performs namaz (i.e. prays five times a day, every day). The dependent variable on column (5) is a dummy variable equal to one if the respondent reported that she performs namaz (Muslim prayer) occasionally or regularly (five times a day). The dependent variable in column (6) is a dummy variable equal to one if the respondent reported that she fasts regularly during the month of Ramadan. All specifications control for a set of dummy variables for the type of education respondent's father/mother has completed (no schooling, completed primary, junior-high or a higher level of school), a dummy variable equal to one if the respondent's mother's primary language was different from Turkish, a dummy variable equal to one if the respondent lives in a rural location, a dummy for whether the respondent's parents were related by blood, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered by month-year-cohort.

Table 4: RD Treatment Effect of Education on Empowerment

	Age at marriage	Decision on marriage	Brideprice paid	Employed non-ag	Self-employed	Unpaid labor	Husband's education	Asset index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: OLS with optimal bandwidth $\hat{h}$								
Mean	18.63	0.54	0.20	0.14	0.03	0.08	8.01	0.43
Yrs. of Schooling	0.207***	0.036***	-0.015***	0.023***	-0.003**	-0.007***	0.452***	0.014***
	(0.023)	(0.003)	(0.004)	(0.003)	(0.001)	(0.002)	(0.020)	(0.001)
Bandwidth	52	94	53	109	85	76	122	81
Obs	1343	2352	1371	2695	2178	1980	2944	2067
Panel B: Linear RD with optimal bandwidth $\hat{h}$								
Treatment	-0.094	0.113***	-0.080**	0.019	0.025*	-0.012	0.165	0.025**
	(0.265)	(0.041)	(0.032)	(0.021)	(0.014)	(0.021)	(0.267)	(0.012)
Bandwidth	52	94	53	109	85	76	122	81
Obs	1343	2352	1371	2695	2178	1980	2944	2067
Panel C: Quadratic RD with bandwidth $\hat{h}$								
Treatment	-0.539	0.170***	-0.070	0.056**	0.039**	-0.020	0.133	0.033*
	(0.410)	(0.058)	(0.047)	(0.026)	(0.019)	(0.030)	(0.359)	(0.018)
Bandwidth	52	94	53	109	85	76	122	81
Obs	1343	2352	1371	2695	2178	1980	2944	2067
Panel D: Linear RD with bandwidth $\hat{h}/2$								
Treatment	-0.685*	0.132**	-0.095**	0.049**	0.030*	-0.029	0.407	0.041***
	(0.346)	(0.052)	(0.039)	(0.022)	(0.017)	(0.027)	(0.292)	(0.015)
Bandwidth	26	47	26	55	43	38	61	40
Obs	690	1237	717	1413	1135	1026	1580	1072
Panel E: Linear RD with bandwidth $2\hat{h}$								
Treatment	-0.274	0.092**	-0.040	-0.029	0.017	-0.008	-0.127	0.008
	(0.228)	(0.038)	(0.027)	(0.020)	(0.013)	(0.018)	(0.248)	(0.011)
Bandwidth	104	187	105	218	171	152	244	161
Obs	2577	4347	2614	4992	4000	3642	5388	3773
Panel F: Linear RD-2SLS with bandwidth $\hat{h}$, Yrs. of Schooling								
Yrs. of Schooling	-0.085	0.138***	-0.065**	0.026	0.026	-0.013	0.232	0.025**
	(0.242)	(0.048)	(0.029)	(0.029)	(0.016)	(0.022)	(0.348)	(0.011)
F-stat	27.3	14.9	26.5	11.6	21.9	18.9	9.9	24.0
Bandwidth	52	94	53	109	85	76	122	81
Obs	1343	2352	1371	2695	2178	1980	2944	2067

Notes: Data is from the *Ever Married Module* of the *2008 Turkey Demographic and Health Survey.* Panel A reports OLS results with years of schooling as the independent variable for an optimal bandwidth $\hat{h}$ determined by the Imbens and Kalyanaraman [40] algorithm. Panel B, using the same bandwidth, reports reduced-form RD treatment effects of being born after September 1986 with a linear control function in month-year-of-birth on each side of the discontinuity. Panel C reports results using the same bandwidth, but controlling for a quadratic function of the running variable (birth month). Panels D and E report results from the same specification as in Panel B, but using half and double of the optimal bandwidth $\hat{h}$ respectively. Panel E reports results from an instrumental variable estimation, using the treatment as an instrument for years of schooling and the same optimal bandwidth $\hat{h}$ as in Panels A through C. The dependent variable in column (1) is the respondent's completed age at the time of her first marriage. The dependent variable in column (2) is a dummy variable equal to one if the respondent decided on her most recent marriage jointly with her husband (as opposed to being decided by their families). The dependent variable in column (3) is a dummy variable equal to one if the respondent's family received a brideprice from her husband's family upon their wedding. The dependent variable in column (4) is a dummy variable equal to one if the respondent is currently employed in the industrial sector or in services. The dependent variable on column (5) is a dummy variable equal to one if the respondent's position at her current job is 'employer' or an 'own-account' worker. The dependent variable in column (6) is a dummy variable equal to one if the respondent's position at her current job is 'unpaid family worker'. The dependent variable in column (7) is the number of completed years of school by the respondent's spouse.The dependent variable in column (8) is The first principal component of 20 dummy variables, each one equal to one if the respondent's household owns the relevant asset/service. The types of assets/services included in the index are: fridge, gas/electric oven, microwave oven, blender/mixer, dishwasher, washing machine, iron, vacuum cleaner, air-conditioner, cellphone, computer/laptop, internet, plasma-TV (LCD), cable-TV, satellite antenna, DVD-player, camera, car, taxi/mini-bus, tractor. All specifications control for a set of dummy variables for the type of education respondent's father/mother has completed (no schooling, completed primary, junior-high or a higher level of school), a dummy variable equal to one if the respondent's mother's primary language was different from Turkish, a dummy variable equal to one if the respondent lives in a rural location, a dummy for whether the respondent's parents were related by blood, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered by month-year-cohort.

Table 5: RD Treatment Effects of Education on Fertility

	Age at first		First birth	Number of		Incidents of		Decision
	marriage	birth	before 18	pregnancies	children	Low birthweight	Child Death	on Contra-ceptives
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: OLS with optimal bandwidth $\hat{h}$								
Mean	18.63	19.65	0.22	1.45	1.41	0.13	0.04	0.86
Yrs. of Schooling	0.207***	0.165***	-0.016***	-0.096***	-0.087***	-0.009*	-0.007***	0.006*
	(0.023)	(0.028)	(0.005)	(0.010)	(0.008)	(0.004)	(0.001)	(0.003)
Bandwidth	52	60	43	48	63	54	75	73
Obs	1343	1169	855	1284	1627	831	1930	1157
Panel B: Linear RD with optimal bandwidth $\hat{h}$								
Treatment	-0.094	-0.179	-0.030	0.130	0.061	-0.017	-0.011	0.101***
	(0.265)	(0.234)	(0.046)	(0.105)	(0.092)	(0.049)	(0.012)	(0.036)
Bandwidth	52	60	43	48	63	54	75	73
Obs	1343	1169	855	1284	1627	831	1930	1157
Panel C: Quadratic RD with bandwidth $\hat{h}$								
Treatment	-0.539	-0.171	0.026	0.238	0.069	-0.045	0.006	0.114**
	(0.410)	(0.319)	(0.059)	(0.153)	(0.120)	(0.069)	(0.015)	(0.052)
Bandwidth	52	60	43	48	63	54	75	73
Obs	1343	1169	855	1284	1627	831	1930	1157
Panel D: Linear RD with bandwidth $\hat{h}/2$								
Treatment	-0.685*	-0.041	0.076	0.408**	0.094	-0.017	0.008	0.098*
	(0.346)	(0.330)	(0.071)	(0.155)	(0.124)	(0.062)	(0.016)	(0.050)
Bandwidth	26	30	21	24	32	27	37	37
Obs	690	596	435	668	844	446	1004	570
Panel R: Linear RD with bandwidth $2\hat{h}$								
Treatment	-0.274	-0.319	-0.054	-0.031	-0.039	0.014	-0.002	0.099***
	(0.228)	(0.213)	(0.038)	(0.091)	(0.083)	(0.043)	(0.010)	(0.030)
Bandwidth	104	120	86	96	127	108	150	147
Obs	2577	2409	1727	2430	3051	1537	3566	2303
Panel F: Linear RD-2SLS with bandwidth $\hat{h}$, Yrs. of Schooling								
Yrs. of Schooling	-0.085	-0.142	-0.026	0.125	0.061	-0.011	-0.011	0.089**
	(0.242)	(0.193)	(0.041)	(0.109)	(0.097)	(0.032)	(0.012)	(0.038)
F-stat	27.3	23.0	17.4	20.9	19.4	30.5	19.4	17.4
Bandwidth	52	60	43	48	63	54	75	73
Obs	1343	1169	855	1284	1627	831	1930	1157

Notes: Data is from the *Ever Married Module* of the *2008 Turkey Demographic and Health Survey*. Panel A reports OLS results with years of schooling as the independent variable for an optimal bandwidth $\hat{h}$ determined by the Imbens and Kalyanaraman [40] algorithm. Panel B, using the same bandwidth, reports reduced-form RD treatment effects of being born after September 1986 with a linear control function in month-year-of-birth on each side of the discontinuity. Panel C reports results using the same bandwidth, but controlling for a quadratic function of the running variable (birth month). Panels D and E report results from the same specification as in Panel B, but using half and double of the optimal bandwidth $\hat{h}$ respectively. Panel E reports results from an instrumental variable estimation, using the treatment as an instrument for years of schooling and the same optimal bandwidth $\hat{h}$ as in Panels A through C. The dependent variable in column (1) is the respondent's completed age at the time of her first marriage; in column (2) the respondent's completed age at the time of her first birth; in column (3) whether the respondent gave birth before the age of 18; in column (4) the respondent's number of pregnancies; in column (5) the respondent's number of children alive at the time of the survey; in column (6) an indicator variable for birthweight under 2500 grams; in column (7) an indicator variable for child death; and in column (8) whether the respondent had a say in the use of contraceptives. All specifications control for a set of dummy variables for the type of education respondent's father/mother has completed (no schooling, completed primary, junior-high or a higher level of school), a dummy variable equal to one if the respondent's mother's primary language was different from Turkish, a dummy variable equal to one if the respondent lives in a rural location, a dummy for whether the respondent's parents were related by blood, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered by month-year-cohort.

