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

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Abstract

We exploit a change in compulsory schooling laws in Turkey to estimate the causal effects of education on religiosity and women's socio-economic status. A new law, implemented in 1998 bound individuals born after a specific date to 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. Using the 2008 Turkish Demographic Health Survey, we show that the reform resulted in a one-year increase in years of schooling among women on average, although it did not increase schooling among men. Over a period of ten years, this education increase resulted in women having lower religiosity, greater decision rights over marriage and fertility, and higher household wealth. We find that a muted average RD effect on labor force participation shrouds heterogenous effects depending on socioeconomic background; women from more socially conservative backgrounds tend to observe no increase in labor force participation whereas women from less conservative backgrounds experience a large increase. Education thus empowers women across a wide spectrum of a Muslim society, yet faces limits in allowing women in the conservative communities from realizing their full potential through the labor market.

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"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 causal effects on women's empowerment in Muslim societies where women's participation in the labor market is limited and they often don't have control over their earnings or their own bodies (Doepke et al [18]). In fact, even though female education has been successfully expanding in many majority-Muslim countries, number of legal rights enjoyed by women are few relative to men and female labor force participation remains low (UNDP [59]). The lack of a corresponding labor force participation effect raises concerns over the efficacy of expanding education as a means of improving women's rights in Muslim societies[2]. On the other hand, education has been shown to have many important non-pecuniary effects outside the labor market (Oreopolous and Salvanes [50]) and to the extent that these effects help empower women, they may constitute alternative mechanisms through which education may lead to women's empowerment (even in the absence of large labor market returns). However, most of this research comes from countries and societies that are not majority-Muslim and where women do work to a larger degree. As such, disentangling non-pecuniary returns to education from its labor market (and thus pecuniary) returns is particularly challenging in most settings and whether education may empower women in the Muslim world remains an open question.

Even though scholars debate the fundamental causes for the severe degrees of gender inequality in Muslim societies, most posit a nexus of patriarchal culture, strong religious values, and restricting social norms as proximate explanatory factors. Historically, Lewis [40] claims women's status was "probably the most profound single difference" between Muslim and Christian civilizations. In more contemporary cross-country studies, Fish [22] documents a negative cross-country correlation between having an "Islamic religious tradition" and female empowerment, while Barro and McCleary [4] 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 [21] find evidence in line with significantly stricter constraints to female labor force

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

[2] This mechanism has been emphasized as conducive to both development and gender inequality (Goldin [25], Doepke and Tertilt [17], Duflo [19], World bank [64]), as well as the severe degrees of gender inequality in this region World Economics Forum [62]

participation among Muslim women. To the extent that barriers to entry due to religious values restrain women's rights, an integral outcome of empowerment is therefore a woman's ability to independently assert her own beliefs.

We exploit an extension of compulsory schooling in Turkey to estimate the causal effect of schooling on female empowerment. Compulsory schooling laws have been extensively used to estimate returns to education, in Western countries, on labor market outcomes (Angrist and Krueger [3], Oreopoulos [49]), health and fertility (McCrary and Royer [46], Lleras-Muney [41], Black et al [6]) among others. We follow a similar strategy to provide meaningful causal parameters for the effect of a year of schooling on outcomes related to social status of women in Turkey, a majority-Muslim country.

In 1997, Turkey's parliament passed a new law to increase compulsory schooling from 5 to 8 years. By this law, individuals born on or after September 1986 were bound to complete 8 years of schooling, whereas those born earlier could drop out after 5 years. Using the sample of ever-married women from the 2008 Turkish Demographic Health Survey (TDHS) we are able to observe outcomes 10 years after the law-change was implemented.

Analysis of the sample of ever-married women focuses the RD treatment effects on a subset of the population that tends to be demonstratively poorer and more socially conservative, i.e. the very subpopulation that the reform was aimed at. In a comparison of ever- and never-married women, the reform only affected education among the former, and as a result, the exclusion of non-married women effectively means exclusion of non-compliers with the reform. We also show that the probability of selection into the married sample is not affected by the law.

We adopt a regression discontinuity (RD) design assigning treatment based on whether an individual's month-and-year of birth was before or after the September 1986 threshold. As such, our identification strategy entails comparing cohorts born one month apart and relies on the assumption that these two groups should exhibit no systematic differences other than being subject to different compulsory schooling laws.

First, we show the effect of the reform on women's years of schooling. As a result of the reform, women's average years of schooling increased by one year, and completion rates for junior-high (secondary) and high school completion increased by 24 and 8 percentage points (ppt) respectively. There is no significant impact of the reform on men's schooling on average (mainly because average man's schooling in Turkey around the age threshold was already at a relatively high level). Thus, the reform effectively served to reduce the education gender gap by half.

Second, our RD estimates reveal that this additional year of schooling had significant secularizing effects. Ten years after the reform was implemented, and relative to sample means, women were 10 percent (8 ppt) less likely to wear a headscarf, 22 percent (10 ppt) less likely to have attended a Qur'anic study center 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 of schooling on the timing of either marriage or birth, nor on the number of children. We do however find significant effects on women's decision rights with regards to both marriage and fertility decisions; a reform-induced year of schooling results in a 10ppt (20 percent relative to the sample mean) increase in the likelihood of having a say in the marriage decision, and a 10ppt (12 percent) increase in the likelihood of having a say in the type of contraceptive method

adopted. 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.

Fourth, we document less pronounced and largely imprecise impacts on women's labor market outcomes. Although our estimates indicate positive effects on non-agricultural employment in general, and self-employment in particular, these estimates are sensitive to the specification used. At the same time, we show significant positive effects of schooling on household wealth, largely driven by appliances related to women's role as housewives. We are unable to explain this by observable increases in spousal quality.

Altogether, our results indicate significant empowering effects of education, but whereas we document precise effects on decision rights, household wealth, and measures of social and religious conservatism, we fail to find equally concise effects on spousal and labor force outcomes. This prevents an interpretation relying exclusively on either labor market or assortative matching in the marriage market as the main channel of empowerment. In fact, an examination of heterogenous effects reveal diverging effects depending on how socially conservative women's backgrounds are; in rural areas, education predominantly allows increased freedom to be more secular, greater decision rights over marriage, and less traditional marriages. In urban areas, education has similar effects, but also lead to increased labor force participation. We interpret this as increased education, and its associated bargaining power in the household, leading to different allocations depending on the preexisting level of women's rights. Education may thus have only a partial effect on employment, as religious or cultural barriers to entry prevent women from realizing larger gains of education through the labor market.

With a few exceptions, research on the consequences of female education in Muslim societies remains limited. Lavy and Zablotsky [38] who 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 markets but had no impact on female labor force participation. They use the termination of the military rule that limited the mobility of the Arab population in Israel as an instrument for schooling costs. Breierova and Duflo [10] exploit a school construction program in Indonesia to estimate the effect of education on age at marriage, fertility and child mortaliy. 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 [51] estimate the effects of the universal (free) primary education program in Nigeria on fertility. All of these studies adopt difference-in-difference techniques and find that education delays marriage and reduces fertility among women exposed to these programs relative to those that were not. We contribute to this literature by adopting a regression discontinuity design that compares women in different monthly birth cohorts in Turkey that were subject to different compulsory schooling laws and we analyze the effects of education on a wide range of outcomes related to women's empowerment. We show that despite having no significant impact on age at first marriage or fertility, higher education enabled women in exposed cohorts to have to greater decision-making power and to live in wealthier households. Moreover, we provide evidence on the mechanisms through which these effects were consumated [3].

[3]Two related paper to ours are Dayıoğlu, Kırdar, and Koç. [35] who uses the same legal change as we do and examines the consequences for teen marriages in Turkey; and Cesur and Mocan [14] who use instrumental variable strategies to look at the effects of education on political outcomes as well as several measures of religiosity in Turkey. Our study differs from these papers methodologically by our implementation of a standard regression discontinuity framework, as well as topically in a broader examination of empowerment outcomes.

With regards to education's impact on secularism, modernization theorists have long argued for a secularizing effect (Stark [56], Swatos and Christiano [57]), 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 [59]). At the same time, an ongoing process commonly referred to as the 'Islamic revival' (Lapidus [37]) 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. Previous empirical research has found mixed results on the relationship between education and religion. For example, investigating time series usually results in negative correlations (Hout and Fisher [30]). While cross-country evidence suggest a positive correlation (Barro and McCleary [4]), others have documented negative ones (Iannacone [32] and Deaton [16]). 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 [55]). Recent work by Hungerman [31] shows negative effects of education on religious affiliation in Canada. Yet, to this date no study has provided a causal identification of the effect of education on broader measures of religious expressions in a Muslim country[4].

A growing literature also reveals empowering effects of education in general. For example, Basu and King [5] find that education tends to have positive effects on Bangladeshi women's participation in attending political meetings. A recent paper by Friedman *et al* [23] examines the educational effects on girls' attitudes related to social issues and finds that schooling has limited impact on girls' pro-democratic or secular attitudes in Kenya. This also links with research on broader non-pecuniary effects of education, recently summarized by Oreopolous and Salvanes [50] and Lochner [42].

Our paper adds to the research literature by providing meaningful causal parameters for the effect of a year of schooling on both social and religious outcomes for women in a majority-Muslim country. 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 (as captured by higher decision-making power and household wealth) but are unable to meaningfully break down the barriers that women face in entering the labor market, particularly in more conservative rural communities. While still having important empowerment consequences for women's empowerment in Muslim societies, education may not be 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.

