The Effect of Girls' Schooling on Their Mothers' Attitudes Towards Domestic Violence: Evidence from Turkey [1]

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Abstract

We estimate the intergenerational spillover effects of children's education on their mothers' attitudes towards domestic violence in Turkey. To identify the causal effect of children's education, we exploit a reform that took place in Turkey in 1997 and expanded compulsory schooling from 5 to 8 years. Using a regression discontinuity design based on monthly birth cohorts and data from the 2008 Turkey Demographic and Health Survey, we show that mothers whose daughters were affected by the reform (which provided them more schooling) are 17 percentage points less likely to find domestic violence justified. Examining the potential mechanisms behind this effect, we find suggestive evidence that mothers are reacting to the increase in domestic violence that resulted from the rise in their daughters' schooling.

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1 Introduction

Domestic violence affects nearly one in three women globally (WHO 2013), with women in low-income countries being nearly 10 times more likely to experience domestic violence compared to women in high-income settings (Heise and Kotsadam 2015). Both economic and cultural factors may be causing this pattern (Alesina et al. 2016). Gender-biased norms and attitudes exist in many developing countries and propagate the unequal treatment of women (Jayachandran 2015). In particular, norms related to the acceptability of domestic violence vary across and within societies, and are highly correlated with the prevalence of domestic violence (García-Moreno et al. 2005). We have limited evidence on what determines these norms, and whether experiences of domestic violence may affect individuals' attitudes towards it.

In this paper, we test whether children's education affects mothers' attitudes towards domestic violence and explore potential mechanisms through which this occurs. In order to identify the causal effect of children's education on their mother's attitudes, we exploit a change in compulsory schooling laws that took place in Turkey in 1997 and increased compulsory schooling from 5 to 8 years [5]. Previous work has shown that the reform increased women's schooling in particular, leading to a narrowing of the education gap between men and women in Turkey (Dincer et al. 2014, Gulesci and Meyersson 2016). Recently, Erten and Keskin (forthcoming) showed that the increase in women's education caused by the reform led to an increase in female employment in the non-agricultural sector, but also increased the incidence of domestic violence.

To identify the effects of the reform, we follow a regression discontinuity design (RDD) similar to Erten and Keskin (forthcoming). In particular, we compare outcomes of women whose children were born after a threshold date, relative to women who gave birth just before that date. When the Turkish parliament passed the new law in 1997, it affected students who were already in grade 4 and hence the law was retroactive (i.e. students and their parents could not manipulate exposure to the reform). The legally required age of starting primary and secondary school in Turkey is six and eleven years, respectively. This implies that children born before January 1987 (who should have completed grade 4 by 1997) could drop out of school after five years, while those born afterwards were required to complete eight years of schooling. As such, our RD design assigns treatment based

[5] Women's rights and female labor force participation remain low in many majority-Muslim societies (UNDP, 2005; Doepke et al., 2012) and Turkey is no exception. In a nationally representative survey, 42% of women reported being subject to domestic violence (TRPM, 2008). Relative to the rest of the world, this rate places Turkey among countries with the highest rates of domestic violence (WHO, 2013; Devries et al., 2013).

on whether a child's date of birth was before or after January 1987. While there may have been imperfect compliance for various reasons, previous studies have shown (and we confirm in our data) that on average the reform increased years of schooling at the threshold by approximately 1 additional year for women.

We use this treatment to estimate the (reduced-form) effects of children's schooling on their mothers' attitudes towards domestic violence and potential mechanisms behind this. We find that women whose daughters were exposed to the new compulsory schooling regime (henceforth "treated") are less likely to think that domestic violence is acceptable. In particular, mothers of treated girls are by 17 percentage points less likely to consider wife-beating justifiable than mothers of untreated girls, which lowers their acceptance of domestic violence by nearly 50% relative to the mean level of the scale. We find no significant effect on mothers whose sons were affected by the reform. The results are robust to alternative specifications, different bandwidths, and placebo tests for alternative threshold dates.

We explore three potential mechanisms through which an increase in girls' schooling may have led to a reduction in their mother's likelihood to find domestic violence acceptable. First, education may have changed girls' own attitudes toward domestic violence (e.g. Friedman et al. 2015), who may have then influenced their mother's attitudes towards it. We call this an "active persuasion" channel. In the TDHS data, we find a small and insignificant effect of the reform on girls' own attitudes towards domestic violence, consistent with the findings of Erten and Keskin (forthcoming) on a different dataset. Therefore, we conclude that this channel is unlikely to be the main one driving the effect on girls' mothers' attitudes towards domestic violence.

Second, a growing literature on the role of domestic violence as an instrument of intra-household bargaining shows that an improvement in women's employment or earnings may affect the incidence of domestic violence positively or negatively, depending on the bargaining power and the outside options of the spouses (Tauchen et al. 1991; Eswaran and Malhotra 2011; Bloch and Rao 2002; Anderson and Genicot 2015). Thus, if the reform had any spillover effects on treated girls' mother's labor market outcomes[6], it may have also affected their own exposure to and attitudes towards domestic violence – we call this the "economic empowerment" channel. When we test for the effect of the reform on girls' mothers' labor market outcomes, we find no significant effect on any dimension. In particular, we test if the mothers of treated girls have different employment rates, occupations, cumulative employment durations and if they are less likely to have

[6] For example, an increase in compulsory schooling may have reduced the need for childcare and enabled mothers who would have otherwise had to quit their jobs (or work part-time) to stay in the labor force (or remain in full-time employment).

had to quit a job due to childcare needs at any point in their lives; but we find no discernable effect on any of these indicators. As such, we conclude that this mechanism is also unlikely to be at work in our setting.

Third, if the increase in girls' schooling caused by the reform affected her exposure to domestic violence, this may influence their mother's attitudes towards it through what we call a "parental empathy" mechanism. There is a large literature in psychology that shows that parents, in particular mothers, may be significantly distressed when they observe their children in painful situations (Guttman and Laporte 2000, Stern et al 2014, Goubert et al. 2006, Caes et al. 2012). Erten and Keskin (fothcoming) show that the resulting increase in women's education increased the likelihood that they experience psychological abuse and financial controlling behaviors by their spouses, and the effects were particularly strong for women who grew up in rural areas. Unfortunately the TDHS data does not include any variables that allows us to directly test for this mechanism, but we explore the heterogeneity of the effects across certain dimensions in order to assess if the parental empathy mechanism is driving the effects. In particular, we find that the effect on domestic violence attitudes are particularly strong for mothers whose daughters are likely to be married [7] and who live in rural areas. This is consistent with mothers reacting to their daughters' experiences of higher domestic violence in their marriages. The fact that the effect is stronger in rural areas could be explained either by the reform's effect on schooling being stronger in rural areas (Erten and Keskin (forthcoming)) or mothers being more likely to be informed about their daughters' marital life in rural areas. However, due to the sample size we have for the subsample analysis, we are cautious in interpreting this evidence as merely suggestive that the parental empathy mechanism is likely to be the one driving the effects.

Our paper contributes to a growing literature on intergenerational spillover effects of education.[8] Much of the empirical work on this subject has focused on estimating the effects that parents' education may have on their children's outcomes (see, for example, Black et al., 2005; Currie and Moretti, 2003; Oreopoulos et al., 2006; and Lundborg et al., 2014, in developed countries, and Breierova and Duflo, 2004; Chen and Li, 2009; and Glewwe, 1999, in developing countries). Few studies have explored the possibility of spillover effects of children's schooling on their parents'

[7] In the TDHS, we only observe the marital status of children who are still living with the respondent. For daughters who are no longer living in the same household, we assume that they are married. This is consistent with social norms in Turkey whereby chidlren, especially girls, are unlikely to leave their parents' residence until they get married.

[8] More generally, this is also linked to research on broader non-pecuniary effects of education that were recently summarized by Oreopolous and Salvanes (2011) and Lochner (2011).

outcomes, mainly in developed countries, and have found mixed evidence. Berniell et al. (2013) find that health education in primary schools in the US led to an increased physical activity among parents of exposed children. Torssander (2013) and Friedman and Mare (2014) find a positive relationship between children's education and their parents' longevity in Sweden and USA, respectively. In contradiction to this, Lundborg and Majlesi (2015) use changes in compulsory schooling laws to estimate the causal effect of children's education on their parents' longevity in Sweden, and do not find a significant effect. Kuziemko (2014) finds that children who acquire certain skills might disincentivize their parents from acquiring the same skill, i.e. parents lean on their children rather than learn from them. We contribute to this literature by providing evidence on the causal effects of children's education on their mothers' attitudes towards domestic violence in a developing country. We also show that parental empathy is likely to be an important mechanism through which children's education and experiences may affect the attitudes of their parents.

Our paper is also related to the literature on cultural transmission. Most studies have looked at the transmission of cultural norms from parents to their children (see Bisin and Verdier, 2010, for a review). In terms of gender norms, empirical work has mainly focused on developed countries and on transmission from older to younger generation within the family (Alesina and Giuliano, 2010; Fernandez, 2007; Fernandez et al., 2004; Farre and Vella, 2013, Dhar et al. 2016). When it comes to women's attitudes towards domestic violence, literature has highlighted the role of intergenerational transmission (Campbell, 2002; Pollak, 2004) as well as the role of traumatic events, such as experience of civil war (Justino et al., 2015). We contribute to this literature by providing evidence on the role that education of the younger generation can play in shaping the gender norms and attitudes of their mothers.

The rest of the paper is organized as follows: Section 2 presents contextual background on the education reform that we study, section 3 presents the data, section 4 describes our empirical strategy, and section 5 presents the results. The findings are further discussed in section 6 and section 7 concludes.

2 Schooling Reform in Turkey

The compulsory schooling reform that we study took place in Turkey in 1997. Before the reform, Turkish education system consisted of three components: 5 years of primary school (ilkokul), 3 years of junior high school (ortaokul), and 3 years of high school (lise). Whereas primary education was compulsory, the two higher levels were voluntary. In 1997, the Turkish parliament adopted law No. 4306 which reformed the primary education system of the country.[9] The new education law stipulated an extension of compulsory schooling from 5 to 8 years, thus effectively merging primary and junior high school into what is now called primary education (ilkogretim). The option to attend religious junior high schools was consequently removed, the traditional diploma that had been awarded at the end of the fifth grade was abolished and replaced by a diploma for successful completion of the eighth grade.[10] The law was adopted on August 16, 1997, and went into effect as of school year 1997/1998.

According to the Turkish law, compulsory schooling begins in September of the year in which a child turns 6 years old. The new law No. 4306 made eight years of primary education compulsory and it was effective starting with the 1997/1998 school year. This implied that students who had completed the fifth grade in 1997 were exempt from the law while those who had completed the fourth grade were required to remain in primary education until they completed 8 years of schooling. The combination of these two laws – the law pertaining to the school starting age and the education reform which made eight years of schooling compulsory starting from 1997 – implied that children who were born in January 1987 or before would have completed fifth grade in 1997-98 school year and thus they would have been exempt from the new law. On the other hand, children born in January 1987 or later would have completed at most four grades in 1997-98 school year and therefore, they would have been required to stay in school for another three years. Naturally, there may have been factors weakening the link between child's date of birth and its exposure to the new compulsory education system, such as imperfect compliance with the school starting age or grade repetition. Nevertheless, the official requirements implied that children born before January 1987 were more likely to be exempt from the 1997 education law as compared to younger cohorts. It is precisely this consequence of the new law that facilitates the use of the RD design. In addition, it also allows us to isolate the effect of compulsory schooling reform from other policy changes that may have occurred in this period as there is no reason to expect that other policy changes should affect children born before or after January 1987 in a different manner.