Table 6: RD Treatment Effects on Education by Mother's Education

	Years of school	Primary school	Jr. High school	High school	Vocational
	(1)	(2)	(3)	(4)	(5)
Panel A: Local linear RD, Full sample					
Mean	6.34	0.82	0.38	0.21	0.06
Treatment	1.017***	0.025	0.213***	0.090***	0.007
	(0.213)	(0.026)	(0.036)	(0.029)	(0.022)
Panel B: Local linear RD, Low mother's education sample					
Mean	4.62	0.68	0.20	0.07	0.03
Treatment	0.684**	0.013	0.145***	0.030	0.022
	(0.293)	(0.042)	(0.043)	(0.027)	(0.022)
Panel C: Local linear RD, High Mother's Education sample					
Mean	8.00	0.95	0.56	0.34	0.09
Treatment	1.300***	0.051*	0.267***	0.129***	-0.012
	(0.284)	(0.026)	(0.044)	(0.048)	(0.040)
Panel D: Test of difference in coefficients between panel B and C					
p-value	0.118	0.446	0.019	0.054	0.474

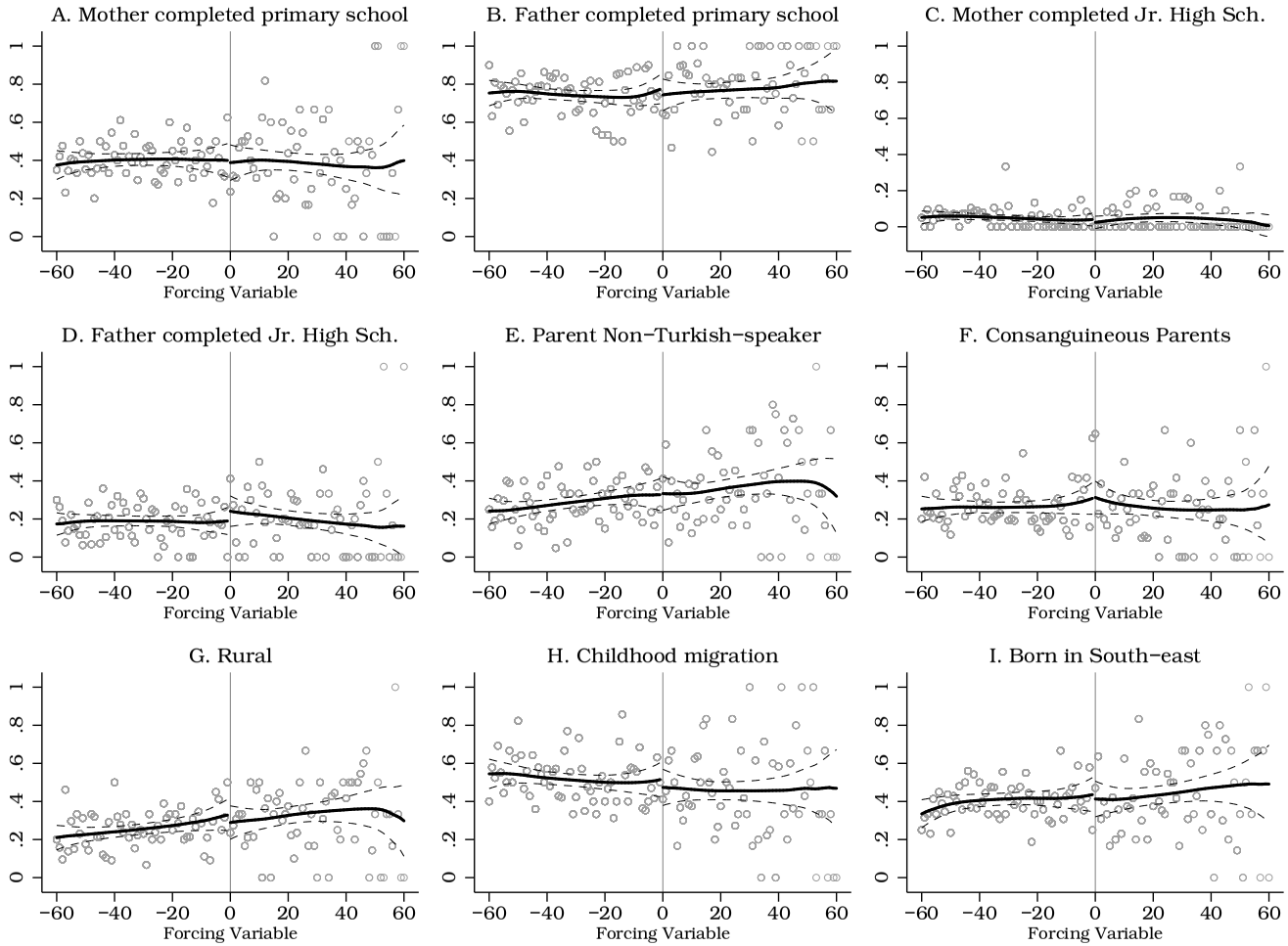
Notes: Data is from the *Ever Married Module* of the *2008 Turkey Demographic and Health Survey*. Panel A reports reduced-form RD treatment effects of being born after September 1986 with a linear control function in month-year-of-birth on each side of the discontinuity. The bandwidth is 69 months in all regressions, which is the optimal bandwidth $\hat{h}$ determined by the Imbens and Kalyanaraman [40] algorithm when years of schooling is the independent variable. Panel B reports results using the same bandwidth, for the subsample of respondents whose mother's had no formal schooling, while Panel C does the same for respondents whose mother's had some formal schooling. Panel D reports the p-value for the test of equality between the treatment effects on the low mother's education and high mother's education (in Panels B and C respectively) subsamples. The dependent variable in column (1) is the number of completed years in school. The dependent variables in columns (2)-(4) are dummy variables equal to one if the respondent completed junior high school (or above), high school (or above), primary school (or above) respectively. The dependent variable in column (5) is a dummy variable equal to one if the final school respondent attended was a vocational school (which includes religious schools). All specifications control for a set of dummy variables for the type of education respondent's father/mother has completed (no schooling, completed primary, junior-high or a higher level of school), a dummy variable equal to one if the respondent's mother's primary language was different from Turkish, a dummy variable equal to one if the respondent lives in a rural location, a dummy for whether the respondent's parents were related by blood, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered by month-year-cohort.

Table 7: RD Treatment Effects on Empowerment by Mother's Education

	Religiosity	Marriage		Brideprice	Employment			Husband's	Asset
	Index	age	Decision	Paid	Non-agr	Self	Unpaid	Yrs of Sch.	Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Local linear RD, Full sample									
Mean	0.43	18.92	0.54	0.18	0.11	0.03	0.08	7.96	0.42
Treatment	-0.052**	-0.190	0.137***	-0.052*	0.048**	0.031**	-0.011	0.199	0.024**
	(0.021)	(0.239)	(0.046)	(0.030)	(0.022)	(0.015)	(0.020)	(0.285)	(0.012)
Panel B: Low mother's education sample									
Mean	0.48	18.50	0.46	0.30	0.06	0.03	0.08	6.87	0.36
1. Linear RD									
Treatment	-0.031	-0.650*	0.142**	-0.045	0.071**	0.057**	-0.057**	-0.611	0.004
	(0.028)	(0.384)	(0.071)	(0.050)	(0.033)	(0.025)	(0.027)	(0.390)	(0.017)
2. Linear RD-2SLS, Yrs of Schooling									
Yrs. of Sch.	-0.046	-0.951	0.211*	-0.065	0.103	0.083	-0.083	-0.863	0.006
	(0.046)	(0.783)	(0.124)	(0.075)	(0.063)	(0.051)	(0.051)	(0.691)	(0.022)
F-stat	5.3	5.4	5.3	5.4	5.4	5.4	5.4	5.9	7.1
Panel C: High Mother's Education sample									
Mean	0.38	19.33	0.63	0.07	0.15	0.03	0.08	9.01	0.49
1. Linear RD									
Treatment	-0.066**	0.281	0.113	-0.057*	0.018	-0.001	0.031	0.914**	0.041***
	(0.032)	(0.227)	(0.070)	(0.031)	(0.033)	(0.021)	(0.036)	(0.461)	(0.016)
2. Linear RD-2SLS, Yrs of Schooling									
Yrs. of Sch.	-0.050**	0.216	0.087*	-0.044*	0.014	-0.001	0.024	0.704**	0.031***
	(0.025)	(0.175)	(0.052)	(0.025)	(0.025)	(0.016)	(0.029)	(0.341)	(0.012)
F-stat	21.6	21.0	20.3	21.0	21.0	21.3	21.3	20.9	22.5
Panel D: Test of difference in coefficients between B1 and C1									
p-value	0.398	0.019	0.779	0.826	0.295	0.082	0.059	0.019	0.096