[4]The education reform studied here was heavily influenced by Turkey's secular elite, and contemporary documents indicate that the education reform may have been an attempt to stem increases in Islamic preferences. This resonates with the view that public education may serve as an instrument for affecting a population's beliefs and values (Pritchett [53], Kremer and Sarychev [36]).

2 The Political Economy of Education in Turkey

> *"He is a weak ruler who needs religion to uphold his government; it is as if he would catch his people in a trap. My people are going to learn ... the dictates of truth and the teachings of science. Superstition must go. Let them worship as they will, every man can follow his own conscience provided it does not interfere with sane reason or bid him act against the liberty of his fellow men.' – Mustafa Kemal Atatürk*[5]

In Turkey, instilling certain values in the 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."[6]

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 [40]). 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 the husband's discretion (Mango [43]). 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.[7] 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 [29]). 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 may potentially play an important role in facilitating social mobility and improved living conditions. In fact, female education is correlated with higher labor force participation, as well as lower levels of religious and social conservatism in Turkey. Participation in voluntary education, however, is often a challenge for many women. Turkey's strictly secular national education system often contrasts with a 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.

[5] Quoted in Atatürk: The Biography of the founder of Modern Turkey, by Andrew Mango; "In a book published in 1928, Grace Ellison quotes [Atatürk], presumably in 1926-27", Grace Ellison Turkey Today (London: Hutchinson, 1928)

[6] "National Education at the Beginning of 2001," Republic of Turkey Ministry of Education http://www.meb.gov.tr/stats/apk2001ing/section_4/compulsoryeducation1.htm

[7] For more on women's participation in Turkey as well as comparisons with other countries, see The Gender Gap, World Economic Forum, http://www.weforum.org/en/initiatives/gcp/Genderpercent20Gap/index.htm

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 mandatory 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 (Çakir *et al* [13]).

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 [65]). 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 more extracurricular Qur'anic study centers (Günay [26]). 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 mandatory secular schooling from 5 to 8 years, and it made quite clear that at stake was more than test scores:

> "*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.*"[8]

Less than five months later, Law No. 4306 passed the Turkish parliament by a vote of 277 to 242. This new law stipulated an extension of mandatory 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

[8] *"Genç nesillerin körpe dimağlarının öncelikle cumhuriyet, Atatürk, vatan ve millet sevgisi, Türk milletini çağdaş uygarık düzeyine çıkarma ülkü ve amacı doğrultusunda bilinçlendirilmesi ve çeşitli mihraklarin etkisinden korunmas bakımından... 8 yıllık kesintisiz eğitim, tüm yurtta uygulamaya konulmalı."* "National Security Council Resolution No. 406", February 28, 1997 Appendix A, available at http://bit.ly/AdtYFl

of the eighth grade.[9] A year later, the law went into effect.

The new law required a massive investment in education. According to a World Bank [60] 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 (Dülger [20]). 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 from the 2008 Turkey Demographic Health Survey (TDHS)[10]. Within this household survey, 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 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. This module of the TDHS includes a number of variables related to health, economic activity, and social status. Specifically for our purposes, this sample also includes variables measuring religious expression such as wearing a headscarf, having attended a Qur'anic course, as well as prayer and fasting. As such, our analysis will be concentrated on the nationally representative sample of ever married women from the TDHS.

Table 1 provides summary statistics on key characteristics of women in the ever married women sample. Since our identification focuses on young women in their early twenties, we report summary statistics for those between 16 and 26 years of age. As seen in Panel A, the average woman in this age bracket had 6.3 years of schooling. Roughly 38 percent had graduated from junior high school while 21 percent of them had completed high school. Around 6 percent had attended a vocational school as their last school type.

Panel B of Table 1 provides descriptive statistics for the measures of religiosity we have in the data. 77 percent of the women in our sample reported wearing a headscarf when they leave the house. The

[9] A further component of the Law raised the minimum grade requirements of attending Qur'anic instruction centers but these were subsequently overturned two years later.

[10] We will also use the 2003 wave of the TDHS for some robustness checks.

prevalence of this custom is consistent with other recent surveys from Turkey[11]. 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 reported 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'anic study and regular prayer, and less weight on praying occasionally and fasting, which are more common[12]. The mean level of the religiosity index is 0.43 and it entails a substantial variation (its standard deviation is 0.24).

Panel C of Table 1 provides descriptive statistics on marriage and fertility-related outcomes. The average age at first marriage among the women in our sample is just under 18.8 years and the corresponding average age at first birth is 19.8 [13]. The average respondent in this age group had 1.2 children in 2008. The next two variables in Panel C are related to the decision-making power of the respondent. The first one is related to how the respondent was matched with her marital partner. In Turkey, arranged marriages are quite common. The specific form in which these arrangements take place can 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 (either via elopment or not) – as opposed to their family making that decision[14]. The second type of outcome variable we have on the decision-making power of the respondent is related to the type of contraceptive method used. This question was directed to respondents who reported that they were using a contraceptive method at the time of the survey. 58% of the respondents born 5 years around the age cutoff (September 1986) reported that they were using contraception at the time of the survey. Among this subsample of women who reported using contraception, 86 percent reported that they decided on the method of contraception themselves or jointly with their husband – the excluded group being those who reported that their partner decided on the method alone[15]. The final row of Panel C is related to the institution of brideprice which, although has been losing its prominence in modern-day Turkish society, is still in use in rural parts of the country[16]. On average, around 19 percent of the women in our sample (born 5

[11] See for example Çarkoğlu and Toprak [12].

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

[13] These low ages of marriage and birth are not specific to the young cohorts in the ever married sample. For ever married women of *all* ages, the corresponding averages are 19.9 for first marriage and 21.1 for first birth respectively.

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

[15] A relevant question upon using this variable as an outcome variable is whether the selection into the subsample of women who reported using any contraception was affected by the policy. We report in section 4 that we do not find any significant effect of the reform on selection into this subsample.

[16] Using data from previous waves of the TDHS, Anderson [2] 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

years around the age cutoff) reported that their family received a brideprice (*başlık*) upon their wedding.

Panel D of Table 1 provides summary statistics on the labor market outcomes of the respondents as well as the relevant characteristics of their spouses and their households. The labor force participation among married women aged 16-26 in our sample was 19 percent and as low as 10 percent when one excludes the agricultural sector. On the other hand, labor force participation rate among the spouses of the women in our sample was much higher, at 93 percent[17]. The spouses of sampled women had on average 1.7 more years of schooling and were just over 5 years older than their wives. The last row in panel D shows an index based on the first principal component across the ownership of 20 different items, which cover all the relevant household assets included in the TDHS module on household wealth[18].

Finally, Panel E of Table 1 provides summary statistics on pre-determined 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. Furthermore, 5 percent of the respondents had mothers who completed junior high school, and 19 percent had fathers who completed junior high school. 26 percent of the respondents had consanguineous parents and 28 percent were from rural areas.

3.2 Identification

An important component of Law No. 4306 was that it decreed that children born on or after September 1986 were bound by the new law, whereas 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, i.e. those born just after, or just 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.

Previous research (McCrary and Royer [46], Oreopolous [49]) using RD designs have often used the birth-date-related discontinuity as an instrument for schooling. As will be clear in Section 4, in

we use, among the entire sample of married women the rates are 29 percent in rural and 14 percent in urban areas.

[17]The striking difference in the labor force participation rates of the respondents and their spouses is further augmented if we look at the types of occupations that they are engaged in. The majority of the women in our sample were employed as unpaid family workers (8 percent of all women in our sample); only 6 percent of them had regular wage-contracts and 3 percent were self-employed in their own business, while the rest (2 percent) worked in informal, low-skill daily wage-labor jobs. On the other hand, 55 percent of the respondents' spouses had regular wage-jobs, 25 percent were self-employed and 11 percent were in daily wage-jobs. Very few (about 1 percent) of the spouses were working as unpaid family workers. This striking difference on labor market outcomes (both in terms of participation and occupational choices) of men vs women has been documented in previous studies (World Bank [61], Özcan *et al* [52]).

[18]The wealth index is 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. 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 the asset using all the assets included in the TDHS separately does not change our findings significantly.

our design the first-stage estimates a discontinuous jump of exactly one year of schooling, providing meaningful reduced-form regressions and making standard two-stage least squares somewhat redundant. We will use a basic RD specification of the form:

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

where y_i is the outcome in question, t_i is the treatment, x_i is the forcing variable, and h is a neighborhood around c, hereby referred to as the bandwidth. The control function $f(x_i)$ 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.* [27], Imbens and Lemieux [34]). In order to determine the correct bandwidth we use the optimal bandwidth routine from Imbens and Kalyanaraman [33]. This allows specifying an optimal bandwidth for each outcome under consideration. As an alternative we also include specifications using the optimal bandwidth from the first-stage (years of schooling) for all outcomes (69 months around the cut-off). In the following text, the former bandwidth type will be referred to as the "optimal" bandwidth, and the second as the "static" bandwidth. Following Card and Lee [11] 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 include a basic set of control variables. In particular, 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 living in 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-of-birth 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. First of all, 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 annually binned averages of pre-determined characteristics against the forcing variable. For ease of demonstration, for parent's education we use dummies denoting whether the respondent's mother and father have completed primary school and for birth region we show a graph for whether the respondent was born in the Eastern or Southeastern region of Turkey[19].