The new law required a massive investment in education. This included expenditure on construction of schools, educational materials, and staff. Within just a few years of the

[9] The education reform was part of a broader set of policies implemented to curb the rise of Islamist movements in politics in 1990s. See Gulesci and Meyersson (2016) for details on the political context in which the reform was passed.

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

implementation of the reform, around 82,000 new classrooms were built thereby increasing the classroom supply by 30 percent, and 70,000 new teachers were recruited. In order to improve school access among 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 its 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. Thus, the law effectively resulted in an extension of the existing secular junior high school curriculum (Dulger, 2004). Finally, the legal change had a particularly strong effect on schooling of women, and especially so for women in more rural and socially conservative communities (Erten and Keskin, forthcoming).

3 Data

The data we use comes from the 2008 Turkey Demographic and Health Survey. TDHS is a nationally representative household survey in which 10,525 households were interviewed in 2008. The survey consists of a household module and a women's module. In the household module, basic information on all household members and general household characteristics is collected. The women's questionnaire is administered only to ever-married women aged 15-49; 7,405 ever-married women were interviewed in the 2008 TDHS. In this module, the respondent is asked extensively about her health, her full birth history and her attitudes, among other things. Importantly for our identification strategy, detailed information on women's and their children's date of birth (month and year of birth) is collected.

The sample of women and children in our analysis is determined by our research question. We are interested in understanding the impact of children's education on their mothers' attitudes towards domestic violence. Since we need to choose educational attainment of only one child per mother, we decide to consider the eldest child. There are several advantages to this approach. First, we avoid problems related to fertility decisions. Having another child is a decision that is influenced by many factors, including a woman's background, preferences, attitudes, and her prior birth history. Therefore, considering any other than the first-born children would arguably lead to endogeneity problems such as sample selection based on the respondents' (unobserved) characteristics. Second, the sex of the first-born child is likely to be reasonably exogenous whereas this is not necessarily the case for later-born children (Bhalotra and Cochrane, 2010; Rosenblum, 2013). In particular, a patriarchal society with preference for male offspring leads parents to behaviors where they desire at least one son. One of the strategies to ensure such an outcome is the so-called stopping rule: If the first-born child is a son, parents might decide not to have other children. However, if the first-born child is a daughter, parents might decide to have more children until they have at least one son. Therefore, the sex ratio among later-born children might be skewed towards boys. As part of our analysis, we look at the role of the child's gender in upward educational spill-over effects and any analysis by gender is only meaningful if the child's gender is reasonably exogenous. Finally, choosing first-born children for our analysis has consequences for the interpretation of our results. These will be discussed in section 6.

Our final sample consists of 6,258 women with first-born children for whom all relevant information is available. At the time of the survey, these women and children were on average 35 and 13 years old, respectively. Because our main results are estimated based on a subsample of women whose first-born children were born around January 1987 (see section 4.1 for more details), Table 1 shows descriptive statistics from a narrower sample of women who are on average 42 years old, and their children are 21 years old.[11] Additionally, the information is disaggregated by their first-born child's gender. The upper panel of Table 1 summarizes respondents' background characteristics. The average woman in the sample had 5 years of schooling. This corresponds to the majority of respondents (55%) having completed primary education. The other half of the sample is split between women with no education (27%) and women with secondary or higher education (18%). In terms of family background, one in five women (21%) comes from a family where Turkish is not the first language (most of these cases are Kurdish families), had consanguineous parents (20%) and around half (52%) of respondents spent their childhood in a rural area.

The second panel of the table shows descriptive statistics about women's labor market outcomes. About half (56%) of the women in the sample reported having ever had a job, and almost one third of them had been employed in the non-agricultural sector at some point in their lives (31%). In terms of their current employment status, only 33% of them were employed at the time of the survey and only 16% were working in the non-agricultural sector. In terms of the types of occupations they were doing, 13% were employed as "unpaid family workers", 7% were "self-employed" and the rest (13%) were "working for a wage". The survey also collected information about their job histories. Using this, we calculate respondents' total duration of employment

[11] Summary statistics are based on a bandwidth of 36 months around the cut-off which is the bandwidth from our main RDD specification – determined by the Calonico et al. (2014) optimal bandwidth algorithm . Descriptive statistics based on the full sample are shown in Table A1 in the Appendix.

(throughout their lives, in any type of employment), which turns out to be 9 years on average. Only 4% of them report ever having to quit a job due to childcare needs.

The bottom panel of Table 1 displays summary statistics on women's attitudes towards domestic violence, which will be our main outcomes of interest. In the 2008 wave of the TDHS, respondents in the woman's module were asked whether wife-beating is acceptable in seven different situations.[12] While domestic violence in some situations has very low acceptance rates, other reasons for wife-beating seem to be more widely acceptable. In particular, wife-beating for burning food, not cooking, and refusing to have sexual intercourse with the partner have low acceptability rates of 3%, 7%, and 10%, respectively. Arguing with the partner and neglecting housework lies in the middle field with 14% and 15% acceptance rate, respectively. On the other hand, neglecting children and wasting money are undesired actions where 19% and 20% of respondents state that a husband is justified in beating his wife. Based on these seven indicators, we create an aggregate measure of domestic violence – a binary variable that takes the value 1 if the woman deems at least one listed situation as justifiable for wife-beating. As shown in Table 1, 31% of women in the sample consider domestic violence to be acceptable in at least one of these situations.

Table 1 also reports the descriptive statistics by the gender of the first born child, and the statistical significance of the difference for every indicator. While there are no significant differences in terms of respondents' background characteristics by gender of their first-born child, there are some small differences in their attitudes towards domestic violence. In particular, 34% of respondents whose first-born child is a girl considered domestic violence to be acceptable (under at least 1 situation they were asked about), while the corresponding rate was 29% for respondents with a first-born son – and the difference is marginally significant at 90% confidence level. This is driven only by one of the situations in the survey (if the woman argues with her partner), which translates into a difference in the aggregate indicator. However, this is expected considering the number of tests we are conducting in Table 1: one out of 24 variables will be statistically significant at 5% level by chance. Therefore, we conclude that overall, the respondent's characteristics are fairly balanced by the gender of their first-born child. This is in line with the gender of the first born child being as good as random and therefore exogenous.

[12] Given that respondents had the option of answering "don't know" and this occurred to some extent (3% of the full sample), we treat "don't know" answers equivalent to finding wife-beating acceptable. The results are qualitatively identical if we omit these observations entirely from the analysis.

4 Empirical Strategy

4.1 Identification Strategy

In order to estimate the causal effect of a child's education on his/her mothers' attitudes towards domestic violence, we implement a regression discontinuity design based on the date of birth of respondents' first-born child. In particular, we compare attitudes of mothers whose first-born child was born in December 1986 or before (who should be exposed to the 5-year compulsory schooling regime) to attitudes of mothers whose first-born child was born in January 1987 or after (who should be exposed to the 8-year compulsory schooling law).[13] Given the close date of birth, we expect mothers of these children to be, on average, similar in their observable and unobservable characteristics.

We define "treatment" as a child's school attendance for at least 8 years.[14] If the treatment was a *deterministic* function of the child's date of birth, i.e. if a child's probability to receive 8 years of education increased from 0% to 100% at the cut-off, we would have a sharp RD design. However, the laws on compulsory schooling stipulate only a *minimum* number of school years to be attended and as will be shown in section 4.3, many children born prior to January 1987 completed 8 years of schooling as well. This leaves us with a fuzzy RD design in which treatment is a *probabilistic* function of the child's date of birth. The identifying condition is that the probability of receiving treatment increases discontinuously at the cut-off. If this condition is fulfilled, the average causal treatment effect can be estimated as a ratio of the discontinuity in the dependent variable at the cut-off to the discontinuity in the treatment variable (Imbens and Lemieux, 2008):

$$\tau_{FRDD} = \frac{lim_{x\downarrow c}\mathrm{E}[\mathrm{Y}|\mathrm{X}=x]-lim_{x\uparrow c}\mathrm{E}[\mathrm{Y}|\mathrm{X}=x]}{lim_{x\downarrow c}\mathrm{E}[\mathrm{W}|\mathrm{X}=x]-lim_{x\uparrow c}\mathrm{E}[\mathrm{W}|\mathrm{X}=x]} \quad (1)$$

where **Y** is the outcome variable, **W** is the treatment (8 years of schooling), **X** is the forcing variable (date of birth), and **c** is the cut-off (January 1987). As identified by Hahn et al. (2001), this ratio technically corresponds to an instrumental variable (IV) approach where being on either side of the cut-off (**X<c** or **X>c**; binary variable) is an instrument for receiving the treatment **W** (also a

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[13] Here we assume a 100% law enforcement and compliance with regard to both (1) child's age when starting primary school, and (2) child's school attendance until its compulsory education is finalized. We discuss possible consequences of an imperfect compliance in section 6.

[14] This technical definition of treatment is based on the fact that compulsory schooling was expanded from 5 to 8 years. More broadly, the treatment could be also seen as an externally imposed increase in education that exposes students to secular education well into their teenage years and that disproportionately affects disadvantaged groups.

binary variable). The average treatment effect is then estimated as the difference in **Y** at the cut-off **c** but only among compliers (i.e. those where either **X>c** and **W=1**, or **X<c** and **W=0**). In our case, this means comparing outcomes of mothers whose children were born in December 1986 and who completed less than 8 years of education to the mothers whose children were born in January 1987 and who received at least 8 years of schooling.

In order to estimate the fuzzy RDD treatment effect as described in equation 1, we need information on school attendance of children. This information is, however, unavailable for a substantial portion of the sample and it is missing in a non-random way. In particular, 2008 TDHS collected information on schooling for all household members residing in the same household as the respondent. Hence, if the respondent's first-born child was not living with her anymore, the schooling information was not collected. We find that the data on educational attainment is systematically missing along two dimensions: child's gender and family's socio-economic background. First, daughters are overrepresented among children with missing schooling information (girls 63% vs. boys 37%) even though our full sample is fairly gender balanced (girls 48% vs. boys 52%).[15] Presumably, the underlying reasons are that daughters traditionally move away from home upon marriage, and that age at first marriage is typically lower for women relative to men in Turkey. Second, we find that girls from lower socio-economic background are missing in our data disproportionately: while the fraction of girls with missing information decreases from 62% in the lowest wealth quintile to 29% in the richest wealth quintile, the fraction of boys with missing information remains fairly stable at 24-28%. Even though it is not surprising that girls from poorer families are missing in our data more often, because they are more likely to marry and to leave home early, this systematic unavailability of educational data poses a serious problem for our study design. The underlying reason is that girls from disadvantaged backgrounds are more likely to drop out of school at an earlier age and therefore, they are the ones who are expected to benefit the most from an education reform that expands compulsory education. In technical terms, the exclusion restriction is particularly binding for individuals who turn out to be disproportionately missing in our data. Finally, data unavailability is not only systematic but its extent is also substantial.