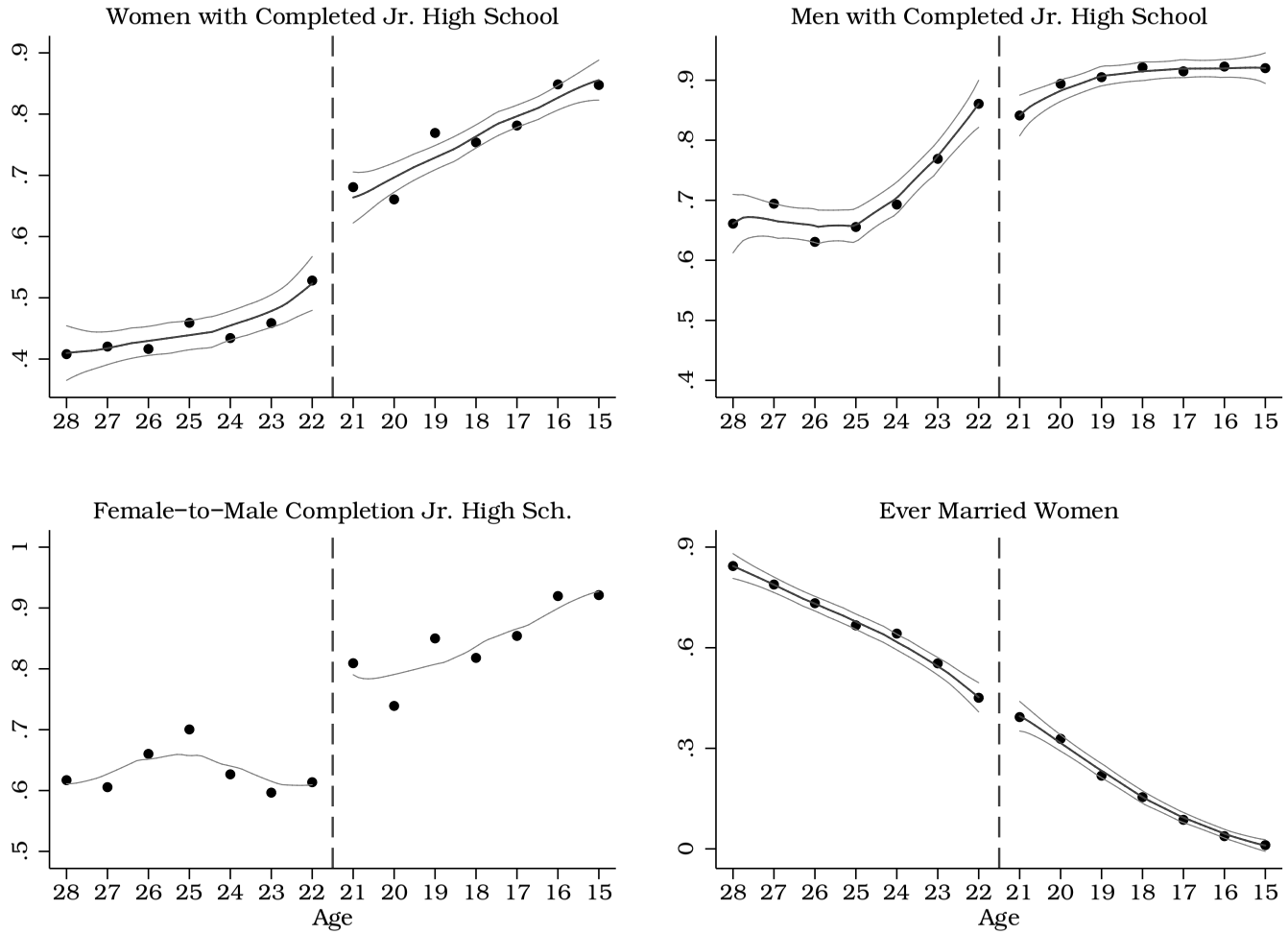
Notes: Data is from the *Ever Married Module* of the *2008 Turkey Demographic and Health Survey*. Panel A reports reduced-form RD treatment effects of being born after September 1986 with a linear control function in month-year-of-birth on each side of the discontinuity. The bandwidth is 69 months in all regressions, which is the optimal bandwidth $\hat{h}$ determined by the Imbens and Kalyanaraman [40] algorithm when years of schooling is the independent variable. Panel B reports results using the same bandwidth, for the subsample of respondents whose mother's had no formal schooling, while Panel C does the same for respondents whose mother's had some formal schooling. Panel D reports the p-value for the test of equality between the treatment effects on the low mother's education and high mother's education (in Panels B and C respectively) subsamples. The dependent variable in column (1) is a weighted average of five indicator variables on religiosity (wearing a headscarf, attended Qur'an course, regular prayer, ever praying, and regularly fasting) where the weights are defined as $w_i = (1-\mu_i)/\sum_J (1-\mu_j)$ where μ_i is the mean of religiosity variable i. For description of the dependent variables in columns (2)-(9), see footnote for Table 4. All specifications control for a set of dummy variables for the type of education respondent's father/mother has completed (no schooling, completed primary, junior-high or a higher level of school), a dummy variable equal to one if the respondent's mother's primary language was different from Turkish, a dummy variable equal to one if the respondent lives in a rural location, a dummy for whether the respondent's parents were related by blood, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered by month-year-cohort.

Figure 1: Balanced Covariates



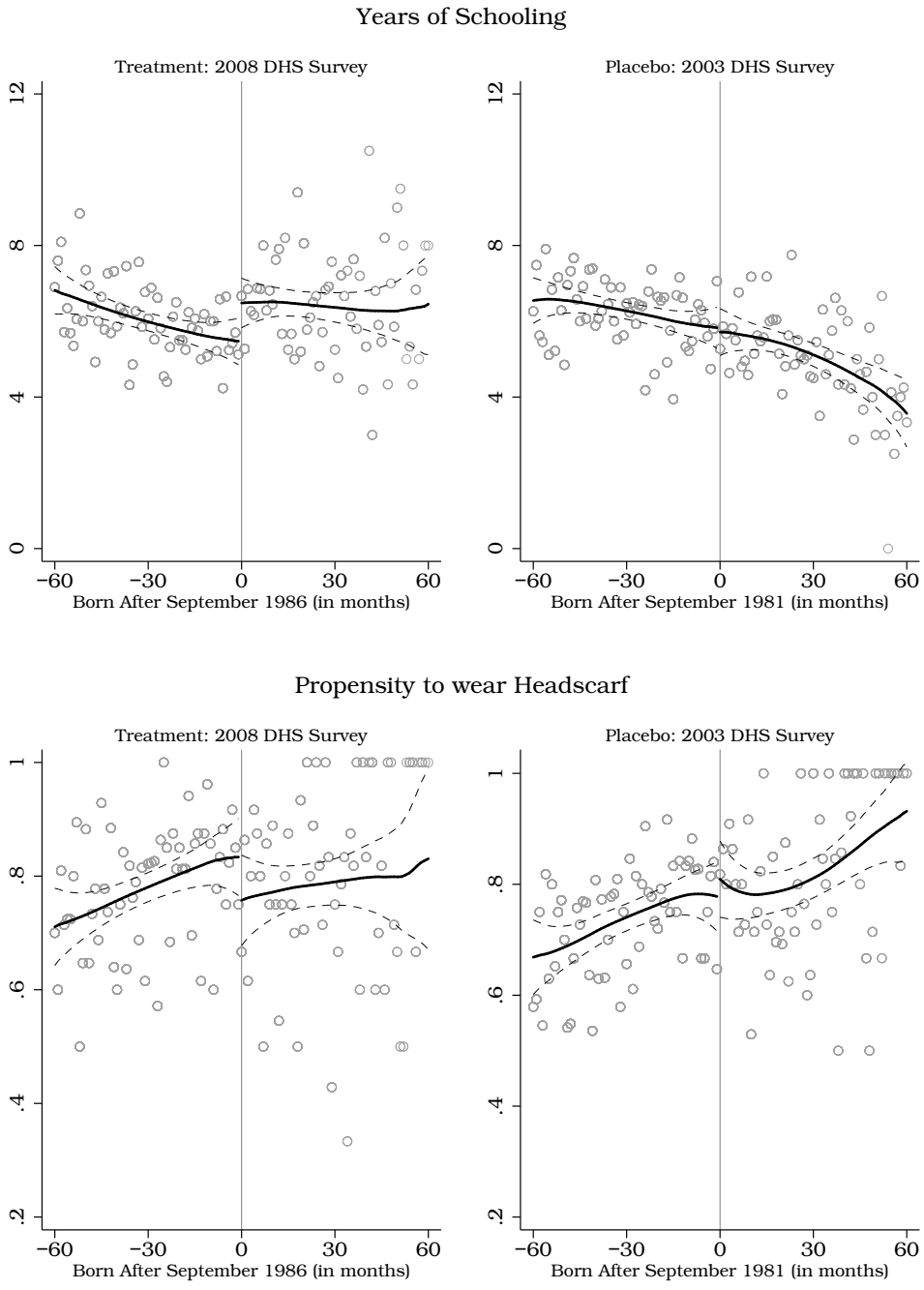
Notes: Data is from the *Ever Married Module* of the *2008 Turkey Demographic and Health Survey*. Figures show pre-determined covariates in monthly bins against the forcing variable 6 years within the threshold of being born in September 1986. The vertical line in each graph represents the cut-off birth date (September 1986). A test of the null hypothesis that the discontinuity jumps in all graphs are jointly zero yields a p-value of 0.82. 95 percent confidence intervals are plotted in light gray lines around the mean level. "Mother/Father completed Primary School" is a dummy variable equal to one if the respondent's mother/father finished primary school or above. "Mother/Father completed Jr. High School School" is a dummy variable equal to one if the respondent's mother/father finished junior high school school or above. "Rural" is a dummy variable equal to one if the respondent lives in a rural location, and zero otherwise. "Parent Primarily Non-Turkish-speaker" is a dummy variable equal to one if neither her mothers' nor her fathers' first language was Turkish. "Consanguineous parents" is a dummy variable equal to one if the respondent reports that her parents were related by blood. "Born in large city" is a dummy variable equal to one if the respondent was born in one of the top-10 most populated provinces in the country. "Childhood migration" is a dummy variable equal to one if the respondent left her residence for more than 3 months during childhood (younger than 12). Born in Southeast is a dummy variable equal to one if the respondent was born in one of the following provinces: Adıyaman, Ağrı, Batman, Bingöl, Bitlis, Diyarbakır, Elazığ, Erzincan, Erzurum, Gaziantep, Hakkari, Kars, Malatya, Mardin, Muş, Siirt, Tunceli, Şanlıurfa, Van, Bayburt, Şırnak, Iğdır, Kilis