Panels A and B of Figure 1 demonstrate two main points: first, one can easily see the severe disparity in educational attainment between mothers' and fathers' educational attainment – approximately 40

[19]For the analysis we will control for parent's education and birth province using a large number of dummy variables, this simplification makes it easier illustrate the balancing of pre-determined characteristics in Figure 1. Similar graphs for the other regions are excluded for the sake of brevity but show no clear evidence of a jump at the discontinuity either.

percent of the respondents reported that their mother had completed primary school or above, while the corresponding figure was 75 percent for their fathers. Second, the relationship between respondents' parents' education and their birth cohort is smooth across the threshold. The rest of the graphs in Figure 1 show that all of the pre-determined covariates appear similarly balanced around the threshold. We also test for evidence of jumps using a more parsimonious test of all variables that we use as controls in the later regressions. For this test we follow Lee [39]. Of a total of 32 control variables – including dummies for parental education, dummies for region-of-birth, dummies for month-of-birth, a dummy for whether their mother had a non-Turkish language as mother tongue, and whether the respondent's parents were consanguineous – none of these are individually 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 0.66.

A second source of concern related to the assumption of exogeneity around the age threshold can be manipulation of the age of the respondent by her parents. In the 1980s, parents in Turkey could often delay registering their children until the age of three. 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 random placement around the threshold may be violated. We do not expect parents to be able to affect the birth dates of their 11-12 year old children after the announcement of the law change. Any seasonality in the birth month cohorts should be picked up by our month-specific fixed effects, although their inclusion has no bearing on the estimates. Furthermore, we run a McCrary [45] density test on the density of the forcing variable yielding an insignificant estimate, as can be seen in Figure 4.

As we will be using the ever married women's module from the 2008 Turkey DHS, another concern is to what extent the treatment may have had an effect on being ever married in 2008 and thus on the inclusion into the main survey modules. In order to test for this selection effect, 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. More specifically, this module contains information on the educational status and the year of birth of the individual household members. Since household survey data only includes age and not 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 slightly 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. We summarize the results on the tests of selection into the married women sample in Figure 2, and we report the corresponding regression estimates in Table 8 in Appendix B.

The upper left graph in Figure 2 shows completion of junior high school for *all* women in the sampled households, in annual age averages 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.

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 annual average female-to-male completion rates in junior high school against the age of the individual. To the left-hand-side of the threshold, the ratio of female-to-male junior high school completion rate fluctuates around 0.6-0.7. On the other hand, to the right-hand-side of the threshold the ratio is always higher than 0.75, and higher than 0.80 for most cohorts. The jump at the threshold is around 0.16, a relative increase of a third compared to the pre-reform ratio. To put the magnitude in broader context, over the last twenty years Turkey has seen the female-to-male completion ratio in junior high school rise by 0.3 points from 0.23 for women in their forties to 0.52 for the 22-year-old women in the control group just below the discontinuity. The magnitude of the jump in Figure 2 implies that the reform at hand accomplished half of this magnitude in just a few years.

Finally, the lower right graph in Figure 2 further investigates whether there was a similar jump at the discontinuity for the likelihood that the respondent was ever married (and thus included in the sub-sample of women that we will use in the following analysis). As this graph shows, there is no commensurate jump in marital status and consequently, 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.

One consequence of our focusing on ever-married women is that the estimated RD treatment effects may be uninformative for never-married women. If these never-married women are of significant interest in evaluating the 1997 reform, then our results would be of limited value. In fact, these two groups differ in marked ways. In order to illustrate these differences we tabulate the means by marriage status for a number of variables related to parental backgrounds in Table 13 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 never-married 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 never-married women aged 16 to 25 in the sample as most never-married women in the sample still live in the same households as their mother (86 %), father (79 %) or both (77 %).

The findings in Table 13 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 law among the ever-married women's sample. If there are plenty of compliers among the non-married women sample, our results on outcomes such as religiosity and empowerment outcomes 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 14 separately for ever- and never-married women respectively.

This table clears shows the reform's impact on education as nearly non-existent for the never-married women in terms of years of schooling, and much smaller in terms of completion of junior high school. Although our main analysis will exclude the never-married women's sample in later sections, doing so has little 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. It is therefore not surprising the absence of a significant first-stage relationship among a group of women who would likely finish junior high school regardless of the reform, as their parental characteristics indicate. Focusing on the ever-married women thus allows the study of a group of women closer to the policy-relevant subpopulation.

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 schooling. The upper two graphs in Figure 3 provide a graphical illustration of the RD design with regards to years of schooling. The upper left graph plots (using observations from the 2008 TDHS) the average years of schooling in annual bins against birth-month where the cutoff is September 1986. Those born before this date were exempt from the law and subject to a minimum of five years of schooling, while those born after this date were bound by the law to a minimum of eight years of schooling. Overlaid is a local linear smoother on each side of the discontinuity. The graph demonstrates that there was a significant jump at the age threshold of approximately one additional year of schooling. In order to make sure that this was the effect of the reform and not due to some underlying age trend that causes women's' schooling in Turkey to differ at this specific age threshold, we 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 in fact due to the reform and not some underlying relationship between age and women's schooling in Turkey.

In order to establish these relationships more thoroughly and to test for their statistical significance, 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 [33] optimal bandwidth of 69 months around the threshold. This yields an estimated impact of 1.018 additional year of schooling, which is significant at 99% confidence level. We test the robustness of our estimates by allowing the

control function to vary in panels A, B, and C using linear, quadratic, and cubic control functions respectively. The estimate changes trivially to 1.004 when we use a quadratic or cubic control function. This corresponds to roughly 16 percent increase relative to the mean level of schooling (reported in the first row of Table 2). Following Imbens and Lemieux [34], in order to further test the robustness of our estimates, in columns 2, 3, and 4 of Table 2 we run the corresponding RD regressions with half, a third of, and twice the optimal bandwidth. Only in the largest bandwidth with a linear control function does the point estimate deviate somewhat from the one-year increase to 0.573 (likely the result of an artificially large bandwidth), but it is still significantly different from zero. To sum up, the results imply that irrespective of the control function and the bandwidth used, the reform led to an increase of one year of schooling. This finding has the consequence that while using the reform to estimate the impact of an additional year of schooling on other outcomes, the reduced-form and the IV estimates will be largely identical. Therefore, we will focus on the reduced-form regressions using the local linear method. Using standard IV specifications do not yield any notable changes in our findings.

Columns 5-8 of Table 2 report estimates for the impact of the reform on various types of educational attainment. In column 5, the dependent variable is a dummy variable equal to one if the respondent completed junior high school or above. Using the local linear control function, the RD estimate for the effect of the reform on the likelihood that the respondent completed junior high school or above is 24 percentage points (ppt) and significant. However, the estimates are not very robust – the point estimate diminishes to 0.12 with a quadratic and to an imprecisely estimated 0.07 with a cubic control function – but it remains positive. Column 7 shows that the reform led to an increase of 6 to 14 ppt in the probability that women in Turkey graduate from primary school. One explanation for the less robust results in column 6 and the strong results in column 7 is misreporting of whether individuals completed primary school or junior high school. As these two types have very similar names – "*İlkokul*" or "primary *school*" (completing 5 years before 1997) and "*İlköğretim*" or "primary *education*" (completing 8 years after 1997) – it is not unlikely that respondents may confound the two. This is another reason why we focus on years of schooling for our analysis as opposed to the main type of schooling completed.

Column 6 shows that the reform had a positive, significant and robust impact on the probability that women in Turkey graduate from high school (which corresponds to 12 grades of completed schooling). The local linear estimates in panel A show a statistically significant impact of 8 ppt, and the corresponding estimates are 9 ppt and 11 ppt using the quadratic and cubic control functions, both significant. This implies that the reform's 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 schooling respondents attained but we know if the last school they attended was a vocational schooling, which includes religious (junior high or high) school. 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. A large negative impact on attending vocational schooling would be consistent with a treatment effect being driven by a switch from religious to secular schooling. Yet the results in column 8 show that the reform has a comparatively small and insignificant effect on the probability of having last attended a vocational (including religious) school. 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 *high school*. We therefore interpret the effects of the reform as mainly working through an increase in years of schooling.

4.2 Religion

> *"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*[20]

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 birth-year means with a local linear smoother on each side of the threshold. 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 similarly aged individuals from the earlier survey wave. The 2008 figure shows that although the propensity to wear a headscarf appears to be increasing in birth year, we observe a negative jump at the threshold. The corresponding graph from 2003 shows a similar upward age trend in the propensity to wear a headscarf but no jump at the threshold. The two figures therefore indicate that women born just after the threshold under an eight-year compulsory schooling were observably less likely to wear a headscarf than those born just before the same threshold under five-year compulsory schooling, and this was not due to an underlying age trend.