[15] The relevant sample in this case are individuals older than 14 years at the time of the survey. These are individuals who had enough time to finish 8 years of education, assuming timely enrolment and no grade repetition. When we look at individuals aged 14 years and younger, we find no indication that educational information is missing in a non-random way. In fact, the distribution of missing values corresponds to the general distribution by gender: girls represent 48% of children below 15 years and 47% of under-15 children with missing educational information. Hence, information seems to be missing in a random way in the younger cohorts (<15 years) whereas this is clearly not the case in the older cohorts (>14 years).

In the relevant sample, education goes unreported for 26% of first-born boys and 48% of first-born girls, which sums up to 37% of children having missing values.

Based on the fact that our treatment variable **W** is missing in a non-random fashion and it is unavailable for a large portion of the sample, any attempt to estimate a fuzzy RDD treatment effect or to take an IV approach would risk a substantial sample selection bias. In this situation, we decide to omit the endogenously missing educational variable **W** and to identify the treatment effect of education exclusively based on the child's date of birth **X**. Thus, we mimic a sharp RDD approach where the average treatment effect is identified solely by the discontinuity in the dependent variable **Y** at the cut-off **c**, see Imbens and Lemieux (2008):

$$\tau_{SRDD} = lim_{x\downarrow c}\mathrm{E}[Y|X = x] - lim_{x\uparrow c}\mathrm{E}[Y|X = x] \quad (2)$$

When taking the IV analogy, the proposed procedure is equivalent to estimating the reduced form instead of the structural equation. The reduced form parameter that we obtain is the product of the IV coefficient from the first stage (that we cannot estimate consistently due to non-randomly missing data) and the treatment coefficient in the second stage of the structural equation. Later on, in section 5.3, we will use a split-sample 2SLS methodology based on Inoue and Solon (2010) in order to estimate this structural estimation. Finally, given that in our case being assigned to treatment (i.e. being born in January 1987 or later) does not necessarily correspond to being treated (i.e. receiving 8 years of education), we identify intent to treat (ITT) rather than the average treatment effect (ATE) of education on mothers' attitudes towards domestic violence.

4.2 Econometric Approach

For the implementation of the RDD estimation, we adopt a local non-parametric approach where we use a subsample of observations lying within a certain "optimal" bandwidth around the cut-off and estimate a local linear regression.[16] We also present results from a local quadratic regression as a robustness check. To determine the "optimal" bandwidth, we use the algorithm proposed by Calonico et al. (2014); we refer to this bandwidth as CCT. In the robustness checks, we assess the robustness of the findings to the choice of bandwidth.

[16] In this non-parametric approach, rectangular and triangular Kernel density functions are most commonly used. Triangular Kernel function gives lower weight to observations further away from the cut-off. Rectangular Kernel function, on the other hand, gives the same weight to each observation. As a consequence, the estimated coefficients correspond to those from a simple parametric OLS regression which is run on a subsample determined by the optimal bandwidth.

In terms of the specific econometric model that we estimate, our preferred specification can be expressed by the following equation:

$$Y_i = \alpha + \tau T_i + f(X_i, T_i) + \varepsilon_i \qquad (3)$$

$$\forall\, X_i \in (c+h,\ c-h), T_i \equiv 1(X_i > c)$$

where Y_i are the domestic violence attitudes of respondent i, α is a constant, and ε_i is error term. The forcing variable X_i is respondent's first-born child's month of birth; the cut-off **c** is January 1987. Treatment T_i is a binary variable which takes the value 1 if the first-born child of the respondent **i** was born in January 1987 or afterwards, and 0 otherwise. $\boldsymbol{h}$ represents the CCT optimal bandwidth in our local regression. Control function $f(X_i, T_i)$ is linear. Finally, τ measures the treatment effect. In all our estimations, we cluster standard errors at the values of our forcing variable X_i to account for correlation of errors within 1 month of birth.

Even though RDD identifies causal effects without controlling for any covariates, incorporating them may: (1) help to eliminate bias coming from observations further away from the cut-off, (2) improve precision if they are correlated with the outcome **Y**, and (3) identify problems in the empirical strategy (Imbens and Lemieux, 2008). In particular, covariates in a valid RDD do not influence the identification strategy and the resulting treatment estimate τ_{RDD} because close to the cut-off they are independent of the treatment. If their inclusion leads to substantial changes in the estimated effects, the credibility of the identification strategy is compromised. Hence, inclusion of covariates serves as an additional test of internal validity. In our robustness checks, we will control for the respondent's year of birth (specified as a set of year fixed effects) for reasons that we discuss in the following section.

4.3 Validity of the Identification Strategy

Even though we cannot use schooling information of first-born children to obtain consistent fuzzy RDD estimates (due to this information being non-randomly missing), it is still necessary to show that the 1997 education reform in Turkey led to a discontinuous increase in schooling. In order to do that, we look at educational attainment of all household members in the 2008 TDHS. In this broader and representative sample, information on date of birth is available only on an annual basis. As shown by the red and blue lines in Figure 1, there is a clear jump in completion rates of 8-year schooling for all cohorts between birth year 1986 and 1987. In particular, we see that the completion rates are gradually rising for cohorts born up to 1986, then they increase discontinuously between 1986 and 1987 but they do not reach 100% even in cohorts fully affected by the new law. The latter must have been caused by an insufficient or ineffective enforcement of the 1997 education law. Another important aspect is that the discontinuity seems to be spreading over cohorts born in 1987-1989. This might have been caused by an imperfect compliance with the school-starting-age rule in early 1990s when the concerned cohorts started attending primary school. Other two potentially contributing factors are grade repetition and a (suspected) delay in implementation of the new law in remote areas of Turkey.[17] As a falsification test, we also plot completion rates of 5-year schooling in yellow and green, and show that completion rates in a schooling variable unaffected by the policy reform do not exhibit discontinuities at the cut-off. In addition to evidence presented in Figure 1, also other studies documented a sharp increase in schooling after the 1997 education reform in Turkey, see Cesur and Mocan (forthcoming), Dincer et al. (2013), Gulesci and Meyersson (2016), Günes (2015, 2016) and Erten and Keskin (forthcoming). All of these studies also report that females benefitted from the reform over-proportionately due to their lower educational status prior to the 1997 intervention. This is consistent with Figure 1 where we observe a closing gender gap in completion rates for cohorts born 1987 and later.

Having shown that the education reform in Turkey led to a substantial rise in education, particularly for girls, the next question is whether individuals could have manipulated their "treatment status". We do not expect any manipulation because (1) the new law was suggested in February and adopted in August 1997, (2) it applied as of school year 1997/1998, and (3) its implementation depended on which grade the child was attending in the running school year 1996/1997. Hence, there was little scope for parents to manipulate the treatment status of their child upon the announcement of the reform. To ascertain this expectation, we conduct the McCrary test that looks for a discontinuity in the density of the forcing variable at the cut-off (McCrary, 2008). Figure 2 shows no evidence of such discontinuity which is confirmed also by the McCrary test: the log-difference in density height at the cut-off is 0.039 with a standard error of 0.079. The resulting t-statistic of 0.493 implies that we cannot reject the null hypothesis that there is no discontinuity in the forcing variable at the cut-off. Thus, we conclude that there is no evidence of treatment manipulation.

The next step in assessing the internal validity of our RDD is to test if predetermined characteristics and covariates exhibit a discontinuity at the cut-off. Table A2 in the Appendix and Figure 3 document that, overall, women's background characteristics do not exhibit a significant

[17] What is also visible in Figure 1 is that completion rates seem to decrease in the youngest cohort. However, these are children whose calculated age is 15 years at the time of the survey. Hence, they might still be enrolled due to incompliance with school-starting-age or due to grade repetition, or their age might have been calculated inaccurately.

discontinuity at the cut-off, except for only 1 covariate: respondent's age. Table A2 shows that women whose first born child were born in or after January 1987 were on average 0.8 year younger than women whose first born child was born in December 1986 or before. In order to ascertain that our covariates are together continuous at the cut-off, we test joint discontinuity in a seemingly unrelated regression (SUR). We cannot reject the null hypothesis that the effect of treatment on all covariates is jointly zero (p-value 0.43). Nevertheless, in order to make sure that our results are not driven by the differences in respondent's age, we will present robustness of our estimates to controlling for this covariate in a flexible way (by controlling for respondent's year of birth fixed effects).

In all tests performed so far, we compared average values of covariates at the cut-off. This means that we looked only at the first moment of each covariate's distribution. In order to complement this evidence, we performed also Bartlett test of equal variances (second moment) and Kolmogorov-Smirnov test of equal distributions before and after the cut-off. The final two columns of Table A2 in the Annex summarizes the results. All covariates pass both the Kolmogorov-Smirnov and the Bartlett test with the exception of respondent's age which, however, has different distributions per construction.

5 Results

5.1 Main Results

First, we present our results regarding the causal impact of children's education on their mothers' attitudes towards domestic violence. Our main outcome of interest is a binary variable indicating that the respondent deems domestic violence acceptable in at least one of the seven situations presented to her – from now on referred to as the aggregate indicator. Table 2 shows results from our baseline specification, which is a local linear regression estimated non-parametrically with CCT optimal bandwidth and uniform Kernel function. We look at the full sample with all first-born children, and also at gender-specific subsamples of first-born daughters and first-born sons.

The first row of Table 2 shows that the average treatment effect of the reform on affected children's mothers' acceptance of wife-beating is –1.6 percentage points (ppt) and statistically insignificant. Apart from being imprecisely estimated, the effect size is economically not substantial. When we estimate the gender-specific effects, we get a very different picture. There is a substantial and statistically significant effect of girls' schooling on their mothers' attitudes. In particular, the acceptability of any type of domestic violence decreases by 16.8 ppt if the daughter was exposed to the compulsory schooling reform. This constitutes a 46% drop with respect to the average acceptability rate of domestic violence in the sample[18]. In the boys-sample, the effect is *opposite* in sign but imprecisely estimated at conventional levels. Figure 4 shows the corresponding RDD graphs where we plot the local linear estimates and control functions for the sample of first-born children, girls and boys. The sample sizes correspond to those determined by the CCT optimal bandwidth algorithm used in the regressions presented in Table 2. The figures confirm the patterns observed in Table 2 – there is no significant jump around the cutoff date for the full sample, while for the girls sample there is a large and significant fall and for boys there is an insignificant increase at the cutoff.

Having seen the aggregate treatment effects of children's education on their mothers' attitudes towards domestic violence, we now ask which particular components or situations are driving the result. The lower panel of Table 2 displays the treatment effects of children's education on the different components of the aggregate indicator. Overall, we note four important points. First, the pattern observed for the aggregate measure is roughly replicated in all its components – the overall effect it negative but relatively small and insignificant; girl's education has a strong, negative, and significant effect on acceptability of domestic violence in nearly all of the situations; for boys the effect is insignificant in all except 1 of the situations ("if the wife refuses to have sex") which suggests there is no discernible effect for boys. Second, the effects of girl's schooling are substantial in magnitude – for the specific components, the effect sizes of significant coefficients reach from 11.9 to 25.1 ppt. Third, acceptability of domestic violence decreases also in the overall "child" sample in two specific situations – if the wife "wastes money" and if she does not cook. The estimated effect sizes are important in magnitude (70% of the sample mean in the former and 88% in the latter case). Lastly, the only component with insignificant effects is "burning food". However, the reported acceptability of domestic violence in this situation is extremely low (3.3%) and therefore there is little variation to exploit. We also employ a seemingly unrelated regression (SUR) to test joint discontinuity of all components at the cut-off. Overall, we find that components pertaining to acceptability of domestic violence exhibit a joint treatment effect in the sample of all children (p-value 0.02) and in girls sample (p-value 0.06). The latter finding confirms the main treatment effect that we estimated for the aggregate measure in section 5.1. This is important because the previous analysis of the aggregate measure did not explicitly take into account that it is composed of a multitude of underlying variables.