Figure 2: 2008 DHS Household Sample



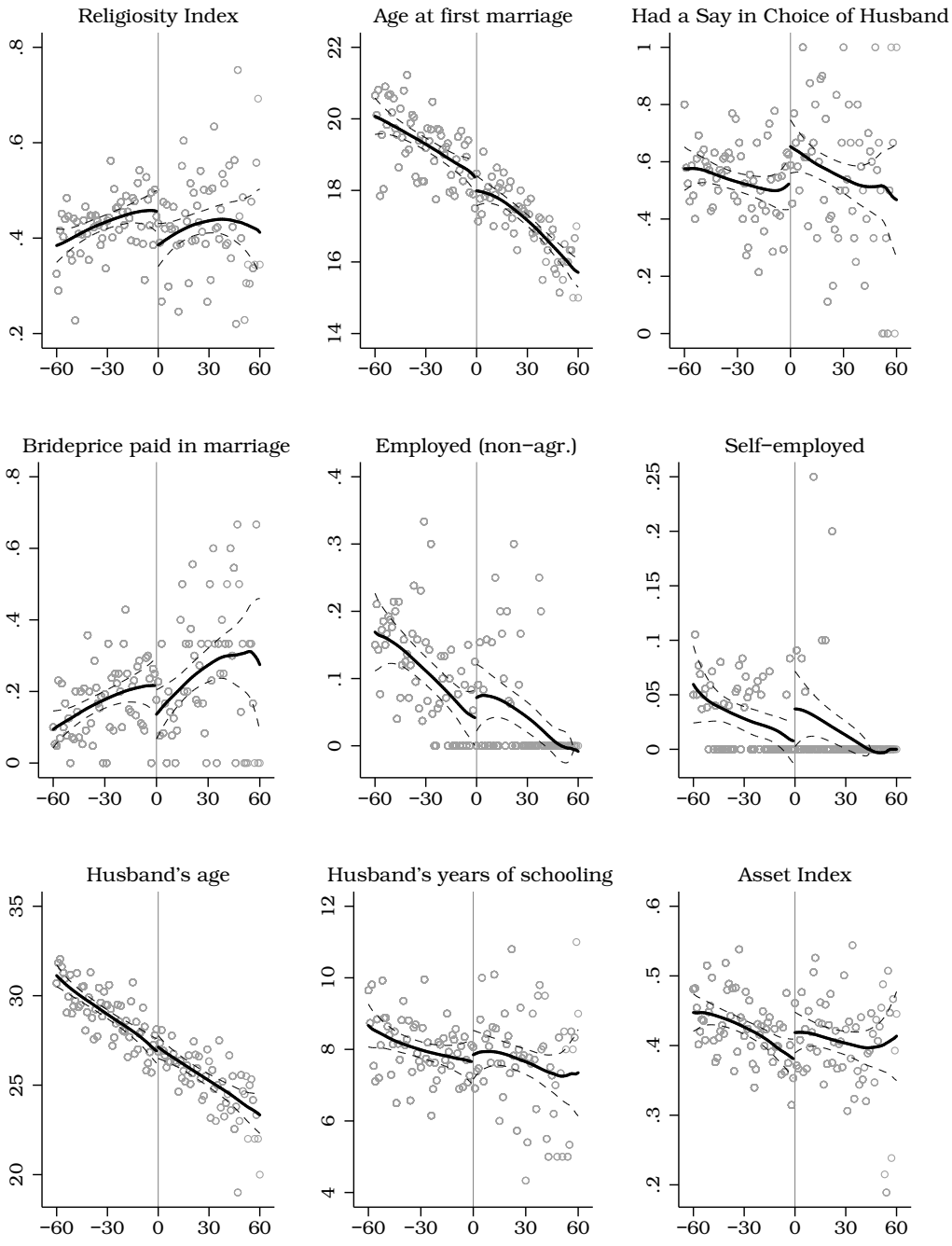
Notes: Data is from the *Household Module* of the *2008 Turkey Demographic and Health Survey*. The forcing variable is the age in years of the relevant individual(s). Upper left hand panel plots the average level of junior high school completion for *all women* in the sampled households, in annual age averages at interview date. The upper right graph plots the average level of junior high school completion for *all men* in the sampled households, in annual age averages at interview date. Lower left hand panel plots the ratios for the annual average female-to-male completion rates in junior high school against the age of the individual. The lower right panel plots the probability of being married in annual age averages at interview date. Whenever possible, 95 percent confidence intervals are plotted in light gray lines around the mean level. In the lower left hand panel no confidence intervals are included as there exists one observation per every cohort level.

Figure 3: Graphical RD: Treatment and Placebo



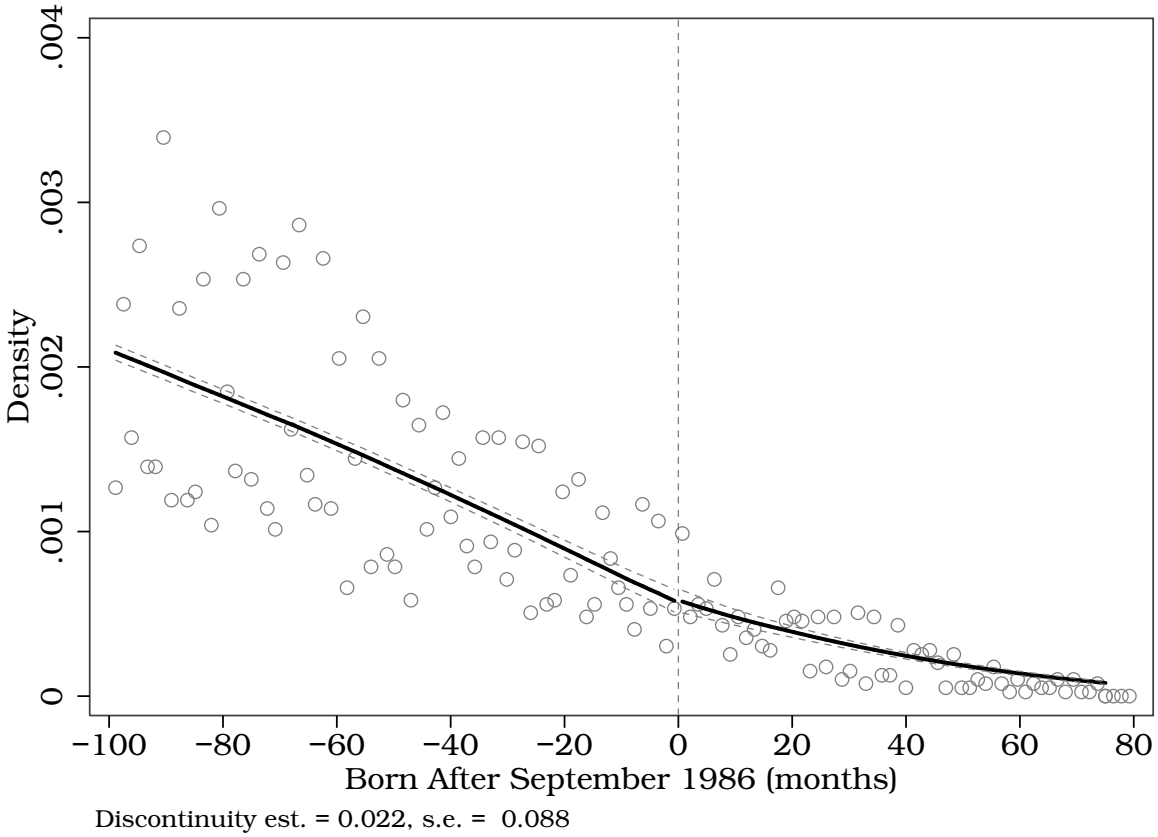
Notes: The sample includes observations from the Turkey DHS ever married women module. The left-hand graphs include observations from the 2008 DHS (to demonstrate treatment effects), while the right-hand graphs include observations from the 2003 DHS (as a placebo test). The forcing variable is the distance in months away from turning 21 in September. The vertical line inside each graph represents the cut-off birth date September 1986 (1981) for the 2008 (2003) survey. Mean levels of the outcome variable are plotted in monthly bins. The black lines represent local linear smoothers fitted to observations on each side of the threshold. 95 percent confidence intervals are plotted in light gray lines around the mean level. "Years of schooling" is the number of completed years of schooling. "Regularly wears headscarf" is a dummy variable equal to one if the respondent reported that she regularly wears a headscarf when she goes out on the street. All outcome variables are self-reported by the respondent.

Figure 4: Graphical RD: Empowerment outcomes



Notes: Data is from the *Ever Married Module* of the *2008 Turkey Demographic and Health Survey*. The forcing variable is the distance in months away from being born in September 1986. The vertical line in each graph represents the cut-off birth date (September 1986). Mean levels of the outcome are plotted in monthly bins. The black lines represent local linear smoothers fitted to observations on each side of the threshold. 95 percent confidence intervals are plotted in dashed, light gray lines. All outcome variables are self-reported by the respondent.

FIGURE 5: DENSITY TEST



Notes: Data is from the *Ever Married Module* of the *2008 Turkey Demographic and Health Survey.* The graph shows the McCrary [56] test of whether there is a discontinuity in the density of month of birth.

A Data variables

- **Outcome variables:**
  - *Years of Schooling* – Number of completed years of school.
  - *Junior High School* – Respondent completed 8 years long education, i.e. both primary and junior high school.
  - *High School* – Respondent completed 11 years long education, i.e. primary, junior high school, and high school.
  - *Vocational School* – The last school attended by the respondent was a vocational school.
  - *Wears Headscarf* – Respondent reported that she regularly wears a headscarf when out on the street.
  - *Attended Quran study* – Reported as having ever attended a course to study the Qur'an.
  - *Prays Regularly (5/day)* – Respondent reported as regularly performing namaz, or prayer.
  - *Prays at All* – Respondent reported as occasionally performing namaz, or prayer.
  - *Fasts Regularly* – Respondent reported as regularly fasting.
  - *Religiosity Index* – An index defined as the weighted average of wearing a headscarf, attended Qur'an course, regular prayer, ever praying, and regularly fasting. The weights are defined as $w_i = \frac{1-\mu_i}{\sum_J (1-\mu_j)}$ where μ_i is the mean of religiosity variable i.
  - *Age at Marriage* – Respondent's achieved age at the time of her first marriage.
  - *Decision on Marriage* – A dummy variable equal to one if the respondent decided on her most recent marriage jointly with her husband (as opposed to being decided by their families).
  - *Brideprice paid* – A dummy variable equal to one if the respondent's family received a brideprice from her husband's family upon their wedding.
  - *Employed in Non-agriculture* – A dummy variable equal to one if the respondent is currently employed in the industrial sector or in services.
  - *Self-employed* – A dummy variable equal to one if the respondent's position at her current job is 'employer' or an 'own-account' worker.
  - *Unpaid labor* – A dummy variable equal to one if the respondent's position at her current job is 'unpaid family worker'.
  - *Husband's education* – Number of completed years of school by the respondent's spouse.
  - *Asset Index* – The first principal component of 20 dummy variables, each one equal to one if the respondent's household owns the relevant asset/service. The types of assets/services included in the index are: fridge, gas/electric oven, microwave oven, blender/mixer, dishwasher, washing machine, iron, vacuum cleaner, air-conditioner, cellphone, computer/laptop, internet, plasma-TV (LCD), cable-TV, satellite antenna, DVD-player, camera, car, taxi/mini-bus, tractor.
  - *Age at First Birth* – Respondent's achieved age at the time of her first birth.
  - *First Birth Before 18* – Respondent gave her first birth at an age below 18 years.
  - *Number of pregnancies* – Number of completed pregnancies.
  - *Number of children* – Number of living children delivered by the respondent.
  - *Low Birthweight* – Respondent gave birth to infant with recorded birthweight less than 2500 grams.