In order to test this formally in a regression framework, we estimate the RD treatment effect on multiple measures of religious expression and practices we have in the data. Table 3 provides the results of estimating specification 1 on these outcomes. As a point of reference, in Panel A, we report basic ordinary least squares (OLS) regressions of our religiosity measures on years of schooling. For the religiosity index in column 1, an additional year of schooling was correlated with a 1.1 percentage point lower value of the religiosity index and a 3.5 percentage points 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 per 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 specifications with optimal bandwidths for each specific outcome. The results imply that one year of schooling led to a 5.8 percentage

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

points lower religiosity index (13 percent relative to the sample mean), a 7.6 percentage points (10 percent) lower likelihood of wearing a headscarf, a 9.6 percentage points (22 percent) decrease in having attended a Qur'an course, and a 7.3 percentage points (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. Taken together, our results imply that the increase in mandatory schooling led to a significant decrease along multiple expressions of religiosity. For the last two outcomes in Table 3, ever praying and fasting, the estimates are close to, and significantly indistinguishable from zero. The RD estimates using the bandwidth from the first-stage results in column 1 of Table 2 are very similar to those estimated in Panel B.

In explaining why we find results for the first three but less so for the last two religion outcomes 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'anic 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 more pure secularizing story.

Furthermore, these results do not necessarily imply that women independently *chose* their level of religious expression. One possibility is that schooling changed the type of husband the women eventually married, and that the differences in religious expressions simply represent different commandments from 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, we in the next sections focus more on empowerment-related outcomes.

4.3 Marriage Characteristics

"Everything we see in the world is the creative work of women." – Mustafa Kemal Atatürk[21]

A commonly noted way in which schooling may affect women's outcomes is through fertility (Becker [7], Mincer [47]). Our design emphasizes comparing women in their early 20s, but due to the low age of first

[21] As quoted in The Macmillan Dictionary of Political Quotations (1993) by Lewis D. Eigen and Jonathan Paul Siegel, p. 424; also in Atatürk: First President and Founder of the Turkish Republic (2002) by Yuksel Atillasoy, p. 15

marriage in Turkey (around 19 years in the sample) analyzing age of first marriage for our relatively young sample is still relevant. Panel A of Table 4 shows that years of schooling is undoubtedly positively correlated with age at first marriage and birth (columns 1 and 2) and educated women tend to have fewer children (column 3). 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. Overall, the RD results imply that the additional year of schooling caused by the education reform had small and insignificant effects on fertility.

As we do not find a notable impact of education on timing of marriage and fertility decisions, one question is to what extent education improved actual decision rights of women in Turkey and the degree to which their environment limited their life choices. Fortunately, the ever-married women module in the 2008 DHS includes several measures related directly to the respondents' decision rights, specifically with regards to whether women had a say in the choice of their husband and whether women had a say in the type of contraceptive methods they used. 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 their family arranging the marriage (column 4). It is therefore noteworthy that the RD treatment effect in Panel B shows an 11 percentage points (21 percent relative to the sample mean) increase in the proportion of women who decided on their husband themselves (as opposed to their families). Furthermore, the additional year of schooling caused by the reform also led to women being 10 percentage points more likely to have a say in the type of contraceptive method adopted (column 5)[22]. These findings suggest that the increase in education caused by the reform led to higher decision-making rights for women in the country.

Finally, in column 6 of Table 4 we estimate the effect on whether the marriage involved the payment of a brideprice by the respondent's husband's family to her parents. This outcome can be interpreted as an alternative measure of the degree to which the marriage was conducted in a socially conservative manner. The RD treatment effect is -0.08 and statistically significant at 95% confidence level. The results so far in this table suggest that despite not affecting timing of either marriage or fertility, education resulted in higher decision rights for women and affected the manner in which the marriage took place, moving away from the traditional institution of brideprice. Together with previous section's documented reduction in religiosity levels, these findings suggest that education had a socially progressive impact on women's status.

4.4 Labor Market Outcomes

> *"Humankind is made up of two sexes, women and men. Is it possible for humankind to grow by the improvement of only one part while the other part is ignored? Is it possible that if half of a mass is tied to earth with chains that the other half can soar into skies?"* – Mustafa

[22]Note that this question was asked only to respondents who reported currently using any contraception. As such, one concern is whether the effect is driven by sample selection in this regression. To test for this, we estimate the RD treatment effect using as the dependent variable a dummy variable equal to one if the respondent reports using any contraceptive method. The point estimate for the treatment effect on whether the couple uses any contraceptive method is 0.013 with a standard error of 0.044. As such, the result on decision-making power with respect to the type of contraceptive method used is unlikely to be driven by a selection effect.

Kemal Atatürk[23]

An extensive literature in economics points to improved labor market outcomes (both in terms of participation and earnings) as an important mechanism through which education may improve women's rights. The 2008 TDHS contains a number of measures on women's labor market outcomes. In particular, we can observe if the surveyed women were currently working, their sector of employment and the position they had in their job (whether they were an employer, own-account worker or a wage worker). Unfortunately, the survey did not include any measure of individuals' labor earnings. In this section, we examine whether the increase in women's education caused by the reform led to any improvements in their labor market outcomes.

Panel A of Table 5 shows that years of schooling is positively and significantly correlated with labor force participation (as measured by whether the respondent is currently employed). Moreover, it is positively correlated with employment in the non-agricultural sector as well as with more regular forms of employment (such as having a regular wage-paying job as opposed to being self-employed, working as an unpaid family worker or a daily/seasonal wage worker). On the other hand, the RD estimates in Panels B and C show in general an insignificant effect of the reform on all outcomes except for employment in the non-agricultural sector and self-employment. The difference, in particular for non-agricultural employment, seems to come from the Imbens and Kalyanaraman [33] algorithm calculating a rather large bandwidth in Panel B – nearly twice that of the bandwidth used in Panel C. The muted effect on labor is likely to be a result of the heterogeneous effect that education may have on women from different social backgrounds. In particular, education may have lower (or no) effects on labor market outcomes of women from more conservative backgrounds who are likely to face other (e.g. social) constraints than lack of human capital to their participation in the labor market. On the other hand, women from more socially progressive backgrounds may benefit more in terms of employment opportunities from the education reform. We will return to this issue in section 5 where we discuss the heterogeneity of the reform's effects. Both RD specifications show a positive effect on female self-employment, which often represents a compromise, allowing female labor force participation under circumstances less subject to gender discrimination and more culturally acceptable than other kinds of labor[24] (White [63]).

Going back to the results in Table 5, in addition to most estimates being individually insignificant, we also fail to reject composite hypothesis that columnwise estimates are jointly different from zero (the test yields a p-value above 0.3) in panels B and C. Thus, while we cannot entirely rule out that the education reform had some impact on labor, these effects are, at least in relative to studies from other (non-Muslim) settings, uncharacteristically subdued and imprecise.

[23] Quoted in Vakit on 30 March 1923.

[24] 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 [61], Özcan *et al* [52]). In results not reported in the paper, we find that the reform did not increase women's likelihood to obtain a university-degree. As such, it is likely that the increase in schooling induced by the reform was enough to induce some women to enter into self-employment but not enough to make them acquire more desirable formal sector jobs.

4.5 Spouse Characteristics and Household Wealth

Having examined impacts on fertility, marriage, and labor-related measures of empowerment, we now turn to the issue of whether the education reform had effects on household and spousal outcomes. We start by examining household wealth. The DHS survey includes data on ownership of different household assets and services (e.g. internet) from which we construct a household wealth index. This index is constructed using principal component analysis from ownership data. The household wealth index equals the first principal component of twenty dummy variables, each equal to one if the household owns different assets or services (the estimates for each individual item are provided in the Appendix Tables 9 and 10). The first column in Panel A of Table 6 shows that years of schooling is correlated positively with the wealth index. The RD estimate in column 1 of Panel B reveals a treatment effect of 2.4 percentage points, corresponding to a 5 percent increase in the wealth index of treated women relative to the sample mean. More detailed examination of the breakdown of the index into its specific components (in Appendix tables 9 and 10) shows that this positive impact is predominantly driven by an increase in the ownership of household assets related to domestic tasks and chores. 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. Moreover, the household wealth-increasing effect of education can also be seen in column 2 of Table 6, which shows a positive effect on the likelihood that the household owns – as opposed to rents – the residence that they live in. Women in treated cohorts are 8 ppt more likely to live in a residence owned by themselves or their family. 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 live in wealthier households where they are more likely to own assets that make it easier to carry out domestic tasks – a task typically carried out by the women in Turkish households.

In the remaining columns of Table 6 we focus on the characteristics of women's spouses, as reported by the female respondents in the TDHS. Our main motivation for examining the impacts of the reform on spousal outcomes is to see whether women's education affected the quality of their husbands, and in particular to test for evidence of assortative matching in the marriage market (Becker [7]). As we discussed earlier in section 4, we find no evidence that the reform had a direct effect on men's schooling. Moreover, the average interspousal age gap in our sample is around five years (5.1) and the average husband in our sample is aged 28 and as such the husbands in our sample are far from the age threshold that would have included them in the cohorts directly affected by the reform.

Column 3 in Panel B of Table 6 shows that the reform did not have any significant effect on the average age-gap between women and their husbands. The point estimate of the RD treatment effect of the reform on women's husbands' age is 0.11, but this is imprecisely estimated at conventional levels. In column 4, we focus on the years of schooling completed by women's spouses. On average, spouses have 8 years of schooling – or equivalently, circa 2 more years of schooling than women. The OLS estimate in panel A shows that women's and their spouses' years of schooling are strongly positively correlated; one additional year of schooling is correlated with nearly half an additional year of the spouse's schooling years. Yet the RD estimates in panels B and C show smaller and statistically insignificant effects of around 0.17-0.2 additional years of schooling. One implication of this finding (of no impact of the reform on spouses' years of schooling) is that the reform effectively reduced the interspousal gap in years of

schooling. The one-year increase in women's years of schooling consequently reduced the interspousal gap by half. Looking at types of schooling in columns 5-7 results in mostly insignificant estimates although the estimates for high school and university completion are large. For example, the point estimate for university completion implies that women in treated cohorts were 3 percentage points (37 percent relative to the sample mean) more likely to have a spouse with a university degree.