[18] See the last column of Table 2 for sample means in the sample of women not affected by the reform whose children were born before January 1987 and within the CCT optimal bandwidth

To summarize, we showed that girl's education has an important and significant effect on her mother's attitudes towards domestic violence. The effects are found for both an aggregate measure as well as for individual components. The effect sizes reach from 11.9 to 25.1 ppt. In two out of seven listed situations, the effects were found also in the overall sample. The effects of boy's education are insignificant. This could either be because the effect of the reform on boys' schooling is weaker (as demonstrated by previous work on the effects of the reform) and/or because the mechanisms through which schooling affected children's mother's attitudes were at work for girls but not for boys.

5.2 Robustness Checks

As a robustness check, we first examine the impact of the bandwidth choice on estimated treatment effects in a local linear regression. In Figure 5, we plot estimates stemming from 17 different regressions where the bandwidth varies between 12 and 60 months in 3-month intervals around the cutoff date. Plotted are also optimal bandwidths from the main specification (blue vertical line) and the sample sizes (dashed line) that vary between 364 and 1750 observations in the full sample, and between 179 and 923 observations in the gender-specific samples. What we can see is that across the board, the point estimates for the treatment effect do not vary dramatically with the bandwidth size. For girls, the estimates are consistently negative and vary between -10ppt to -20ppt; and for boys' the estimates are always insignificant and vary between 0 and 10ppt.

The second set of robustness checks is presented in Table 3 for the aggregate attitude index and in Appendix Table A3 for the individual situations. The table(s) present the results of a number of variations in our regression approach. In particular, we:

a) calculate bias-corrected estimates as suggested by Calonico et al. (2014),
b) apply different Kernel functions (triangular and Epanechnikov),
c) control for a local quadratic function of the forcing variable,
d) use Imbens and Kalyanamaran (2014) optimal bandwidth algorithm to determine the bandwidth size,
e) estimate a segmented OLS regression on the CCT optimal bandwidth, [19]

[19] This means that we restrict the sample to the same set of observations as in our main specification and then we fit an OLS regression with different slopes to the left and to the right of the cut-off. The point estimate of this approach corresponds to the one of local linear regression with uniform Kernel function, i.e. to our main specification. However, the standard errors and therefore also confidence intervals and statistical inference do differ between the local linear regression (non-parametric approach) and CCT-sample OLS regression (parametric approach). If we apply other than

f) estimate the same specification as (e) controlling for the respondent's year of birth fixed effects,

g) estimate a global higher order polynomial OLS estimation where we use the full sample (no bandwidth restriction) and allow the control function to be a cubic, quartic, and quintic polynomial of the forcing variable (i.e. child's month of birth).

All these robustness checks are presented in Table 3 for the aggregate indicator of whether the respondents consider domestic violence acceptable under any of the situations she was asked about. Fro brevity we report the results only for the pooled and the girls samples (the results for the boys' sample and for the components of the index are shown in Appendix Table A3). Across the board, the treatment effects are fairly stable both in terms of magnitude and significance. The only variation where the effect for girls' is imprecisely estimated is when we use the Imbens and Kalyanamaran optimal bandwidth algorithm to select the bandwidth size (column 12). While the point estimate is -7ppt and imprecisely estimated, we cannot reject the null hypothesis that this coefficient is statistically equal to the -17ppt effect we find in column (2) with the CCT optimal bandwidth algorithm [20]. The fact that overall, the effect of girls' schooling on their mothers' attitudes towards domestic violence is robust to the choice of the estimation method, to the specific parameters within that method and to the inclusion of covariates builds confidence that the effect is stable and robust.

5.3 Two-Samples IV Estimation

The results thus far relied on a design that mimics a sharp RDD even though the true nature of our data is a fuzzy RDD. Ideally, we would like to estimate the effect of a daughter's schooling on her mother's attitudes towards domestic violence using a two-stage-least-squares (2SLS) framework. However, we are not able to estimate the first stage because the endogenous treatment variable (daughter's education) is not available for all observations. Moreover, as we discussed in section 4.1, the information is missing for a large fraction of the sample and in a non-random way. In general, systematic data unavailability is a problem as it leads to sample selection bias. Therefore, any estimates based on the inclusion of the schooling variable would be inconsistent.

uniform Kernel function, such as triangular or Epanechnikov, the results will differ from simple OLS because these Kernel functions give higher weight to observations closer to the cut-off.

[20] Calonico et al. (2014) argue that their algorithm is preferable to the Imbens and Kalyanaraman (2012) algorithm, as the latter tends to be too wide and leads to a larger bias than the CCT bandwidth; CCT bandwidth uses a local quadratic regression to construct a bias correction.

As an alternative, we use schooling data of the main respondents (i.e. ever-married women) in the 2008 TDHS to estimate the first stage parameter, i.e. the effect of the reform on schooling level of treated cohorts. Then, we apply the split-sample-IV or two-samples-2SLS estimation method (Inoue and Solon, 2010) to estimate the fuzzy RDD estimate. The method builds on the fact that in a 2SLS framework, the reduced form parameter is the product of the IV coefficient in the first stage and of the treatment coefficient in the second stage. Given this relationship, it is possible to obtain the structural equation parameter if the reduced form and the first stage estimates are available, even if they are estimated in different samples. More specifically, the reduced form can be estimated in one data set, the first stage in another data set, and the structural equation parameter can be then calculated as the ratio of these two coefficients.

Table 3 displays the results. The reduced form point estimate in the first panel comes from the first sample and it is therefore identical to the one estimated in Table 2. The second panel shows the first stage stemming from the second sample, i.e. regressions where the effect of the treatment variable (respondent was born in January 1987 or later) on four measures of education is estimated [21]. In particular, the variables measuring education are: (1) binary variable equal to one if the respondent completed at least 8 years of schooling, (2) years of schooling, (3) years of schooling capped at 8 years (i.e. 0–8), and (4) years of schooling capped at 13 years (i.e. 0–13).[22] Given that the instrument (respondent's date of birth) is arguably exogenous, has a positive and highly significant effect on all measures of education, and the first stages have a reasonably high R-squared, the IV approach is valid. The resulting 2-sample-2SLS estimates in the third panel display the expected signs and are statistically significant. The point estimates in the first column (completed 8 years of education) are large because the non-treated women in the reference group

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[21] One caveat with this analysis is that our first stage estimation is based on a sample of women who married relatively early because the 2008 TDHS sample is limited to "ever-married" women and respondents born around the cutoff date (January 1987) are approximately 21 years old at the time of the survey in 2008. As such, they are women who were married earlier than the average woman in the country. In line with this, the average age at first marriage in the full sample is 19.8 while in the estimations sample (of ever-married women born ±36 months around the cutoff date) it is 17.9. As women who are married early tend to come from more conservative, typically rural communities, the effect of the reform are likely to be larger on them (see Gulesci and Meyersson (2016) for a detailed discussion).

[22] The third and fourth measure of education cap years of schooling at 8 and 13 years, respectively, in order to take into account that younger respondents did not have a chance (enough time) to complete as many years of education as their older counterparts. For the same reason, the first three measures are defined only for respondents 15 years or older (who were able to complete 8 years of schooling) and the last measure is defined only for women 20 years or older (who were able to complete 13 years of schooling).

have often 5 years of primary education or less. Thus, the coefficient measures the impact of several additional years of schooling. Columns 2-4 present effects of the reform on different variations of years of schooling. Qualitatively, the results do not differ from those in the first column. In terms of magnitudes, they are very similar to those estimated in our main local linear specification in Table 2. Thus, the 2-sample-2SLS regression results confirm that our sharp RDD approach is a valid approximation of the fuzzy RDD.

5.4 Falsification Tests

In order to prove that our results are not a mere artifact of data and to further validate our identification strategy, we run a set of falsification tests where we define placebo cut-off dates in points in time other than January 1987. More specifically, we create placebo RD designs by moving the cut-off back in time in 3-month intervals. It has to be noted that this approach has two disadvantages: (1) relevant characteristics of respondents change, particularly their own and their children's age increase as we move back in time, and (2) observations to the right of the false cut-off are a mix of treated and untreated individuals which can lead to problems such as detecting fake and counterintuitive "treatment" effects, especially relatively close to the real cut-off. We estimate 20 placebo regressions for each outcome, shifting the cut-off back in time in 3-month intervals. Hence, the most extreme placebo cut-off is 60 months, i.e. 5 years, prior to January 1987. Figure 6 graphically summarizes the results of these placebo regressions, both in the overall sample and in gender-specific subsamples. In all graphs, the x-axis captures the distance of the fake cut-off from the real one: zero corresponds to the true cut-off and negative values represent how many months prior to January 1987 the false cut-off was set. The y-axis plots the resulting "treatment" effects and 95% confidence intervals around them. Across the board, we see that (1) false treatment effects are extremely volatile in both their magnitude and sign, (2) there is no systematic pattern in the treatment effects neither over time nor across gender, and (3) the vast majority of false treatment effects are statistically indistinguishable from zero. Regarding the last point, it should be noted that some false treatment effects turn out to be statistically significant. However, the number of these cases is low – 0, 1, and 1 in the overall, girls, and boys sample, respectively. Given that in each graph we test 20 false treatment effects at significance level of 5%, we do expect that on average one of them will come out as statistically significant just by chance [23]. We therefore conclude that the falsification tests help corroborate the validity of our identification strategy.

[23] Moreover, in the one placebo test that yields a significant results for the girls sample, the effect goes in the opposite direction to our main findings (younger girls' mothers are more likely to find domestic violence acceptable) and we have no explanation for why there may be such an effect at the given cutoff date.

5.5 Examining Potential Mechanisms

As summarized in the introduction, the literature on upward intergenerational spillover effects is limited, but existing studies on the topic as well as the broader literature on domestic violence suggest a number of potential mechanisms through which the change in compulsory schooling and the resulting increase in girls' education level may have affected their mothers' attitudes towards domestic violence in Turkey.

First, teenage children may affect their family life [24], and to the extent that additional schooling acquired by children due to the reform changed their own attitudes towards domestic violence, they may actively engage with their mothers in order to try and amend their positions – we call this the "active persuasion" channel. However, while evidence from other countries suggests that education may influence girls' attitudes towards domestic violence (Friedman et al. 2015), Erten and Keskin (forthcoming) find that the reform in Turkey did not lead to a significant change in attitudes towards domestic violence among women in treated cohorts. We test the same hypothesis, using the 2008 TDHS data. One caveat with this analysis is that it is based on a sample of women who married relatively early [25]. Despite this caveat, we find similar effects to Erten and Keskin (forthcoming). The results in Table 5 shows that the treatment effect on the aggregate indicator is –0.097 and imprecisely estimated. The point estimate is much smaller than the effect we observed on mothers' attitudes towards domestic violence.[26] This, and the fact that Erten and Keskin (forthcoming) also fail to find a direct effect of the education reform on women's own attitudes towards domestic violence suggests that the effect we find on mothers' attitudes cannot be mainly explained by an "active persuasion" mechanism.