- *Child death* – Respondent had a child who died.
- *Contraception decision* – A dummy variable equal to one if the respondent decided herself or jointly with her husband on the type of contraception currently used [conditional on currently using contraception].

- **Covariates:**
  - *Non-Turkish mother* – A dummy variable taking the value of one if the respondent's mother speaks a non-Turkish language as her mother tongue.
  - *Mother's education* – Dummy variables for whether the respondent's mother has no schooling, completed primary school, junior-high school or a higher level of school.
  - *Father's education* – Dummy variables for whether the respondent's father has no schooling, completed primary school, junior-high school or a higher level of school.
  - *Parents are related* – A dummy variable taking the value of one if the respondent reported her parents as being related by blood, and zero otherwise.
  - *Rural* – A dummy variable taking the value of one if the respondent lives in a rural location, and zero otherwise.
  - *Region dummies* – Dummy variables for each of the five regions where the respondents were surveyed.

- **Outcome Variables in Appendix B:**
  - *Any employment* – A dummy variable equal to one if the respondent reported that she is "currently working", and zero otherwise.
  - *Sector of employment: Agriculture* – A dummy variable equal to one if the respondent is currently employed in the agricultural sector.
  - *Type of employment: Regular wage-job* – A dummy variable equal to one if the respondent's position at her current job is 'wage worker (regular)' or 'salaried, government officer (regular)'.
  - *Type of employment: Daily wage job* – A dummy variable equal to one if the respondent's position at her current job is 'daily waged (seasonal, temporary)'.
  - *Type of husband's job* – Variables related to respondent's husband's type of job are defined identical to respondent's type of job variables, described above. They are reported by the female respondent in the ever-married women module.
  - *Domestic violence attitudes* – A set of seven dummy variables, each equal to one if the respondent stated that she though domestic violence conducted by a husband on his wife was justified under a certain scenario. The scenarios were: (i) if she does not cook (ii) if she burns the food (iii) if she neglects the housework (iv) if she neglects the children (v) if she answers him back (vi) is she wastes money (vii) is she refuses to have sexual intercourse with her husband.
  - *Attitudes on women's status relative to men* – A set of nine dummy variables, each equal to one if the respondent stated that she agrees with a statement on women's status relative to men. The statement are as follows: (i) The important decision in the family can be the women of the family (ii) Men should also do house chores like cooking, washing etc. (iii) A woman may argue with her husband if she disagrees with him (iv) A married woman should work outside the home is she wants to (v) Educating one's daughter is equally important as educating one's son (vi) A woman may go anywhere she wants without her husband's

permission (vii) Women are as smart as men (viii) Women should be more involved in politics (ix) women don't need to be virgins when they get married.

Appendix B Additional Tables and Figures:

TABLE 8: TDHS HOUSEHOLD MODULE: SCHOOLING AND MARITAL STATUS EFFECTS OF REFORM

	Completed Junior High School				Ever Married	
	Women		Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 15-40 age bandwidth, Cubic polynomial						
Mean	0.49	0.49	0.71	0.71	0.61	0.61
Treatment	0.160***	0.115***	-0.034	-0.025	0.033	0.042
	(0.036)	(0.031)	(0.029)	(0.027)	(0.035)	(0.035)
p-value (1)=(3)	0.000					
p-value (2)=(4)		0.001				
Obs	9644	9639	9427	9413	9649	9643
Controls	N	Y	N	Y	N	Y
Panel B: 16-27 age bandwidth, Linear polynomial						
Mean	0.61	0.61	0.82	0.82	0.41	0.41
Treatment	0.144***	0.123***	0.021	0.029	-0.026	-0.024
	(0.028)	(0.024)	(0.022)	(0.021)	(0.028)	(0.027)
p-value (1)=(3)	0.001					
p-value (2)=(4)		0.003				
Obs	5001	4999	4901	4890	5003	5000
Controls	N	Y	N	Y	N	Y

Notes: Data is from the *Household Module* of the *2008 Turkey Demographic and Health Survey*. Panel A reports reduced-form RD estimates using a bandwidth of individuals between 15-40 years of age, a cubic control function in the forcing variable. Panel B reports reduced-form RD estimates using a bandwidth of individuals between 16-27 years of age, a linear control function in the forcing variable. "Treatment" is defined as being 21 years or younger at the time of the survey. The forcing variable measures age below or above 21, normalized to equal 0 at age 21. In columns (1)-(2) and (5)-(6), the sample include all women in the household, while in columns (3)-(4) it includes the men. The dependent variable in columns (1)-(4) is a dummy variable equal to one if the individual completed junior high school or above. The dependent variable in columns (5)-(6) is a dummy variable equal to one if the individual is included in the ever-married women sample. All regressions control for birth province fixed effects, type of birth place (village, province center, district center, or abroad), and whether mother and father are alive respectively. Robust standard errors in parenthesis.

Table 9: Correlates of Being Selected into the Ever-Married Women Sample

	Ever-married		Never-married		Difference
	Observations	Mean (SD)	Obervations.	Mean (SD)	
	(1)	(2)	(3)	(4)	(5)
Literate mother	1614	0.485	2481	0.616	-0.131***
		(0.500)		(0.486)	(0.017)
Mother never went to school	1601	0.510	2412	0.415	0.094***
		(0.500)		(0.493)	(0.019)
Mother finished primary school or above	1601	0.383	2412	0.510	-0.127***
		(0.486)		(0.500)	(0.018)
Mother finished secondary school or above	1601	0.046	2412	0.117	-0.070***
		(0.210)		(0.321)	(0.006)
Literate father	1611	0.875	2269	0.926	-0.050***
		(0.331)		(0.263)	-0.050
Father never went to school	1544	0.170	2088	0.132	0.039**
		(0.376)		(0.338)	(0.014)
Father finished primary school or above	1544	0.751	2088	0.817	-0.066**
		(0.433)		(0.387)	(0.022)
Father finished secondary school or above	1544	0.190	2088	0.296	-0.106***
		(0.393)		(0.296)	(-0.106)
Nonturkish	1614	0.279	2855	0.269	0.009
		(0.449)		(0.444)	(0.019)
Born in village	1614	0.449	2869	0.352	0.098***
		(0.498)		(0.478)	(0.016)
Born in village or town	1614	0.705	2869	0.574	0.131***
		(0.456)		(0.495)	(0.012)

Notes: Data is from *2008 Turkey Demographic and Health Survey*. Columns (1) and (2) report the number of observations, mean and standard deviations of pre-determined characteristics for ever-married women sample. Columns (3) and (4) do the same for never-married women. Column (5) reports the difference in pre-determined characteristics of ever-married and never-married women, with standard errors clustered at the annual birth cohort level.

TABLE 10: RD TREATMENT EFFECTS ON EDUCATION FOR MARRIED VS UNMARRIED WOMEN

	(1)	(2)	(3)	(4)
Panel A: Dependent Variable – Years of Schooling				
	Sample: Ever-married Women		*Unmarried Women*	
Treatment	0.734***	0.734***	-0.137	-0.137
	(0.274)	(0.274)	(0.285)	(0.285)
Outcome Mean	6.99	6.99	9.32	9.32
Bandwidth	5	5	5	5
Obs	1361	1361	1827	1827
Panel B: Dependent Variable – Completed Junior-high School				
	Sample: Ever-married women		*Unmarried women*	
Treatment	0.210***	0.232***	0.047	0.082**
	(0.049)	(0.044)	(0.055)	(0.039)
Outcome Mean	0.44	0.44	0.77	0.79
Bandwidth	4	5	3	5
Obs	1131	1361	1103	1827

Notes: Data is from the *Household Module* of the *2008 Turkey Demographic and Health Survey*. In columns (1) and (2), the sample is restricted to women included in the Ever-Married Women Sample while in columns (3) and (4) sample includes women in the Unmarried Women Sample. Columns (1) and (3) report reduced-form RD treatment effects of being born after 1986 for an optimal bandwidth $\hat{h}$ determined by the Imbens and Kalyanaraman [40] algorithm, with a linear control function in month-year-of-birth on each side of the discontinuity. The forcing variable is annual age cohorts. Columns (2) and (4) report results from the same specification but using the optimal bandwidth for years of schooling, which is 5 years. Outcome variable is years of schooling in Panel A and a dummy variable equal to one if the respondent obtained a junior-high school degree in Panel B

Table 11: RD Treatment Effects on Covariates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	No School		Incomplete primary		Complete primary		Jr. High school		High School		University education	
(M)other/(F)ather	M	F	M	F	M	F	M	F	M	F	M	F
Treatment	-0.023	0.029	0.023	-0.043	-0.017	-0.063	0.005	0.031	0.002	0.040*	-0.003	-0.012
	(0.044)	(0.040)	(0.026)	(0.027)	(0.034)	(0.053)	(0.016)	(0.030)	(0.011)	(0.024)	(0.003)	(0.010)
Bandwidth	94	70	99	74	129	87	54	99	81	88	81	140
Obs	2359	1824	2462	1905	3101	2219	1390	2462	2088	2219	2088	3360