Finally, in columns 8-11 of Table 6, we look at the spousal employment characteristics including whether they have a job in the nonagricultural sector and whether they are in self-, regular wage, or daily-wage employment. Our results show no clear effects on these outcomes; RD estimates are somewhat larger and have the same sign as the OLS estimates but are all statistically insignificant. Naturally, the reform may still have had an effect on some unobserved dimension of spousal quality that we do not capture with our data, such as wages or individual wealth.[25] Yet, in employing a standard set of spousal outcomes common in the literature on assortative matching, we do not find robust evidence that this is the main channel at work.

5 Discussion

Our results reveal broad empowerment effects of education across multiple outcomes. Yet whereas we document precise and meaningful impacts on measures of religiosity, decision rights and household wealth, we fail to find systematic evidence that these effects are driven by improvements in women's labor market outcomes or by improvements in the 'quality' of their spouses through assortative matching in the marriage market.

Our focus on a sample of married women in their early twenties could further emphasize our treatment effects on women who happen to marry earlier. Thus our local treatment effect may focus on, not just women in their early twenties, but also those with certain characteristics making them more likely to marry somewhat earlier. Although the RD estimate may differ from the average treatment effect, it still carries significant policy relevance, and as discussed in Section 3.3, the women under study is likely a representative set of compliers with the reform. Most accounts of the reform document the explicit aim of increasing education among the poorer and more socially conservative parts of the Turkish society (Dülger [20]). To the extent that women in these environments tend to marry earlier, our treatment effect should be particularly informative of women in poorer and more socially conservative communities, and therefore highly relevant for an evaluation of the reform.

Putting our results in a context more specific to this subpopulation allows us to better understand the variation in impacts across the outcomes. Overall we find little effects on timing of marriage and fertility and cannot document systematic differences in spousal quality except indirectly through increases in household wealth. A potential explanation for this is that the benefits of delaying marriage are weighed against those of marrying earlier – and more independently – into a household with substantial bargaining power. Our documented effects on household wealth, largely driven by appliances related to the role of a housewife, as well as better control over decisions may be enough to balance against benefits of delaying marriage.

[25] One interpretation of the effect on household wealth could be higher spousal quality. This interpretation would require the assumptions that the only source of income for respondent's household is her husband's labor or rental income, and that our ownership indicators in Tables 9 and 10 in the Appendix contain sufficient information on wealth.

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 11 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 reveal estimates in line with a relative empowerment[26]. The same can be said when investigating attitudes towards domestic violence (Table 12 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".

If it is indeed the case that the reform's effects are especially pronounced among the more socially conservative groups in the country, this may also help understand the lack of clear effects on women's labor market outcomes. These are often the environments where barriers to female entry into the labor market are higher. In such cases, increases in bargaining power due to more education could in some cases lead to higher labor force participation. But in cases where women face significant discrimination in the labor market, the mere possibility of entering the labor market could also result in higher rents – in our case, more home appliances – in their roles as housewives. This raises the possibility that increased education may affect women differentially, depending on the degree of severity of the constraints to their labor force participation.

We explore such heterogenous effects by splitting the sample into two: women who live in rural parts of the country and those who live in an urban setting. To the extent that women in rural settings also live in socially conservative environments compared to women form urban areas, we may expect the effects of education to vary across these two subpopulations. Table 7 reports results on some key outcomes from previous tables using local linear RD specifications in three panels; Panel A representing results for the full sample, Panel B for the rural sample, and Panel C for the urban sample. To ease the comparison of groups, we focus on specifications using one single bandwidth, the optimal bandwidth form the first-stage regression in column 1 of Table 2.[27] Comparison of the mean levels of the outcome variables in Panels B and C shows that women with rural backgrounds tend to exhibit more socially conservative outcomes; they are less educated, more pious, have less of a say in marriage decisions, are more likely to marry under a bride price, less likely to be employed in the non-agricultural sectors or be self-employed.

Comparing the estimates in Panels B and C of Table 7 shows that even though treatment effects on years of schooling are essentially constant across the two subsamples, the effects on other outcomes exhibits some considerable divergence. Among the rural sample, education seems to have lowered religiosity levels, significantly increased the possibility for women to choose their own husbands, lowered the incidence of bride price paid, albeit having essentially no effect on labor force participation. For the urban sample, we observe similar yet somewhat smaller effects on religiosity levels and marriage decisions, no effect on the incidence of bride price paid, but rather large and significant impacts on employment in the nonagricultural sector and self-employment. Also, while we find no statistically significant evidence of assortative matching on either spousal age or schooling, the estimates for the subgroups have the opposite signs, negative for the rural sample and positive for the urban sample.

[26]There is also a positive, although less precise effect, on agreeing with the statement that "women don't need to be virgins on the wedding night".

[27]Results using specifications allowing the bandwidth to vary with outcomes results in very similar estimates.

Effects on household wealth are similar across groups.

Overall, the results presented in Table 7 suggest that not only does education affect women across the whole socioeconomic spectrum, but it also exhibits heterogeneous effects depending on whether they live in a rural or an urban area. In more socially conservative communities, education may empower women in the sense of allowing more freedoms not just in religious expressions, but also in marriage decisions and in reducing traditional characteristics of marriage. In more socially progressive communities, education's effects also lead to increased labor force participation and changes in occupational choices. One policy implication is increased schooling at the secondary level may lead to higher female labor force participation in urban areas, even in a pre-dominantly Muslim society like Turkey. On the other hand, to increase female labor force participation for women in rural and socially conservative communities, increasing schooling may not be sufficient.

6 Concluding Remarks

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

Can education empower women in a majority-Muslim setting? Economists have long hypothesized that one important mechanism through which higher education may improve women's status within the household is by improving their (potential) earnings and their corresponding bargaining power. In Muslim societies where women's labor force participation is notoriously low and has remained so despite progress in female education, a key question is whether education is sufficient to enable women to break out of traditional institutions and to enjoy greater decision-making power and eventually higher labor force participation and wealth.

To answer this question, we assess empowerment effects of female schooling in Turkey using a change in compulsory schooling laws. A new law, implemented in 1998, bound individuals born after a specific date to 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.

Using this RD Design, we document a policy-induced increase in schooling for married women of one year on average. A key component of our institutional setting, similar to Muslim countries in general, is a society where barriers to female labor force participation are considerable and linked to religiously conservative preferences. As such, we first examine the effects of this additional year of schooling on the religiosity of the targeted women. We find large reducing effects of this additional year of schooling on expressions of religiosity. In particular, we find that treated women are on average less likely to wear a headscarf, attend Qur'anic courses and to pray regularly. These effects on religiosity are matched by a partial empowerment effect, whereby women are more likely to make decisions on issues like marriage and family planning themselves. Despite the pro-empowering effects of schooling we fail to find precise effects on labor force participation or other labor market outcomes. We also do not find strong evidence of educational assortative matching in the marriage markets. On the other hand, we find that treated women are more likely to live in wealthier households. Consequently, our results show that returns to

schooling in terms of women's status and living conditions may be substantial even when labor-related returns are not.

Turkey is particularly interesting for our purposes as it allows us to test the effect of education in widely different environments varying in the level of women's rights. In more rural and socially conservative communities, we find that education allows more freedom of religious expression, greater decision rights over marriage, less traditional forms of marriage, but limited impact on labor force participation. In the urban and more socially progressive communities, education had many similar effects and it also induced an increase in female labor force participation.

Although our findings are consistent with a general secularizing effect of schooling, they also raise the question to what extent this is driven by teaching values and habits? Even though we cannot entirely rule out the possibility of an indoctrination effect of secular schooling in our context, the fact that we find parallel empowerment effects of the policy implies that the additional schooling did more than impose state-mandated religious behaviors on the affected women.

Our estimated effects of education point to a relative empowerment, yet they also point to a limit in what education may accomplish in socially conservative communities, at least in the short to medium term. Turkey, as well as many other Muslim countries have made significant advances in reducing gender inequality in education. But as long as education policy serves to make women better mothers or home makers, the full potential of female participation may not be achieved.

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TABLE 1: SUMMARY STATISTICS OF EVER MARRIED WOMEN 16-26 YEARS OF AGE

Panel A: Education			
	Mean	SD	Obs
Years of Schooling	6.29	3.80	1557
Junior High School	0.38	0.49	1557
High School	0.21	0.41	1557
Primary School	0.88	0.32	1557
Vocational School	0.06	0.23	1557
Panel B: Religiosity			
	Mean	SD	Obs
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: Marriage and Birth			
	Mean	SD	Obs
Age of First Marriage	18.75	2.77	1557
Age at First Birth	19.67	2.57	1187
Own marriage decision	0.55	0.50	1554
Own contraception decision	0.86	0.34	908
Bridesmoney paid	0.19	0.39	1557
Number of children	1.24	1.03	1557
Panel D: Labor, Household, and Spouse Outcomes			
	Mean	SD	Obs
Employed	0.19	0.39	1555
Employed in non-agricultural sector	0.10	0.30	1557
Husband employed	0.93	0.25	1519
Interspousal schooling difference	1.67	3.53	1511
Interspousal age difference	5.34	3.79	1519
Wealth index	0.47	0.17	1539
Panel E: Pre-determined covariates			
	Mean	SD	Obs
Non-Turkish-speaking mother	0.31	0.46	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 description are in Appendix A.