A second potential mechanism is based on a burgeoning literature in economics that studies the relationship between women's bargaining power and domestic violence. In theoretical models of

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[24] Previous work has shown that teenage sons and both teenage and adult daughters influence decision-making in British households (Dauphin et al., 2011), and that Indian women with small children are more patient than other women and any men (Bauer and Chytilova, 2013). Lastly, Washington (2008) shows that US legislators with daughters vote more women-friendly on reproductive rights; and Warner (1991) and Warner and Steel (1999) find that US and Canadian parents with only daughters are more likely to hold feminist views.

[25] See footnote 21 for a more detailed discussion of this issue.

[26] When we look at the individual situations that the women were asked about, we do find a marginally significant negative effect on respondents' likelihood to find domestic violence acceptable in 2 out of 7 situations (if the woman wastes money and if she argues with her husband). One reason for this slight difference between our results and the findings of Erten and Keskin (forthcoming) may be due to the fact that our sample consists of ever-married women, while theirs includes women who ever had a relationship.

household bargaining, spousal violence can be modeled as an instrument through which the abuser increases his bargaining power. In such a framework, an improvement in women's access to economic opportunities (such as employment or earnings) may decrease or increase the incidence of domestic violence, depending on the initial allocation of bargaining power within the couple and whether the reservation utility of the woman or her spouse is binding (Tauchen et al., 1991; Eswaran and Malhotra, 2011; Bloch and Rao, 2002; Anderson and Genicot, 2015). This implies that a change in the economic opportunities of a woman relative to her husband may affect the incidence of domestic violence [27]. Therefore, if girls' schooling had any indirect effects on their mothers' labor market outcomes, this could affect their bargaining power within the household and affect whether or not they expect and/or tolerate domestic violence. This may happen, for example, if the increase in compulsory schooling alleviated the need for women to provide childcare and enabled them to get or obtain full-time employment. Moreover, in the context of the reform we study, Gulesci and Meyersson (2016) and Erten and Keskin (forthcoming) show that women who got higher schooling due to the reform were more likely to be working in the non-agricultural sector and more likely to be self-employed. Therefore, it is likely that treated girls may have encouraged and enabled their mothers to get jobs in the non-agricultural sector or in different occupations.

To test for this mechanism, we rely on a module in the TDHS that collected information on surveyed women's current employment status and their retrospective employment history (for every job that they worked in for at least 6 months, the survey recorded their sector of employment, role/position, duration of employment and the main reason for leaving the job). In Table 6, we report the effects on the respondents' relevant labor market outcomes, using the same RDD strategy as our main specification where the age of the first-born child (daughter) is the running variable. In particular, we test if respondents whose daughters were treated are more likely to be employed, their type of occupation (as an unpaid family worker or self-employed), the cumulative duration of employment throughout their lives (number of years for which they have worked), and whether they ever had to leave a job in order to take care of children in the household. We do not find statistically or economically significant effects on any one of these outcomes. As such, we

[27] Empirical literature testing these predictions have studied how employment or earning opportunities of women may influence the incidence of domestic violence across a variety of settings (Aizer, 2010; Alesina et al., 2016; Andenberg et al., forthcoming; Chin, 2013; Heath, 2014; Angelucci, 2007; Blattman et al., 2013; Bobonis et al., 2013; Amaral et al., 2015; Anderson and Genicot, 2015; Heise and Kotsadam, 2015). Broadly speaking, the evidence suggests that an increase in women's bargaining power reduces domestic violence in high-income settings, while it leads to an increase in domestic violence in low-income countries.

conclude that an indirect "economic empowerment" channel through which girls' schooling may have helped to improve their mothers' labor market outcomes does not seem to be driving the effect on their mother's attitudes towards domestic violence.

A third potential mechanism is related to "parental empathy". The psychology literature on the topic has shown that parents, in particular mothers (Goubert et al. 2008), tend to feel distressed if they imagine or observe their children in painful situations (Goubert et al. 2006, 2008; Caes et al. 2012, 2014) [28] [29]. Erten and Keskin (forthcoming) show that the compulsory schooling reform we study led to an increase in women's likelihood to be working in the non-agricultural sector, thus increasing their economic power, but this was faced with a backlash from their husbands and with an increase in (psychological) domestic violence as well as financial control behavior of the husbands. They also show that the effects were stronger for women who grew up in rural areas. To the extent that the mothers of affected women observe their daughters being mistreated by their husbands, this may make them more likely to change their attitudes towards domestic violence and make them more likely to find it unfair and unjustified. Unfortunately, we cannot test for this mechanism directly, since the survey data we use does not contain any direct measures of mothers' experiences of their children's domestic violence. We can, however, assess the heterogeneity of the effects in order to shed light on which mothers are driving the effects and whether the pattern is consistent with what the parental empathy mechanism would suggest. In particular, if this is the relevant mechanism that is driving the results, we expect the effects to be driven mainly by mothers whose daughters are married at the time of the survey. However, the survey did not collect information on the marital status of respondents' all children. We only have information on whether the child is living in the same household as the respondent, and for those who do (live in the same household as the respondent) we know their marital status. Here, we exploit a social norm in Turkey whereby most children, and in particular female children, tend to reside in their parental home until they get married. Therefore, it is reasonable to assume that daughters who are living in another household are more likely to be married. Based on this, we define a child to be "married"

[28] Another important correlate of parental empathy tends to be "parental catastrophizing", which refers to parents' tendency to overemphasize the pain of their children and think of it as being more catastrophic than it may actually be in reality (Goubert et al. 2006). Parental empathy tends to be particularly strong among individuals with greater tendency for "parental catastrophizing" (Goubert et al. 2006, 2008; Caes et al. 2012, 2014).

[29] This is also related to a growing literature in economics and political science showing that exposure to violence due to war or crime can affect individuals' preferences (Bauer et al (2016), Rojo-Mendoza (2014), Voors et al (2012)). These studies typically do not distinguish between direct and indirect experiences of violence, but estimate the effect of *any exposure* (whether it is experienced directly by the individual, or at the household/community level) on preferences related to time, risk, political participation or social cohesion.

if (i) she does not live in the same household as the respondent (ii) she lives in the same household but she is married. If the parental empathy mechanism is the one driving the results, we expect the effect to be greater on mothers whose daughter are "married" according to this classification. Moreover, we also expect the effect to be more pronounced in rural areas for two reasons: (i) the compulsory schooling reform had a stronger impact on women's education in these areas (Erten and Keskin (forthcoming) (ii) we expect mothers to be more likely to be informed of their daughters' lives in rural areas compared to urban regions on average due to, for instance, tighter social networks that exist in rural areas.

Table 7 reports the results of the heterogeneity analysis along these two dimensions. First, in columns 1 and 2 of Panel A, we divide our main estimation sample (36 month bandwidth around the cutoff date) into respondents who live in rural areas versus non-rural areas at the time of the survey [30]. The treatment effect is -38ppt for respondents from rural areas, while it is only -13ppt for respondents who reside in urban areas. Next, we assess whether the effects are stronger for mothers whose eldest daughters are classified as "married" according to our classification discussed above. Column 3 (4) of Panel A shows the effect for mothers whose eldest daughter is "married" ("unmarried"). The point estimate for respondents whose eldest daughters are "married" is -26ppt and significant at 95% confidence level, while for the rest, the effect is -15ppt and imprecisely estimated. Finally, in Panel B of the table, we present the treatment effects for the four subsamples that result from the interaction of these two dimensions. The results show that the treatment effects is large and significant (-41ppt) for respondents who reside in rural areas and whose eldest daughters are "married", while for the rest the effects are imprecisely estimated. Overall, the results in Table 7 are in line with the "parental empathy" mechanism whereby respondents who are more likely to observe their daughter's interaction with their son-in-law are the ones more inclined to

[30] Ideally, we want to use an indicator for whether the child lived in a rural area when she was 11-12 years old, since the reform affected children of that age in 1997 and the effects were heterogeneous depending on where they lived at that moment in time (Erten and Keskin (forthcoming). In the absence of this information for children of the respondent, we use the respondent's residence at the time of the survey as a proxy for it. Another alternative would be to use an indicator for whether the respondent's place of birth or childhood was rural, but this is likely to be a noisier proxy for her eldest daughter's residence at age 11-12, especially because in Turkey rural-urban migration rates have been very high in recent decades. One concern with using respondent's current residence is that this could be endogenous to the treatment (i.e. respondents whose eldest daughters got more schooling due to the reform may be more or less likely to reside in rural areas in 2008 when they're surveyed) but the results in Appendix Table A4 show that this concern does not seem to be relevant. In particular, respondents whose eldest daughter were exposed to the new compulsory schooling regime are not significantly more (or less) likely to have changed their place of residence in the 5 or 10 years preceding the survey compared to the control group.

change their attitudes towards domestic violence. However, given the small sample sizes we use for the subsample analysis, these findings should be interpreted as suggestive rather than conclusive evidence.

Lastly, we also estimated the treatment effects of girls' schooling on their mothers' opinions about gender roles. In particular, the 2008 TDHS contained 9 statements related to gender norms and the respondents were asked if they agree or disagree with these statements[31]. We tested if mothers of treated girls were less likely to agree with gender-biased statements and scored differently on an aggregate index (whether they expressed a gender-biased opinion on any one of the 9 statements) compared to mothers of non-treated girls. Appendix Table A5 shows the results. Out of the 10 indicators related to the individual statements and the aggregate index, we find a significant treatment effect on only one ("men are wiser than women"). Moreover, when we test the joint significance of these using a SUR specification, we fail to reject that the joint effect is equal to 0 (p-value=0.304). This implies that the increase in girls' schooling caused by the reform did not have a considerable effect on their mothers' attitudes towards gender norms in general. Therefore, we cannot say that the effect on their attitudes towards domestic violence was due to a general shift in their gender norms. It seems more likely that it was a reaction to the increase in their daughters' exposure to domestic violence.

6 Discussion

In this section, we discuss a number of issues that are important to highlight for the interpretation of the findings presented above. First, we reflect on the decision to include only first-born children in our analysis. As explained earlier, we compare women whose first-born child (daughter or son) was exposed to the education reform to women whose first-born child was not exposed. Limiting the sample to first-born children has the following consequences: If the first-born children were exposed to the reform, so were their younger siblings. If, on the other hand, the first-born children were not exposed, then their younger siblings may or may not have been exposed. This depends

[31] The specific statements were "The important decisions in the family should be made only by men of the family.", "Men should also do the housework like cooking, washing, ironing, and cleaning.", "A woman shouldn't argue with her husband even if she disagrees with him.", "A married woman should work outside the home if she wants to.", "It is better to educate a son than a daughter.", "A woman may go anywhere she wants without her husband's permission.", "Men are wiser.", "Women should be more involved in politics.", and "Women should be virgins when they get married."

on how far the first-born child's birth date is from January 1987 and how big the birth intervals between the first-born and later-born children are. If the untreated first-born children were born close enough to the cut-off and/or if the birth interval was sufficiently large, then all of their younger siblings may have been treated. Since we estimate the discontinuity *precisely* at the cut-off, the resulting coefficient represents a marginal effect of having *one less child* educated for 8 years. For example, when looking at a treated and untreated family with four children each, we are effectively comparing the effect of having 100% of children exposed to 8-year compulsory schooling as opposed to having 75% of children exposed. Hence, any treatment effect that we detect comes from the 25% exposure rate. In this sense, we are estimating a lower bound of the true treatment effect in all families except for those with one child.[32] In our sample, the average number of children ever born is 2.8 which means that the average exposure rate is 1 out of 2.8 or 36%. If we assumed that the treatment effect is linear in number of children exposed, the "full" treatment effect should be 2.8 times larger than what we estimated. However, it is not intuitive to assume a linearity in this respect as first-borns may have a larger impact on their parents' lives than later born children. Therefore, we maintain that our treatment effects are lower bound estimates and the true impacts lie between 1 and 2.8 times the estimated coefficients. Ultimately, our lower bound claim is supported also by Maruyama's et al. (2012) finding that the higher is the number of children in a family that are exposed to health-related information at school, the higher is the probability that they will transmit the information to their parents who will then act accordingly to this newly acquired knowledge.