	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
	Region					Non-Turkish	Consang.	Rural	Childhood	Born in
	West	South	Central	North	East	language	parents	Rural	migrant	southeast
Treatment	-0.020	0.003	0.012	0.031	-0.012	0.026	0.017	-0.004	-0.027	-0.019
	(0.046)	(0.036)	(0.033)	(0.033)	(0.039)	(0.052)	(0.061)	(0.037)	(0.045)	(0.047)
Joint p-pvalue	0.754									
Bandwidth	58	79	102	51	84	82	44	71	73	78
Obs	1516	2035	2552	1325	2163	2100	1195	1849	1873	2035

Notes: Data is from the *Ever Married Module* of the *2008 Turkey Demographic and Health Survey*. The table reports reduced-form RD treatment effects of being born after September 1986, using for an optimal bandwidth $\hat{h}$ determined by the Imbens and Kalyanaraman [40] algorithm, with a linear control function in month-year-of-birth on each side of the discontinuity. The dependent variables are pre-determined covariates. In particular, the dependent variable in column 1(2) is a dummyvariable equal to one if the respondent's mother (father) had no formal schooling, the dependent variable in column 3(4) is a dummy variable equal to one if the respondent's mother (father) had incomplete primary school education, the dependent variable in column 5(6) is a dummy variable equal to one if the respondent's mother (father) had graduated from primary school, the dependent variable in column 7(8) is a dummy variable equal to one if the respondent's mother (father) had completed junior high school, the dependent variable in column 9(10) is a dummy equal to one if the respondent's mother (father) had completed high school, the dependent variable in column 11(12) is a dummy equal to one if the respondent's mother (father) had completed university degree. The dependent variables in columns 13 to 17 are dummy variables equal to one if the respondent is from western, southern, central, northern or eastern Turkey respectively. The dependent variable in column 18 is a dummy variable equal to one if the respondent's mother's main language was not Turkish. The dependent variable in column 19 is a dummy variable equal to one if the respondent's parents were related by blood. The dependent variable in column 20 is a dummy variable equal to one if the respondent was born in a rural area. The dependent variable in column 21 is a dummy variable equal to one if the respondent migrated to a different area from her birth region during childhood. The dependent variable in column 22 is a dummy variable equal to one if the respondent was born in one of the southeast provinces. The row entitled "Joint p-value" provides the p-value from a test of joint significance of the covariated in columns 1 to 22. Standard errors are clustered by month-year-cohort.

Table 12: Treatment Effects on Women's Labor Market Outcomes

	Type of employment						
	Any	Non-Agriculture	Agriculture	Self-employed	Unpaid family-labor	Regular wage-job	Daily wage-job
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome mean	0.21	0.14	0.09	0.03	0.08	0.09	0.03
Panel A: OLS							
Years of Schooling	0.010***	0.023***	-0.008***	-0.003**	-0.007***	0.028***	-0.002***
	(0.003)	(0.003)	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)
Bandwidth	75	109	63	85	76	116	103
Obs	1956	2695	1625	2178	1980	2853	2549
Panel B: Local linear RD with optimal bandwidth							
Treatment	0.035	0.019	-0.009	0.025*	-0.012	-0.001	0.013
	(0.034)	(0.021)	(0.027)	(0.014)	(0.021)	(0.013)	(0.014)
Joint p-value	0.469						
Bandwidth	75	109	63	85	76	116	103
Obs	1956	2695	1625	2178	1980	2853	2549
Panel C: Local linear RD with static bandwidth							
Treatment	0.043	0.048**	-0.005	0.031**	-0.011	0.017	0.002
	(0.034)	(0.022)	(0.026)	(0.015)	(0.020)	(0.014)	(0.014)
Joint p-value	0.338						
Bandwidth	69	69	69	69	69	69	69
Obs	1799	1801	1799	1799	1799	1799	1799

Notes: Data is from the *Ever Married Module* of the *2008 Turkey Demographic and Health Survey*. Panel A reports OLS results with years of schooling as the independent variable for an optimal bandwidth $\hat{h}$ determined by the Imbens and Kalyanaraman [40] algorithm. Panel B, using the same bandwidth, reports reduced-form RD treatment effects of being born after September 1986 with a linear control function in month-year-of-birth on each side of the discontinuity. Panel C reports results from the specification but using the optimal bandwidth from the first-stage results (where the dependent variable is years of schooling) in column 1 of Table 2. The dependent variable in column (1) is a dummy variable equal to one if the respondent reported that she is "currently working", and zero otherwise. The dependent variable in column (2) is a dummy variable equal to one if the respondent is currently employed in the industrial sector or in services. The dependent variable in column (3) is a dummy variable equal to one if the respondent is currently employed in the agricultural sector. The dependent variable in column (4) is a dummy variable equal to one if the respondent's position at her current job is 'employer' or an 'own-account' worker. The dependent variable in column (5) is a dummy variable equal to one if the respondent's position at her current job is 'unpaid family worker'. The dependent variable in column (6) is a dummy variable equal to one if the respondent's position at her current job is 'wage worker (regular)' or 'salaried, government officer (regular)'. The dependent variable in column (7) is a dummy variable equal to one if the respondent's position at her current job is 'daily wage worker (seasonal, temporary)'. All specifications control for a set of dummy variables for the type of education respondent's father/mother has completed (no schooling, completed primary, Dummy variables for whether the respondent's mother has no schooling, completed primary school, junior-high school or a higher level of school.), a dummy variable equal to one if the respondent's mother's primary language was different from Turkish, a dummy variable equal to one if the respondent lives in a rural location, a dummy for whether the respondent's parents were related by blood, month-of-birth fixed effects, and region fixed effects. The reported "Joint p-value" in Panels B and C is from a test for joint significance of treatment estimates using seemingly unrelated regressions (SUR) for columns (1) through (7). Standard errors are clustered by month-year-cohort.

Table 13: RD Treatment Effects on Spouse Characteristics

			Husband's schooling				*Type of husband's job*		
	Age of husband	Years of schooling	Jr.High	High	University	Non-agricultural	Self-employed	Regular wage-job	Daily wage-job
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Mean	28.59	8.01	0.54	0.37	0.09	0.86	0.26	0.54	0.11
				Panel A: OLS					
Schooling	-0.115***	0.452***	0.041***	0.041***	0.026***	0.012***	-0.010***	0.025***	-0.008***
	(0.036)	(0.020)	(0.003)	(0.005)	(0.003)	(0.003)	(0.004)	(0.004)	(0.002)
Bandwidth	74	122	98	61	70	63	80	78	88
Obs	1858	2944	2375	1539	1741	1570	2038	1952	2188
				Panel B: Local Linear RD, optimal bandwidth					
Treatment	0.108	0.165	-0.039	0.085*	0.033	0.049	-0.029	0.048	-0.014
	(0.281)	(0.267)	(0.041)	(0.044)	(0.025)	(0.030)	(0.037)	(0.035)	(0.027)
Joint p-value	0.137								
Bandwidth	74	122	98	61	70	63	80	78	88
Obs	1858	2944	2375	1539	1741	1570	2038	1952	2188
				Panel C: Local linear RD with static bandwidth					
Treatment	0.148	0.199	-0.020	0.058	0.033	0.041	-0.025	0.038	-0.009
	(0.283)	(0.285)	(0.042)	(0.042)	(0.025)	(0.029)	(0.040)	(0.037)	(0.028)
Joint p-value	0.420								
Bandwidth	69	69	69	69	69	69	69	69	69
Obs	1757	1790	1741	1741	1741	1757	1757	1757	1757

Notes: Data is from the *Ever Married Module* of the *Turkey Demographic and Health Survey of 2008*. Panel A reports OLS results with years of schooling as the independent variable for an optimal bandwidth $\hat{h}$ determined by the Imbens and Kalyanaraman [40] algorithm. Panel B, using the same bandwidth, reports reduced-form RD treatment effects of being born after September 1986 with a linear control function in month-year-of-birth on each side of the discontinuity. Panel C reports results from the specification but using the optimal bandwidth from the first-stage restuls in column 1 of Table 2. The dependent variable in column (1) is the completed age (in years) of the respondent's husband. All husband characteristics are reported by the female respondent in the ever-married women module of the TDHS. Variables related to respondent's husband's schooling or type of job are defined identical to respondent's type of job variables, described in footnote of Table12 above. Regressions include same set of control variables as in Table12 above. The reported "Joint p-value" in Panels B and C is from a test for joint significance of treatment estimates using seemingly unrelated regressions (SUR) for columns (1) through (9). Standard errors are clustered by month-year-cohort.