Table 2: RD Treatment Effects on Schooling

Outcome	Years of Schooling				Completed Education			
					Jr. High	High	Primary	Vocational
Bandwidth	$\hat{h}$	$\hat{h}/2$	$\hat{h}/3$	$2\hat{h}$	$\hat{h}$	$\hat{h}$	$\hat{h}$	$\hat{h}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome mean	6.32	6.15	6.15	6.42	0.38	0.23	0.88	0.06
Panel A: Linear control function								
Treatment	1.018***	1.087***	0.901***	0.573**	0.237***	0.079***	0.058**	0.005
	(0.213)	(0.246)	(0.290)	(0.226)	(0.038)	(0.028)	(0.026)	(0.021)
Panel B: Quadratic control function								
Treatment	1.004***	1.015***	0.917**	1.227***	0.121***	0.094**	0.099***	-0.004
	(0.295)	(0.356)	(0.381)	(0.315)	(0.044)	(0.039)	(0.035)	(0.033)
Panel C: Cubic control function								
Treatment	1.004***	1.124*	1.186*	0.858**	0.071	0.112**	0.140***	-0.021
	(0.357)	(0.625)	(0.611)	(0.394)	(0.053)	(0.046)	(0.045)	(0.046)
Bandwidth	69	34	23	137	60	95	44	71
Obs	1777	923	607	3279	1536	2412	1195	1849

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 [33] 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. 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-of-birth 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	Quran study	Prays 5/day	Prays At All	Fasts Regularly
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome mean	0.43	0.77	0.44	0.39	0.72	0.88
Panel A: OLS						
Years 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: Local linear RD with optimal bandwidth						
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)
Joint p-value		0.039				
Bandwidth	66	71	62	65	90	101
Obs	1679	1847	1591	1680	2294	2499
Panel C: Local linear RD with static bandwidth						
Treatment	-0.052**	-0.070*	-0.078*	-0.054	0.001	-0.007
	(0.021)	(0.037)	(0.042)	(0.044)	(0.039)	(0.027)
Joint p-value		0.101				
Bandwidth	69	69	69	69	69	69
Obs	1798	1799	1801	1799	1799	1798

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 [33] 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 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 $w_i = \frac{1-\mu_i}{\sum_J (1-\mu_j)}$ 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-of-birth 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 (2) through (6). Standard errors are clustered by month-year-cohort.

Table 4: RD Treatment Effects of Education on Marriage Characteristics

	Age at first marriage	birth	Number of children	Own decision on marriage	contracep.	Brideprice paid
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: OLS						
Outcome mean	18.63	19.65	1.18	0.54	0.86	0.20
Years of Schooling	0.207***	0.165***	-0.077***	0.036***	0.006*	-0.015***
	(0.023)	(0.028)	(0.008)	(0.003)	(0.003)	(0.004)
Bandwidth	52	60	52	94	73	53
Obs	1343	1169	1343	2352	1157	1371
Panel B: Local linear RD with optimal bandwidth						
Treatment	-0.094	-0.179	0.053	0.113***	0.101***	-0.080**
	(0.265)	(0.234)	(0.097)	(0.041)	(0.036)	(0.032)
Joint p-value	0.001					
Bandwidth	52	60	52	94	73	53
Obs	1343	1169	1343	2352	1157	1371
Panel C: Local linear RD with static bandwidth						
Treatment	-0.190	-0.149	0.071	0.137***	0.108***	-0.052*
	(0.239)	(0.218)	(0.088)	(0.046)	(0.037)	(0.030)
Joint p-value	0.001					
Bandwidth	69	69	69	69	69	69
Obs	1801	1396	1801	1798	1082	1801

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 [33] 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 respondent's achieved age at the time of her first marriage. The dependent variable in column (2) is respondent's achieved age at the time of her first birth. The dependent variable in column (3) is the number of living children delivered by the respondent. The dependent variable in column (4) is a dummy variable equal to one if the respondent reported that she decided on her most recent marriage jointly with her husband (as opposed to being decided by their families). The dependent variable in column (5) is 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]. The dependent variable in column (6) is a dummy variable equal to one if the respondent's family received a brideprice from her husband's family upon their wedding. 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-of-birth 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 (6). Standard errors are clustered by month-year-cohort.

Table 5: 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 [33] 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-of-birth 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 6: RD Treatment Effects of Schooling on Husband Characteristics and Wealth

	Household Wealth		*Husband's schooling*					*Type of husband's job*			
	Index	House Owner	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)	(10)	(11)
Outcome mean	0.48	0.66	28.59	8.01	0.54	0.37	0.09	0.86	0.26	0.54	0.11
Panel A: OLS											
Years of Schooling	0.015***	-0.009***	-0.115***	0.452***	0.041***	0.041***	0.026***	0.012***	-0.010***	0.025***	-0.008***
	(0.001)	(0.003)	(0.036)	(0.020)	(0.003)	(0.005)	(0.003)	(0.003)	(0.004)	(0.004)	(0.002)
Bandwidth	87	103	74	122	98	61	70	63	80	78	88
Obs	2197	2577	1858	2944	2375	1539	1741	1570	2038	1952	2188
Panel B: Local Linear RD, optimal bandwidth											
Treatment	0.024**	0.082**	0.108	0.165	-0.039	0.085*	0.033	0.049	-0.029	0.048	-0.014
	(0.012)	(0.038)	(0.281)	(0.267)	(0.041)	(0.044)	(0.025)	(0.030)	(0.037)	(0.035)	(0.027)
Joint p-value	0.001										
Bandwidth	87	103	74	122	98	61	70	63	80	78	88
Obs	2197	2577	1858	2944	2375	1539	1741	1570	2038	1952	2188
Panel C: Local linear RD with static bandwidth											
Treatment	0.022*	0.076*	0.148	0.199	-0.020	0.058	0.033	0.041	-0.025	0.038	-0.009
	(0.012)	(0.040)	(0.283)	(0.285)	(0.042)	(0.042)	(0.025)	(0.029)	(0.040)	(0.037)	(0.028)
Joint p-value	0.009										
Bandwidth	69	69	69	69	69	69	69	69	69	69	69
Obs	1782	1801	1757	1790	1741	1741	1741	1757	1757	1757	1757

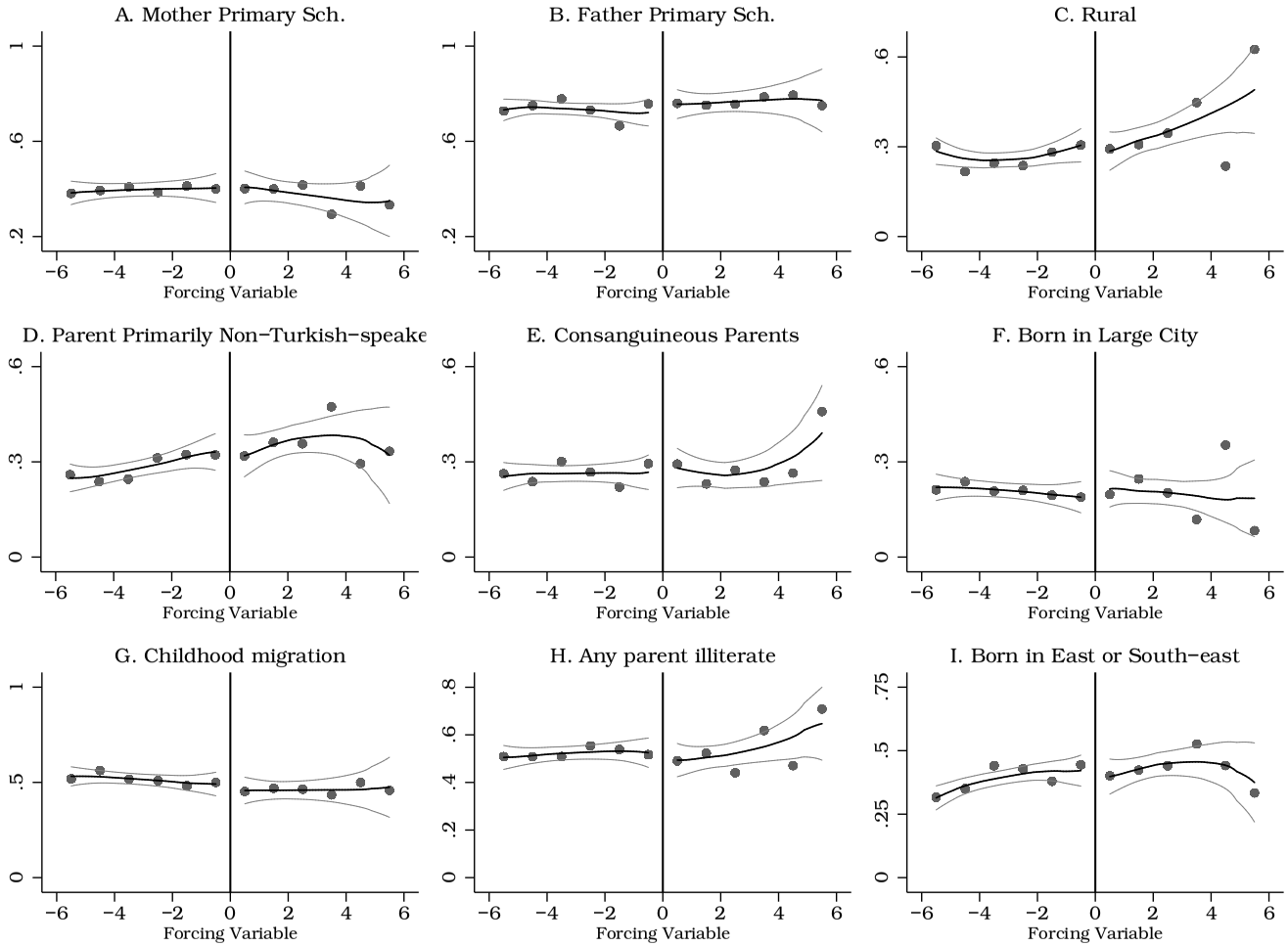
Notes: Data is from the *Ever Married Module* of the *Turkey Demographic and Health Survey of 2008*. Panel A reports OLS results with years of schooling as the independent variable for an optimal bandwidth $\hat{h}$ determined by the Imbens and Kalyanaraman [33] algorithm. Panel B, using the same bandwidth, 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 a household wealth index equal to the first principal component of 20 dummy variables, each one equal to one if the respondent's household owns the relevant asset/service. Details on the types of assets/services included in the index are provided in Appendix A. The dependent variable in column (2) is a dummy variable equal to one if the respondent's family owns the house that they currently reside in so that they don't need to pay rent for it. The dependent variable in column (3) 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 Table5 above. Regressions include same set of control variables as in Table5 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 (11). Standard errors are clustered by month-year-cohort.