Second, we discuss consequences of an imperfect compliance with the education reform. This happens if not all children born to the right of the cut-off are exposed to at least 8-years of schooling. And in fact, this is what we saw in Figure 1 – completion rates increased discontinuously at the cut-off but they did not reach 100%. Additionally, due to the fact that staying in school for 8 years was possible (and fairly common) on a voluntary basis also prior to the reform, observations to the left of the cut-off are a mix of treated and untreated individuals. Nevertheless, the probability of treatment differs at both sides of the cut-off. This set-up calls for implementation of a fuzzy RDD. As already mentioned, we cannot implement fuzzy design due to non-randomly missing educational data. However, we have shown in section 5.3 that our sharp RDD is a valid approximation of fuzzy RDD. In addition, the 2-samples-2SLS results indicate that the point

[32] We attempted to estimate the "exact" treatment effect in one-child families only. Despite the fact that 22% of women in our sample have had only one child, the vast majority of these women are young mothers with continuing fertility. The estimation was ultimately not possible due to sample size of 26 children to the left of cut-off, i.e. only 26 women in our sample had an only child that was born prior to January 1987.

estimates in our main specification (Table 2) are correctly estimated also in terms of their magnitude.

Third, we reflect on the possibility that the reform was not implemented immediately in all regions of Turkey. Due to the short time window between approving the law (August 1997) and its intended implementation (September 1997), it is possible that not all schools adopted the law as of the 1997/1998 school year. In particular, remote regions and rural areas might have been slower in receiving the information, in spreading it to parents, and ultimately in implementing the law. If this was the case, we would again be underestimating the true impact because the jump in the beneficial effect would not be entirely accumulated at the cut-off but rather, it would be spread out to the right of the cut-off. This is true if we assume both homogenous and heterogeneous treatment effects. In the latter case, it is reasonable to expect that remote regions, where the law was potentially implemented later, are more traditional and hence initially worse off in terms of mother's outcomes. At the same time, they could see larger improvements due to the reform as compared to the more central (and potentially more advanced) regions.[33] In order to see whether there is evidence supporting gradual implementation of the education reform – which would mean an immediate effect in central areas and a postponed effects in remote areas – we examine whether the discontinuity in 8-year-schooling completion rates differs by region in Figure 7. What we find is that (1) the reform seems to be similarly binding in all regions except for the least developed East, and (2) surprisingly, most rural areas seem to implement the new law as quickly and effectively as the urban areas, thus experiencing a bigger "jump" at the cut-off due to their lower pre-reform completion rates.[34] Overall, the jumps seem to be reasonably similar across the whole country and therefore we conclude that if we happen to underestimate the true treatment effect, we do so only to a small extent. In a related spirit, we look at 8-year-schooling completion rates by socio-economic background, also in Figure 7. While all wealth quintiles are affected, poorer families benefit more in the sense that the increase in their completion rates is the highest. On the other hand, however, the reform is less binding for them as the completion rates in the poorest quintile reach only around 70% post-reform.

[33] The only case in which we do not underestimate the true effect is if the remote areas were *substantially* worse off in terms of mother's outcomes prior to implementation of the reform and if the improvements in these outcomes due to the reform were *extraordinarily* large.

[34] Note that East is not only the least developed region but it also has a large Kurdish minority. Kurdish population tends to drop out from school earlier, either for economic reasons or because the language used in schools is Turkish.

Another important issue is that of validity and generalizability of the estimated effects. Generally, if the RDD is valid, the resulting treatment effects have a high degree of internal validity but the degree of external validity and potential for extrapolation are limited. In our case, we are confident that our identification strategy is valid as we are not aware of any parallel policy changes that would impact differently individuals born one month apart at the end of 1986. Additionally, due to the "retroactive" nature of the education reform we do not expect any manipulation of treatment status. Nevertheless, we see that compliance was not 100%, particularly in the least developed region of Turkey. In terms of external validity, it is important to keep in mind the specific context in which the reform was implemented. On one hand, Turkey is a majority-Muslim country so that traditional gender norms might be more prevalent there than in countries in other regions. On the other hand, it is a fairly secular state within its own region. In this sense, Turkey is a specific case and replicability of the herein documented effects of girls' education on their mothers' attitudes towards domestic violence may not be straightforward in other majority-Muslim countries.

7 Conclusion

In this study, we examined the question of whether the education of the younger generation has spillover effects on their parents' attitudes. In particular, we exploited a reform in the compulsory schooling law in Turkey that increased legal requirement of schooling from 5 to 8 years to test if mothers whose first-born child received more education because of the reform have different attitudes towards domestic violence. Previous literature has shown that this very reform has increased women's labor force participation (in the non-agricultural sector) but also led to a rise in the incidence of (psychological) domestic violence in their households (Erten and Keskin, forthcoming). Our results show that the increase in girls' schooling caused by the reform made their mothers less likely to find domestic violence acceptable. The estimated effects are of substantial magnitude, and robust. Moreover, the effects are found only for girls and not for boys who were affected by the reform. This is consistent with previous work that has shown that the reform has a stronger impact on girls' schooling. When we assess the potential mechanisms behind the effect, we find evidence that suggests that "parental empathy" is what's driving the result. In particular, mothers whose daughters experienced more domestic violence (due to their increased schooling) are more likely to change their views towards domestic violence.

Overall, the results are very relevant because they show that improvements in education, in particular in girls' education, may have significant impacts that go beyond the targeted generation of girls and affect the older generation as well. Previous literature has demonstrated spillover effects of girls' education on younger generations (e.g. child health) but as far as we are aware, this is the first paper to demonstrate an upward intergenerational spillover effect. Moreover, our findings suggest that parental empathy can be an important mechanism through which children's experiences may influence their parents' attitudes. Future work on cultural transmission and intergenerational spillover effects should study this mechanism more seriously.

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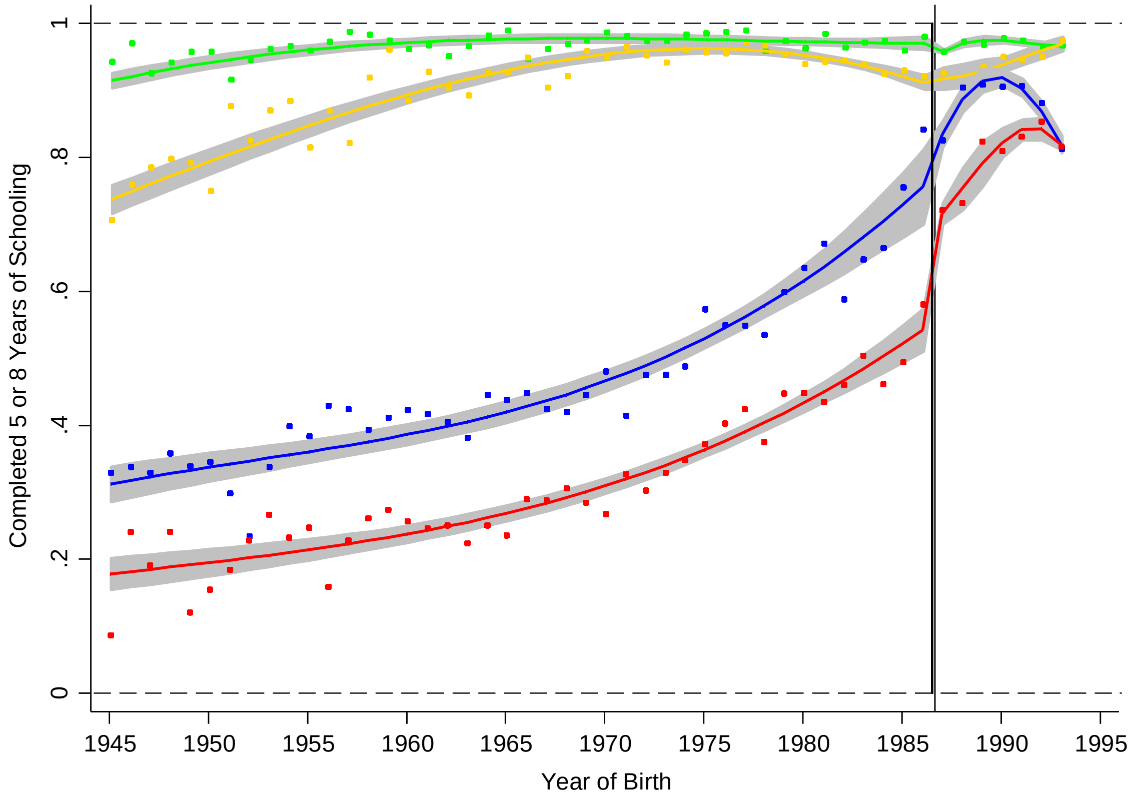
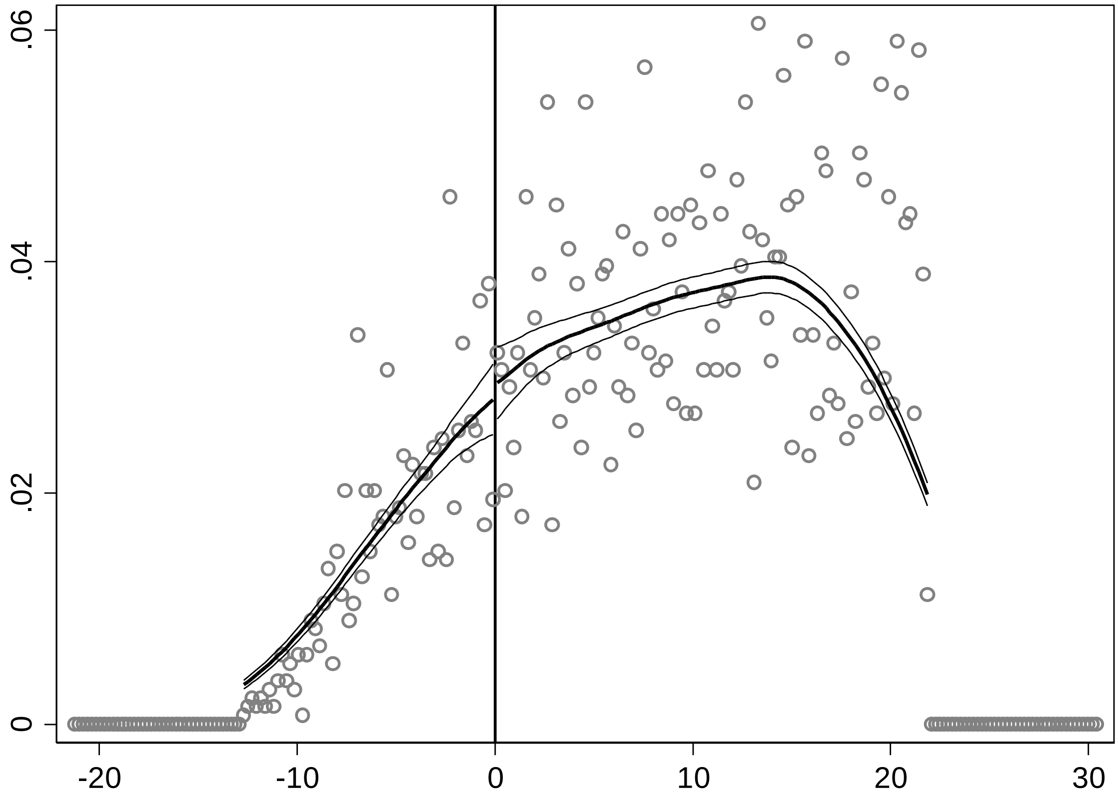
9 Figures