Table 14: RD Treatment Effects on Types of Household Assets Owned: Part 1

	fridge	oven	micro wave	blender	dish- washer	washing machine	iron	vacuum cleaner	AC
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: OLS									
Outcome mean	0.96	0.76	0.11	0.49	0.25	0.89	0.88	0.84	0.09
Years of Schooling	0.006***	0.018***	0.011***	0.036***	0.031***	0.011***	0.015***	0.014***	0.004***
	(0.001)	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.001)
Bandwidth	85	66	147	146	86	75	70	55	114
Obs	2162	1712	3480	3454	2198	1958	1823	1442	2787
Panel B: Local Linear RD with optimal bandwidth									
Treatment	-0.008	-0.008	0.002	-0.035	0.060**	0.052**	0.031	0.082**	0.043*
	(0.020)	(0.036)	(0.025)	(0.038)	(0.029)	(0.025)	(0.029)	(0.040)	(0.022)
Joint p-value	0.002								
Bandwidth	85	66	147	146	86	75	70	55	114
Obs	2162	1712	3480	3454	2198	1958	1823	1442	2787
Panel C: Local linear RD with static bandwidth									
Treatment	-0.011	-0.004	0.013	0.018	0.065**	0.047*	0.027	0.065*	0.042*
	(0.022)	(0.035)	(0.026)	(0.039)	(0.030)	(0.025)	(0.029)	(0.035)	(0.023)
Joint p-value	0.031								
Bandwidth	69	69	69	69	69	69	69	69	69
Obs	1800	1800	1800	1800	1800	1800	1800	1800	1799

Notes: Data is from the *Household Module* of the *Turkey Demographic and Health Survey of 2008*. Panel A reports OLS results with years of schooling as the independent variable for an optimal bandwidth $\hat{h}$ determined by the Imbens and Kalyanaraman [40] algorithm. Panel B, using the same bandwidth, reports reduced-form RD treatment effects of being born after September 1986 with a linear control function in month-year-of-birth on each side of the discontinuity. Panel C reports results from the specification but using the optimal bandwidth from the first-stage results (where the dependent variable is years of schooling) in column 1 of Table 2. The dependent variable in each column in each column is a dummy variable equal to one if the respondent's household owns the relevant asset/service. The assets/services in each column are as follows: column (1) – fridge, column (2) – gas or electric oven, column (3) – microwave oven, column (4) – blender or mixer, column (5) – dishwasher, column (6) – washing machine, column (7) – iron, column (8) – vacuum cleaner, column (9) –air-conditioner. The reported "Joint p-value" in Panels B and C is from a test for joint significance of treatment estimates using seemingly unrelated regressions (SUR) for columns (1) through (10). Standard errors clustered by month-year-cohort. The reported "Joint p-value" in Panels B and C is from a test for joint significance of treatment estimates using seemingly unrelated regressions (SUR) for columns (1) through (8). Standard errors clustered by month-year-cohort.

Table 15: RD Treatment Effects on Types of Household Assets Owned: Part 2

	cellphone	computer	internet	LCD	cable-tv	antenna	DVD	camera	car	taxi/minibus	tractor
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: OLS											
Years of Schooling	0.002***	0.021***	0.017***	0.006***	0.009***	0.006*	0.026***	0.027***	0.015***	0.001	-0.004**
	(0.001)	(0.004)	(0.003)	(0.001)	(0.002)	(0.003)	(0.003)	(0.004)	(0.003)	(0.002)	(0.002)
Bandwidth	84	48	76	122	77	105	110	75	79	62	67
Obs	2134	1267	1958	2952	1982	2613	2692	1930	2030	1585	1739
Panel B: Local Linear RD with optimal bandwidth											
Treatment	0.015	0.031	0.022	-0.012	-0.024	0.030	0.023	0.010	0.054	0.040*	-0.029
	(0.011)	(0.039)	(0.027)	(0.011)	(0.018)	(0.037)	(0.036)	(0.040)	(0.034)	(0.022)	(0.026)
Joint p-value	0.180										
Bandwidth	84	48	76	122	77	105	110	75	79	62	67
Obs	2134	1267	1958	2952	1982	2613	2692	1930	2030	1585	1739
Panel C: Local Linear RD with static bandwidth											
Treatment	0.015	0.033	0.011	-0.006	-0.027	0.023	0.025	0.011	0.055	0.044**	-0.026
	(0.012)	(0.036)	(0.028)	(0.014)	(0.019)	(0.041)	(0.039)	(0.041)	(0.035)	(0.020)	(0.025)
Joint p-value	0.086										
Bandwidth	69	69	69	69	69	69	69	69	69	69	69
Obs	1794	1801	1800	1799	1800	1800	1799	1801	1797	1795	1796

Notes: Data is from the *Household Module* of the *Turkey Demographic and Health Survey of 2008*. Panel A reports OLS results with years of schooling as the independent variable for an optimal bandwidth $\hat{h}$ determined by the Imbens and Kalyanaraman [40] algorithm. Panel B, using the same bandwidth, reports reduced-form RD treatment effects of being born after September 1986 with a linear control function in month-year-of-birth on each side of the discontinuity. Panel C reports results from the specification but using the optimal bandwidth from the first-stage results (where the dependent variable is years of schooling) in column 1 of Table 2. The dependent variable in each column in each column is a dummy variable equal to one if the respondent's household owns the relevant asset/service. The assets/services in each column are as follows: column (1) – cellphone, column (2) – computer or laptop, column (3) – internet, column (4) – plasma or LCD television, column (5) – cable-TV, column (6) – satellite antenna, column (7) – DVD-player, column (8) – video and/or photo camera, column (9) – car, column (10) – taxi/mini-bus, column (11) – tractor. The reported "Joint p-value" in Panels B and C is from a test for joint significance of treatment estimates using seemingly unrelated regressions (SUR) for columns (1) through (10). Standard errors clustered by month-year-cohort. The reported "Joint p-value" in Panels B and C is from a test for joint significance of treatment estimates using seemingly unrelated regressions (SUR) for columns (1) through (9). Standard errors clustered by month-year-cohort.

Table 16: RD Treatment Effects on Gender Attitudes

	Men should help with hh chores	Women should work if they wish to	A woman may go anywhere w/o husband's permission	Women are as smart as men	Women should be more active in politics	Women don't need to be virgins on wedding night	Women can take important decisions	Women can argue with their spouse if they disagree	Educating daughters is as important as sons
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: OLS									
Outcome mean	0.63	0.90	0.26	0.87	0.69	0.18	0.81	0.54	0.90
Years of Schooling	0.022***	0.008***	0.008**	0.019***	-0.003	0.015***	0.024***	0.021***	0.012***
	(0.003)	(0.002)	(0.003)	(0.002)	(0.004)	(0.003)	(0.002)	(0.004)	(0.002)
Bandwidth	93	141	76	80	58	95	81	62	118
Obs	2348	3341	1959	2005	1316	2321	2089	1589	2865
Panel B: Local Linear RD with optimal bandwidth									
Treatment	-0.025	0.053**	-0.013	0.029	0.033	0.052*	0.014	-0.034	-0.016
	(0.045)	(0.024)	(0.040)	(0.032)	(0.056)	(0.031)	(0.033)	(0.050)	(0.029)
Joint p-value	0.088								
Bandwidth	93	141	76	80	58	95	81	62	118
Obs	2348	3341	1959	2005	1316	2321	2089	1589	2865
Panel C: Local Linear RD with static bandwidth									
Treatment	-0.024	0.066**	-0.023	0.051	0.022	0.071**	0.025	-0.039	0.001
	(0.049)	(0.027)	(0.042)	(0.033)	(0.053)	(0.035)	(0.036)	(0.045)	(0.029)
Joint p-value	0.017								
Bandwidth	69	69	69	69	69	69	69	69	69
Obs	1790	1779	1779	1752	1584	1751	1792	1780	1796

Notes: Data is from the *Ever Married Module* of the *2008 Turkey Demographic and Health Survey*. Panel A reports OLS results with years of schooling as the independent variable for an optimal bandwidth $\hat{h}$ determined by the Imbens and Kalyanaraman [40] algorithm. Panel B, using the same bandwidth, reports reduced-form RD treatment effects of being born after September 1986 with a linear control function in month-year-of-birth on each side of the discontinuity. Panel C reports results from the specification but using the optimal bandwidth from the first-stage results (where the dependent variable is years of schooling) in column 1 of Table 2. The dependent variable in each column in each column is a dummy variable equal to one if the respondent states that she agrees with the statement stated in the relevant column. The reported "Joint p-value" in Panels B and C is from a test for joint significance of treatment estimates using seemingly unrelated regressions (SUR) for columns (1) through (7). Standard errors clustered by month-year-cohort. The reported "Joint p-value" in Panels B and C is from a test for joint significance of treatment estimates using seemingly unrelated regressions (SUR) for columns (1) through (8). Standard errors clustered by month-year-cohort.

Table 17: RD Treatment Effects on Attitudes towards Domestic Violence

	Respondent thinks physical violence towards a woman by her husband is justified if she...						
	neglects her kids	answers back her husband	refuses to have intercourse	burns the food	wastes money	doesn't cook	neglects hh chores
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: OLS							
Mean	0.147	0.127	0.049	0.023	0.147	0.045	0.106
Bandwidth	69	68	116	73	93	113	153
Years of Schooling	-0.016***	-0.015***	-0.007***	-0.004***	-0.015***	-0.009***	-0.014***
	(0.003)	(0.003)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)
Bandwidth	69	68	116	73	93	113	153
Obs	1765	1722	2829	1901	2313	2782	3628
Panel B: Local Linear RD with optimal bandwidth							
Treatment	0.002	-0.066**	-0.001	-0.003	0.040	-0.016	0.014
	(0.038)	(0.033)	(0.017)	(0.013)	(0.033)	(0.018)	(0.029)
Joint p-value	0.046						
Bandwidth	69	68	116	73	93	113	153
Obs	1765	1722	2829	1901	2313	2782	3628
Panel C: Local Linear RD with static bandwidth							
Treatment	0.004	-0.065**	-0.022	-0.006	0.009	-0.021	-0.004
	(0.038)	(0.033)	(0.018)	(0.013)	(0.035)	(0.020)	(0.031)
Joint p-value	0.183						
Bandwidth	69	69	69	69	69	69	69
Obs	1789	1779	1781	1797	1785	1795	1794