Table 7: Heterogenous RD Treatment Effects in Rural and Urban Samples

	Years of Schooling	Religiosity Index	Marriage Decision	Brideprice Paid	Employment		Husband's		Wealth Index
					Non-agriculture	Self-employed	Yrs of Sch.	Age	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Local linear RD, Full sample									
Outcome mean	6.34	0.43	0.54	0.18	0.11	0.03	7.96	28.45	0.32
Treatment	1.017***	-0.052**	0.137***	-0.052*	0.048**	0.031**	0.199	0.148	0.018*
	(0.213)	(0.021)	(0.046)	(0.030)	(0.022)	(0.015)	(0.285)	(0.283)	(0.010)
Bandwidth	69	69	69	69	69	69	69	69	69
Obs	1801	1798	1798	1801	1801	1799	1790	1757	1780
Panel B: Local Linear RD, Rural sample									
Outcome mean	4.91	0.46	0.44	0.29	0.04	0.01	6.73	27.92	0.39
Treatment	1.084**	-0.080*	0.197**	-0.130**	0.015	-0.010	-0.191	-0.314	0.022
	(0.452)	(0.045)	(0.087)	(0.065)	(0.026)	(0.013)	(0.380)	(0.672)	(0.022)
Bandwidth	69	69	69	69	69	69	69	69	69
Obs	504	502	503	504	504	504	502	496	498
Panel C: Local Linear RD, Urban sample									
Outcome mean	6.89	0.42	0.58	0.14	0.14	0.04	8.44	28.66	0.50
Treatment	1.074***	-0.039*	0.127**	-0.022	0.071**	0.046**	0.436	0.234	0.025*
	(0.243)	(0.021)	(0.057)	(0.036)	(0.029)	(0.020)	(0.348)	(0.361)	(0.014)
Bandwidth	69	69	69	69	69	69	69	69	69
Obs	1297	1296	1295	1297	1297	1295	1288	1261	1284

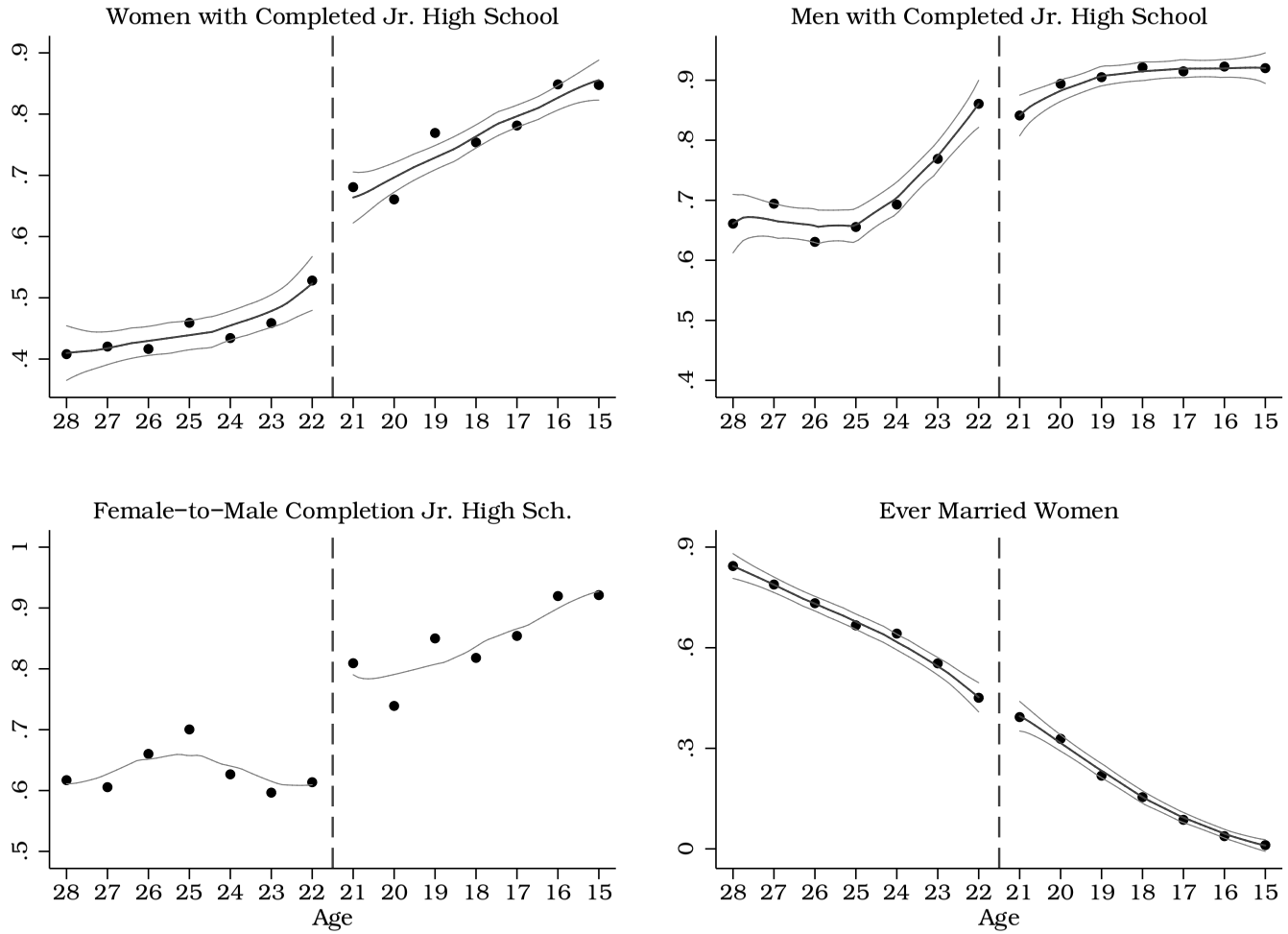
Notes: Data is from the *Ever Married Module* of the *2008 Turkey Demographic and Health Survey*. Panel A reports, for the full sample, local linear RD results using the static bandwidth of 69 months and a linear control in month-year-of-birth, where being born after September 1986 is the treatment variable. Panel B and C report corresponding results but for the rural and urban samples respectively. The dependent variable in column (1) is the number of completed years in school. The dependent variable in column (2) 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 $w_i = \frac{1-\mu_i}{\sum_J (1-\mu_j)}$ where μ_i is the mean of religiosity variable i. The dependent variable in column (3) is a dummy variable equal to one if the respondent reported that she decided on her most recent marriage jointly with her husband (as opposed to being decided by their families). The dependent variable in column (4) 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 (5) 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 (6) 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 (7) is the number of years respondent's husband completed in school. The dependent variable in column (8) is the completed age (in years) of the respondent's husband. The dependent variable in column (9) is a household wealth index equal to the first principal component of 20 dummy variables, each one equal to one if the respondent's household owns the relevant asset/service. Details on the types of assets/services included in the index are provided in Appendix A. 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 for whether the respondent's parents were related by blood, month-of-birth fixed effects, and region-of-birth fixed effects. Standard errors are clustered by month-year-cohort.