Figure 1: Proportion of household members who have completed at least 5 or 8 years of education, by gender and year of birth



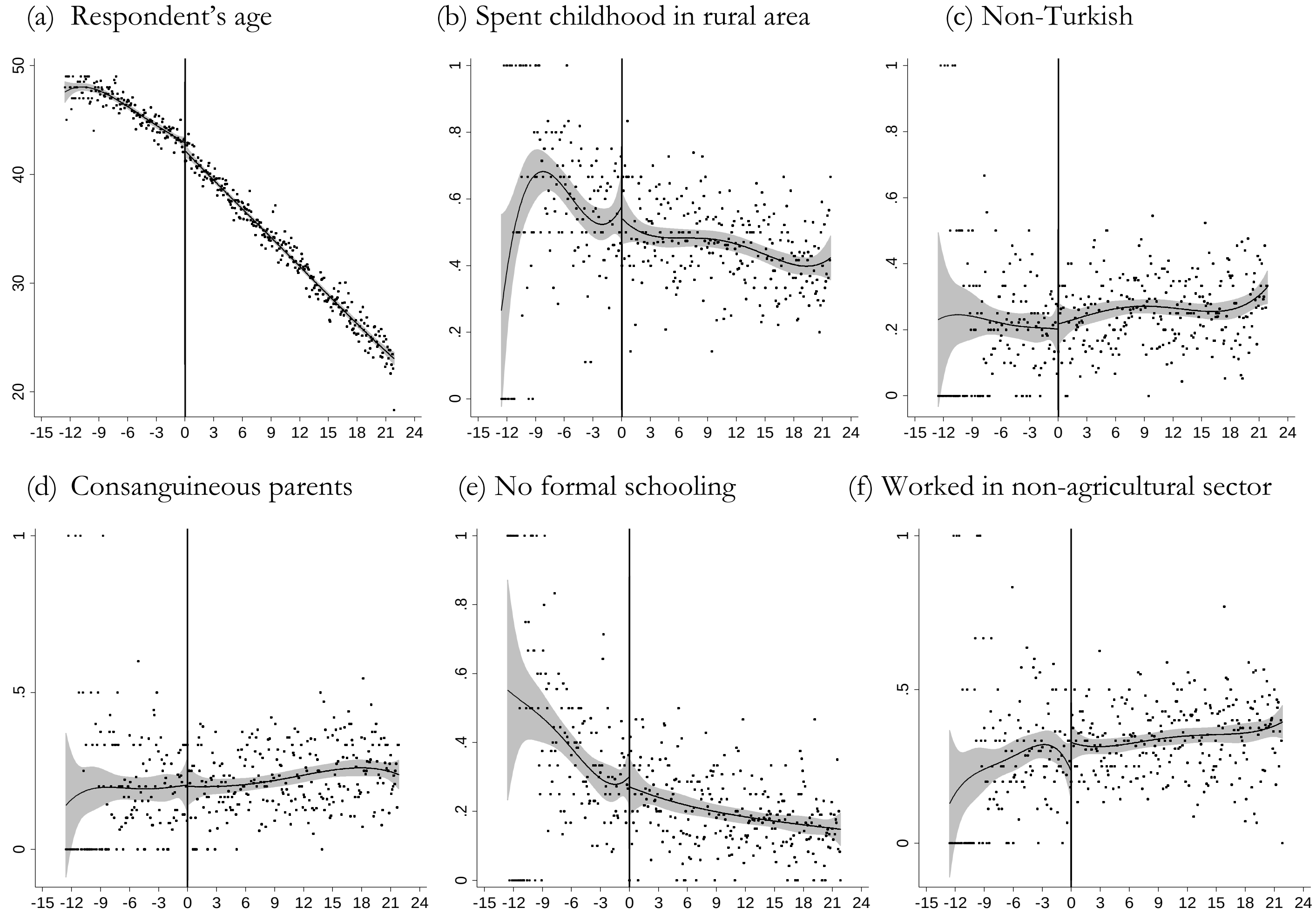
Notes: Red and blue color represents women and men with at least 8 years of education, respectively. Yellow and green color denotes women and men with at least 5 years of education, respectively. Dots represent fractions of individuals who went to school for a given number of years; the fractions are calculated on an annual basis by individual's year of birth. Lines are predicted probabilities to have completed at least 5 or 8 years of schooling stemming from a simple linear probability model (OLS regression) that includes gender and a third order polynomial in year of birth. The regression is segmented into (1) years up to 1986 and (2) years 1987 and later; the control function for year of birth is allowed to differ by segment and by gender. Grey areas represent 95% confidence intervals corresponding to the predicted probabilities. Standard errors are clustered at the year level. Sample covers all 26,504 household members in the 2008 TDHS who are older than 14 years. Year of birth is calculated as the difference between year 2008 and individual's reported age; interviews were conducted between October and December 2008. For presentational reasons, birth years 1913-1944 are not shown in the graph.

Figure 2: McCrary test of discontinuity in the forcing variable at the cut-off



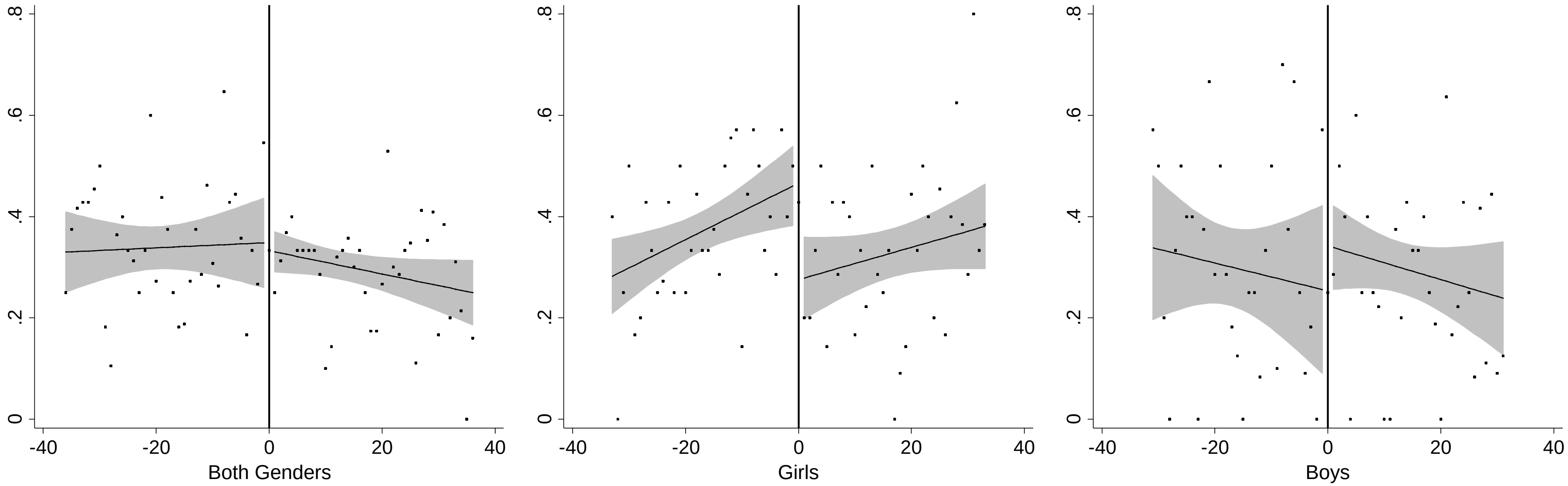
Notes: The figure displays the density function (y-axis) of the forcing variable (x-axis) with 95% confidence intervals. The forcing variable is the difference between individual's month of birth and January 1987, measured in months. The log-difference in density height at the cut-off is 0.039 with a standard error of 0.079. Based on the resulting t-statistic (0.493) we cannot reject the null hypothesis that there is no discontinuity in the forcing variable at the cut-off. Estimation based on 6,277 observations.

Figure 3: Individual tests of discontinuity of covariates at the cut-off



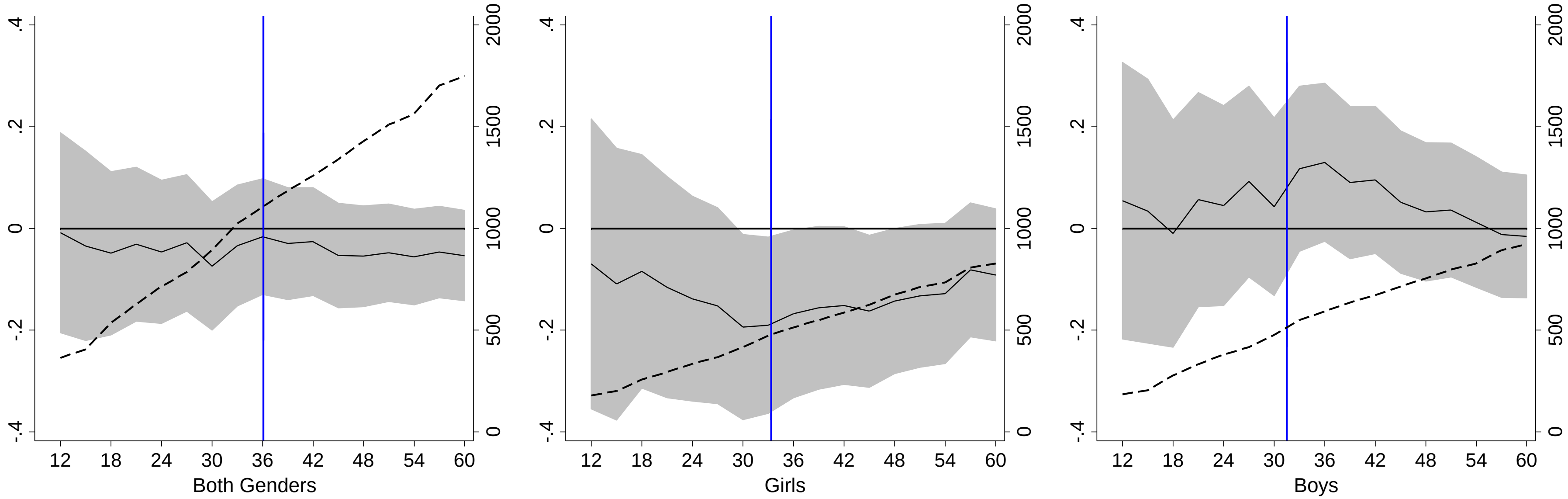
Notes: The figure displays the average values of covariates along with predicted values from a quartic polynomial regression with 95% confidence intervals (y-axis) by the forcing variable (x-axis). Covariates shown in the upper panel are respondent's age, whether the respondent spent her childhood in a rural area, and respondent is non-Turkish; lower panel shows variables indicating that parents are relatives, respondent has no formal schooling, and respondent ever worked in the non-agricultural sector. Forcing variable is the difference between child's month of birth and January 1987, measured in months but displayed in years.