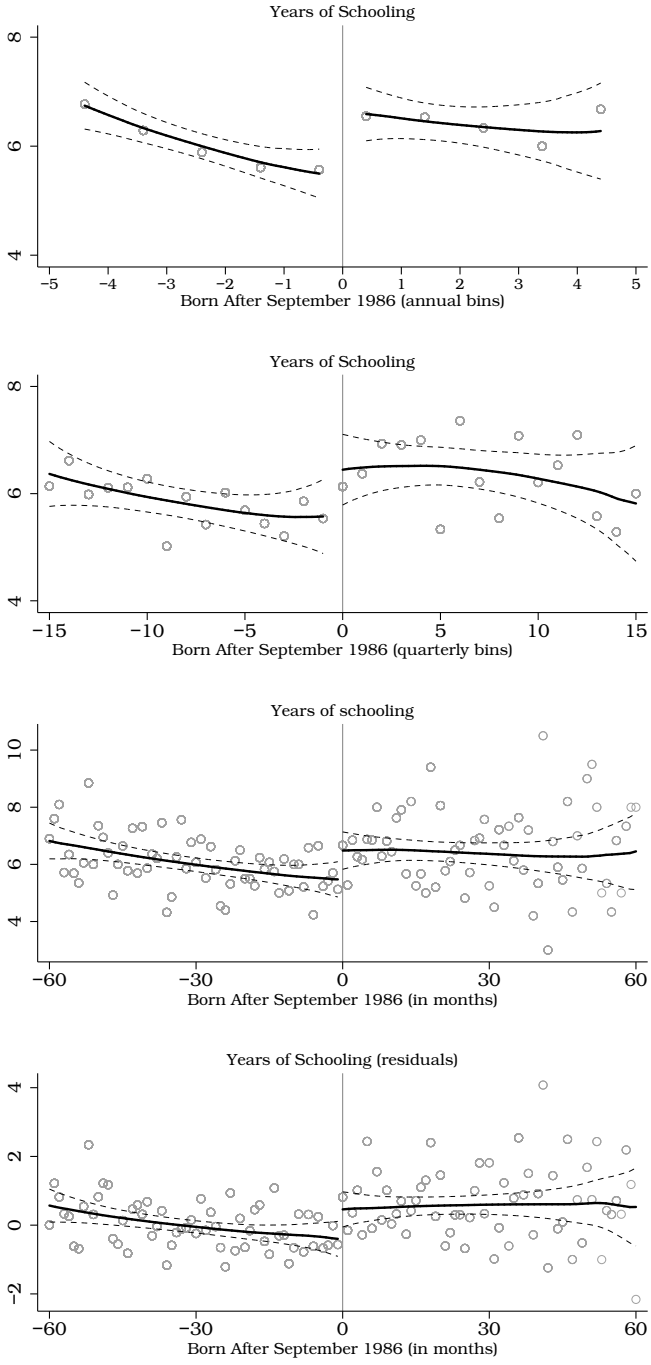
Notes: Data is from the *Ever Married Module* of the *2008 Turkey Demographic and Health Survey*. Panel A reports OLS results with years of schooling as the independent variable for an optimal bandwidth $\hat{h}$ determined by the Imbens and Kalyanaraman [40] algorithm. Panel B, using the same bandwidth, reports reduced-form RD treatment effects of being born after September 1986 with a linear control function in month-year-of-birth on each side of the discontinuity. Panel C reports results from the specification but using the optimal bandwidth from the first-stage results (where the dependent variable is years of schooling) in column 1 of Table 2. The dependent variable in each column in each column is a dummy variable equal to one if the respondent states that she thinks physical violence towards a woman by her husband is justified is she behaves in the manner stated in the relevant column. The reported "Joint p-value" in Panels B and C is from a test for joint significance of treatment estimates using seemingly unrelated regressions (SUR) for columns (1) through (7). Standard errors clustered by month-year-cohort.

Table 18: RD Treatment Effects by whether Respondent Spent Childhood in Rural/Urban location

	Years of Schooling	Jr. High School	Vocational School	Religiosity index	Marriage Decision
	(1)	(2)	(3)	(4)	(5)
			Panel A: All		
Mean	6.34	0.38	0.06	0.43	0.54
Treatment	1.017***	0.213***	0.007	-0.052**	0.137***
	(0.213)	(0.036)	(0.022)	(0.021)	(0.046)
Obs	1801	1801	1801	1798	1798
			Panel B: Rural		
Mean	4.82	0.21	0.02	0.48	0.46
Treatment	0.966***	0.196***	0.009	-0.072**	0.094
	(0.305)	(0.051)	(0.018)	(0.031)	(0.060)
Obs	833	833	833	831	831
			Panel C: Urban		
Mean	7.64	0.52	0.09	0.39	0.62
Treatment	0.899***	0.189***	-0.010	-0.006	0.166***
	(0.325)	(0.047)	(0.036)	(0.027)	(0.061)
Obs	968	968	968	967	967
			Panel D: Tests of Equality between panel B and C		
p-value	0.89	0.91	0.64	0.09	0.38

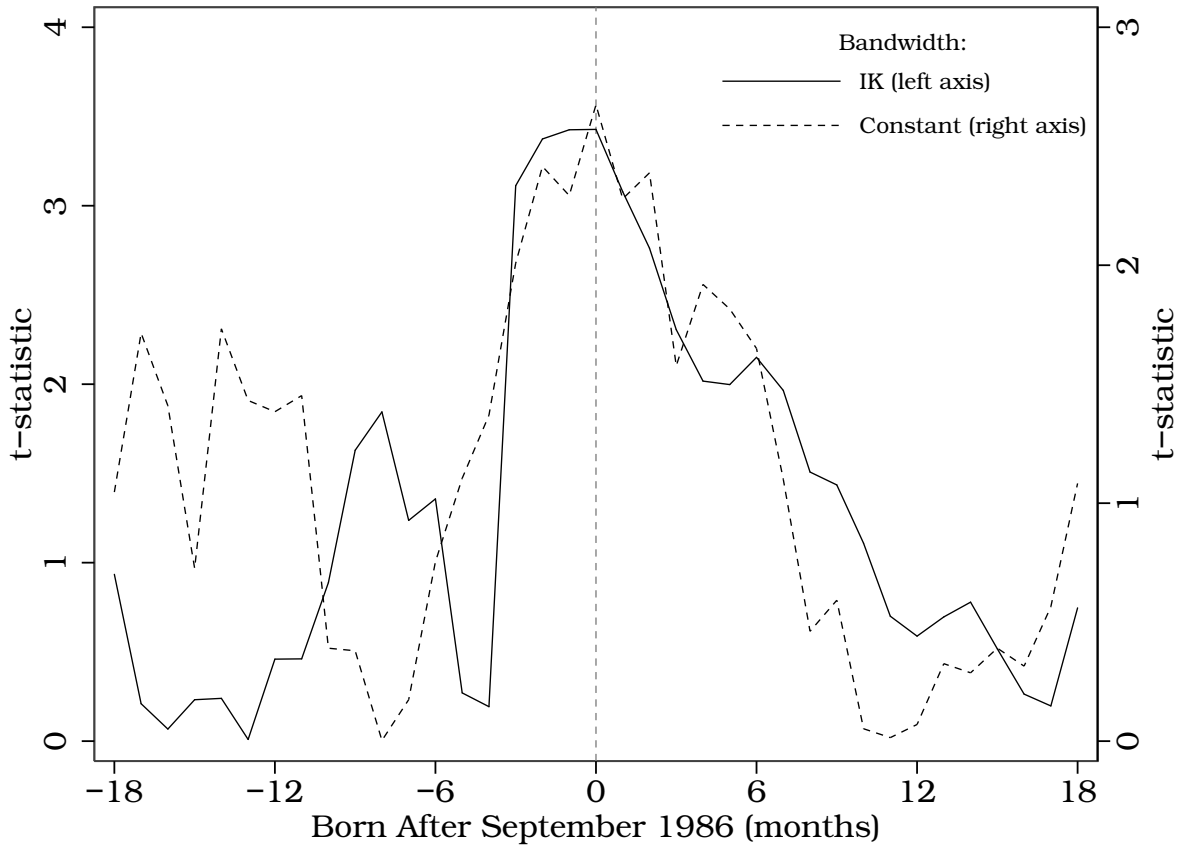
Notes: Data is from the *Ever Married Module* of the *2008 Turkey Demographic and Health Survey*. Panel A reports reduced-form RD treatment effects of being born after September 1986 with a linear control function in month-year-of-birth on each side of the discontinuity. The bandwidth is 69 months in all regressions, which is the optimal bandwidth $\hat{h}$ determined by the Imbens and Kalyanaraman [40] algorithm when years of schooling is the independent variable. Panel B reports results using the same bandwidth, for the subsample of respondents who spend their time below 12 years of age in a rural location, while Panel C does the same for respondents who spend their childhood in an urban location. Panel D reports the p-value for the test of equality between the RD treatment effects in Panels B and C by column. The dependent variable in column (1) is years of schooling; (2) a minimum of 8 years of schooling; (3) having attended a vocational school; (4) a weighted average of five indicator variables on religiosity (wearing a headscarf, attended Qur'an course, regular prayer, ever praying, and regularly fasting) where the weights are defined as $w_i = (1-\mu_i)/\sum_J (1-\mu_j)$ where μ_i is the mean of religiosity variable i; (5) a dummy variable equal to one if the respondent had a say in the choice of husband and zero otherwise. All specifications control for a set of dummy variables for the type of education respondent's father/mother has completed (no schooling, completed primary, junior-high or a higher level of school), a dummy variable equal to one if the respondent's mother's primary language was different from Turkish, a dummy variable equal to one if the respondent lives in a rural location, a dummy for whether the respondent's parents were related by blood, month-of-birth fixed effects, and region fixed effects. Standard errors are clustered by month-year-cohort.

Figure 6: RD graphs for years of education by bin size



Notes: The sample includes observations from the 2008 Turkey DHS ever married women module. The forcing variable is the distance in years away from turning 21 in September. The vertical line in each graph represents the cut-off birth date (September 1986) as defined by the compulsory schooling reform – any individual born before this date is allowed to drop out after 5 years in school, while anyone born after September 1986 is required to complete 8 years in school. Mean levels of years of education at the annual (uppermost graph), quarterly (upper middle graph), and monthly (lower middle graph) birth cohort level are plotted as dots. The (bottom) graph represents years of education conditional on covariates (explained in the text) in monthly birth cohorts. The black lines represent local linear smoothers fitted to observations on each side of the threshold. 95 percent confidence intervals are plotted in light gray lines around the mean level.

FIGURE 7: ALTERNATIVE DISCONTINUITIES



Notes: The figure plots the ratio of the RD estimate and its standard error for possible discontinuities over a range of 18 months before and after September 1986. Each regression includes linear control function in month-year-of-birth on each side of the possible discontinuity, has an optimal IK bandwidth (solid line) or a constant bandwidth of 30 months (dashed line), and has the following controls: a set of dummy variables for the type of education respondent's father/mother has completed (no schooling, completed primary, junior-high or a higher level of school), a dummy variable equal to one if the respondent's mother's primary language was different from Turkish, a dummy variable equal to one if the respondent lives in a rural location, a dummy for whether the respondent's parents were related by blood, month-of-birth fixed effects, and region fixed effects.