Figure 1: Balanced Covariates



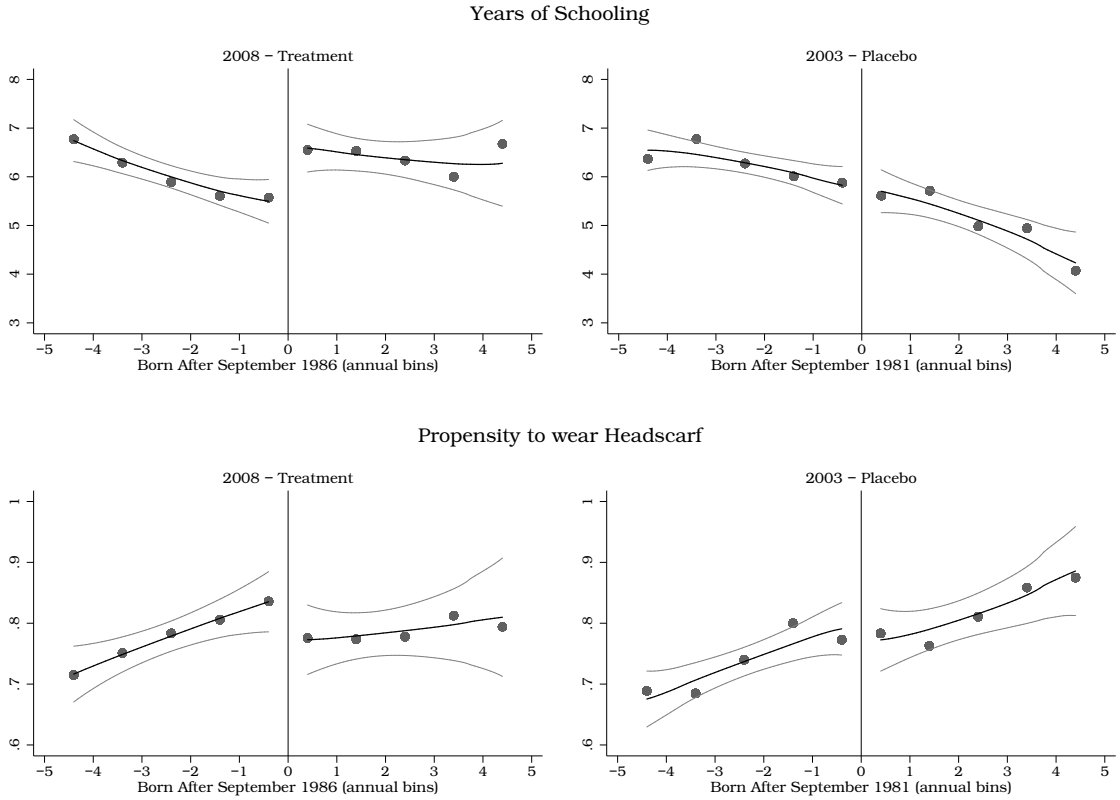
Notes: Figures show pre-determined covariates in annual average means against the forcing variable 6 years within the threshold of being born in September 1986. The vertical line in each graph represents the cut-off birth date (September 1986) as defined by the compulsory schooling reform – any individual 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. A test of the null hypothesis that the discontinuity jumps in all graphs are jointly zero yields a p-value of 0.82. The sample includes observations from the Turkey DHS ever married women module. 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 d dummy variable taking the value of one if the respondent lived 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. . 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



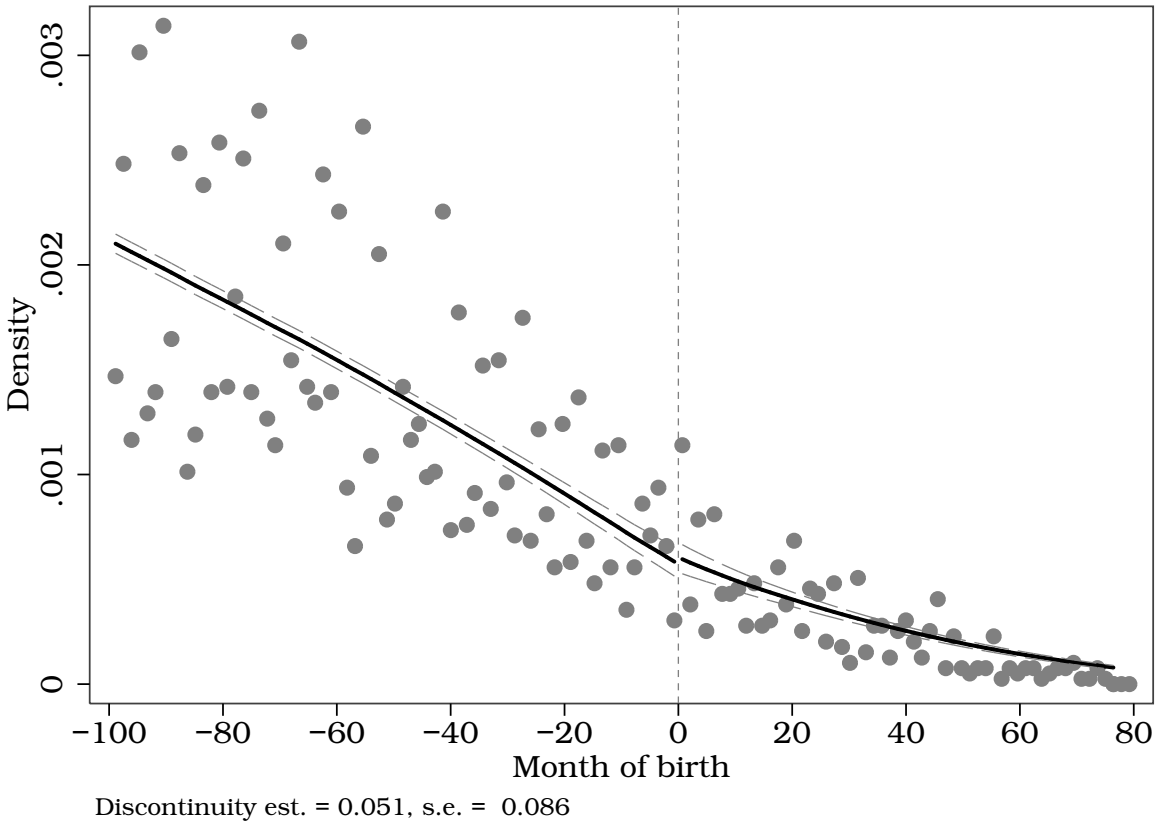
Notes: The sample includes observations from the Turkey DHS household module. The forcing variable is the age in years of the relevant individual(s). Upper left hand panel plots the average level of completion of junior high school for *all women* in the sampled households, in annual age averages at interview date. The upper right graph plots the average level of completion of junior high school 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 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



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 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 the outcome variable at the annual birth cohort level are plotted as dots. 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 to the street. All outcome variables are self-reported by the respondent.

FIGURE 4: DENSITY TEST



Notes: The graph shows the McCrary [45] test of whether there is a discontinuity in the density of month of birth..

A Data variables

- *Years of Schooling* – Number of completed years of school.
- *Junior High School* – Respondent completed 8 years long education, i.e. both primary and lower secondary (junior high school) education.
- *High School* – Respondent completed 11 years long education, i.e. primary, lower secondary, and upper secondary (high school) education.
- *Vocational School* – The last school attended by the respondent was a vocational school.
- *Wears Headscarf* – Respondent reported that she regularly wears a headscarf when going out on the street.
- *Attended Qur'an course* – Reported as having ever attended a course to study the Qur'an.
- *Prays 5/day* – Respondent reported as regularly performing namaz, or prayer.
- *Prays at All* – Respondent reported as occasionally performing namaz, or prayer.
- *Fasts Regular* – 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 of First Marriage* – Respondent's achieved age at the time of her first marriage.
- *Age at First Birth* – Respondent's achieved age at the time of her first birth.
- *Number of children* – Number of living children delivered by the respondent.
- *Marriage decision* – 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).
- *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].
- *Brideprice paid* – A dummy variable equal to one if the respondent's family received a brideprice from her husband's family upon their wedding.
- *Any employment* – A dummy variable equal to one if the respondent reported that she is "currently working", and zero otherwise.
- *Sector of employment: Non-agriculture* – A dummy variable equal to one if the respondent is currently employed in the industrial sector or in services.

- *Sector of employment: Agriculture* – A dummy variable equal to one if the respondent is currently employed in the agricultural sector.
- *Type of employment: Self-employed* – A dummy variable equal to one if the respondent's position at her current job is 'employer' or an 'own-account' worker.
- *Type of employment: Unpaid family-labor* – A dummy variable equal to one if the respondent's position at her current job is 'unpaid family worker'.
- *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)'.
- *Husband's schooling* – Variables related to respondent's husband's schooling is defined identical to respondent's schooling variables, described above. They are reported by the female respondent in the ever-married women module.
- *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.
- *Household Wealth 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.
- *House owner* – A dummy variable equal to one if the respondent's family owns the house that they currently reside in so that they don't need to pay rent for it.
- *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.

- *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.
- *Birth province dummies* – Dummy variables for each of the 83 birth provinces where the respondent was born.

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: RD Treatment Effects of Schooling 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 [33] 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 10: RD Treatment Effects of Schooling 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 [33] 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 11: RD Treatment Effects of Schooling 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 [33] 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 12: RD Treatment Effects of Schooling 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 [33] 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 13: 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 14: RD TREATMENT EFFECTS ON EDUCATION FOR MARRIED VS NEVER-MARRIED WOMEN

	(1)	(2)	(3)	(4)
Panel A: Dependent Variable – Years of Schooling				
	Sample: Ever-married Women		*Never-married 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		*Never-married 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 *Turkey Demographic and Health Survey of 2008*. 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 Never-Married 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 [33] 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