Figure 4: Reduced form effects on mothers' attitudes towards domestic violence



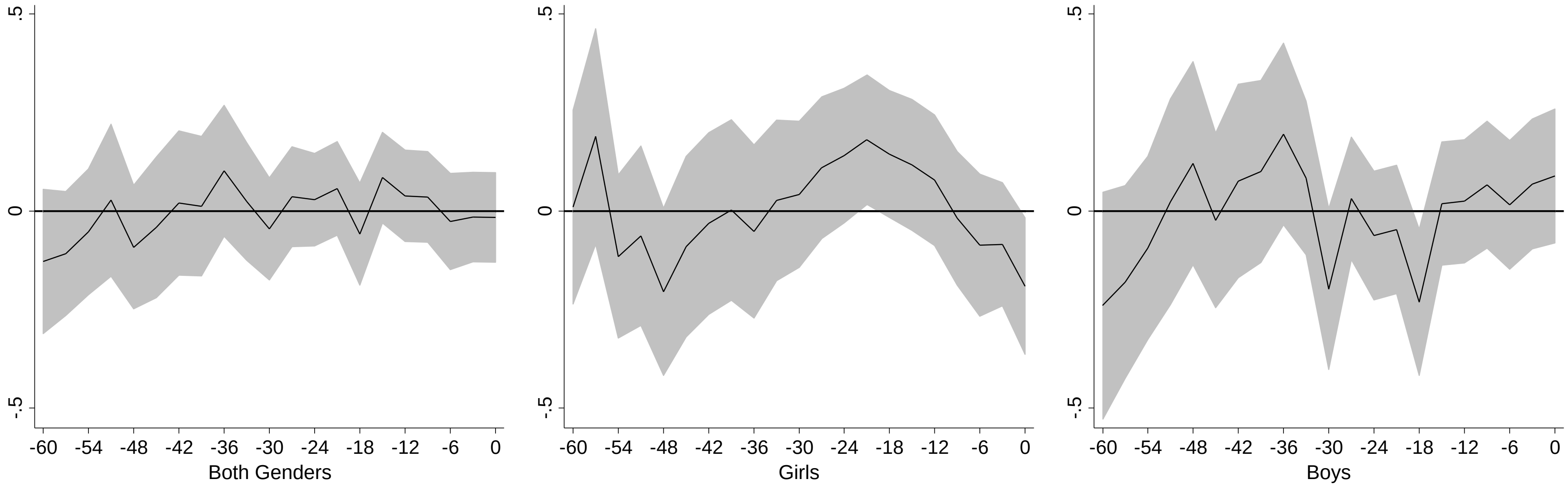
Notes: Dots represent fractions of respondents who deem domestic violence acceptable in at least one of the seven listed situations (x-axis) by the forcing variable (y-axis); fractions are calculated on a monthly basis by month of birth of respondent's first-born child. Lines are predicted probabilities stemming from a simple linear probability model (OLS regression) that is estimated locally within the corresponding CCT optimal bandwidth. Grey areas represent 95% confidence intervals corresponding to the predicted probabilities. Standard errors are clustered at year level. Graphs correspond to estimations shown in Table 2. Forcing variable is the difference between child's month of birth and January 1987, measured in months.

Figure 5: Treatment effects with varying bandwidth



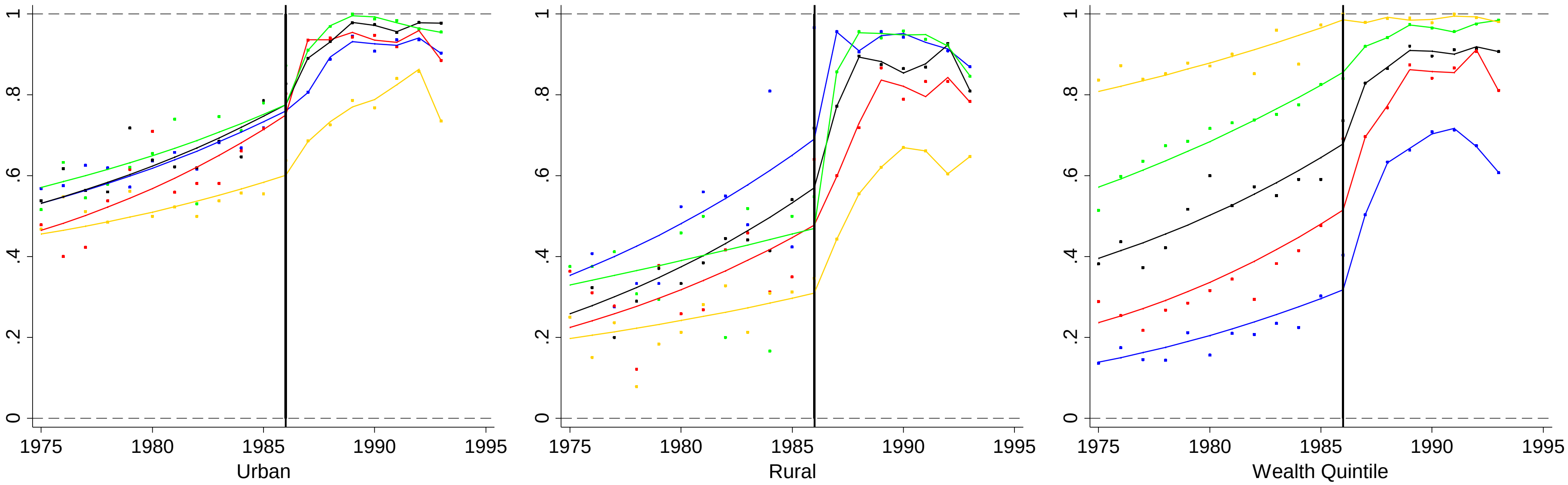
Notes: Solid lines (left y-axis) show how treatment effects on our main outcome of interest (whether domestic violence is deemed acceptable by the respondent in at least one of the seven listed situations) change with varying bandwidth (x-axis). Estimates stem from 17 local linear regressions with uniform Kernel function where the bandwidth varies between 12 and 60 months in 3-month intervals. Grey areas represent 95% confidence intervals. Plotted are also optimal bandwidths from the main specification in Table 2 (blue vertical line). Dashed lines represent sample sizes corresponding to each estimation (right y-axis).

Figure 6: Falsification tests with false cut-offs



Notes: Graphs depict placebo treatment effects on our main outcome of interest (whether domestic violence is deemed acceptable by the respondent in at least one of the seven listed situations) stemming from 20 regressions where the cut-off is shifted back in time in 3-month intervals. The x-axis measures the distance of the fake cut-off from January 1987 in months. Grey areas represent 95% confidence intervals.

Figure 7: Proportion of household members who have completed at least 8 years of education, by region, wealth, and year of birth



Notes: Colors represent regions in Turkey or wealth quintiles as follows: blue = West (including Istanbul and Izmir) or poorest quintile, red = South or second quintile, black = Central (including Ankara) or third quintile, green = North or fourth quintile, yellow = East or richest quintile. Dots represent fractions of individuals who went to school for at least 8 years; the fractions are calculated on an annual basis by individual's year of birth. Lines are predicted probabilities to have completed at least 5 or 8 years of schooling stemming from a simple linear probability model (OLS regression) that includes gender and a third order polynomial in year of birth. The regression is segmented into (1) years up to 1986 and (2) years 1987 and later; the control function for year of birth is allowed to differ by segment and by gender. Standard errors are clustered at year level. Sample covers all 26,504 household members in the 2008 TDHS who are older than 14 years. Year of birth is calculated as the difference between year 2008 and individual's reported age; interviews were conducted between October and December 2008. For presentational reasons, birth years 1913-1974 are not shown in the graph.

10 Tables

Table 1: Descriptive statistics of women in the estimation sample

	Child	Daughter	Son	Difference (3)-(2)	Observations Child/Daughter/Son
Background Characteristics	(1)	(2)	(3)	(4)	(5)
Age	42.02	41.94	42.09	-0.149	1,111/515/596
	(3.45)	(3.49)	(3.41)	(0.196)	
Years of education	4.82	4.79	4.85	-0.068	1,111/515/596
	(3.54)	(3.48)	(3.60)	(0.231)	
No education	0.27	0.26	0.28	-0.015	1,111/515/596
	(0.44)	(0.44)	(0.45)	(0.031)	
Primary education	0.55	0.57	0.53	0.039	1,111/515/596
	(0.50)	(0.50)	(0.50)	(0.033)	
Secondary or higher education	0.18	0.17	0.19	-0.024	1,111/515/596
	(0.38)	(0.37)	(0.39)	(0.024)	
Not Turkish	0.21	0.22	0.21	0.006	1,111/515/596
	(0.41)	(0.41)	(0.41)	(0.028)	
Parents are relatives	0.20	0.19	0.20	-0.014	1,111/515/596
	(0.40)	(0.39)	(0.40)	(0.026)	
Spent childhood in rural area	0.52	0.53	0.51	0.025	1,110/514/596
	(0.50)	(0.50)	(0.50)	(0.031)	
Labor Market Outcomes					
Ever worked	0.56	0.56	0.55	0.009	1,111/515/596
	(0.50)	(0.50)	(0.50)	(0.026)	
Ever worked in non-agriculture	0.31	0.30	0.31	-0.007	1,111/515/596
	(0.46)	(0.46)	(0.46)	(0.029)	
Currently employed	0.33	0.33	0.33	0.005	1,111/515/596
	(0.47)	(0.47)	(0.47)	(0.024)	
Currently employed in non-agriculture	0.16	0.16	0.16	0.000	1,111/515/596
	(0.37)	(0.37)	(0.37)	(0.022)	
Employed as unpaid family worker	0.13	0.14	0.13	0.009	1,111/515/596
	(0.34)	(0.35)	(0.34)	(0.019)	
Self-employed	0.07	0.06	0.08	-0.016	1,111/515/596
	(0.26)	(0.25)	(0.27)	(0.012)	
Duration of employment	9,08	9,23	8,95	0.282	1,024/472/552
	(11,70)	(11,75)	(11,67)	(0.669)	
Ever had to quit a job for childcare	0.04	0.04	0.04	0.006	1,111/515/596
	(0.20)	(0.20)	(0.19)	(0.011)	
Wife beating is acceptable if the wife…					
does any of these 7 things	0.31	0.34	0.29	0.051*	1,106/514/592
	(0.46)	(0.47)	(0.45)	(0.029)	
neglects children	0.19	0.21	0.17	0.038	1,106/514/592
	(0.39)	(0.41)	(0.38)	(0.027)	
argues with husband	0.14	0.16	0.11	0.046**	1,106/514/592
	(0.34)	(0.37)	(0.32)	(0.019)	
refuses to have sex	0.10	0.11	0.09	0.021	1,106/514/592
	(0.29)	(0.31)	(0.28)	(0.017)	
burns the food	0.03	0.03	0.03	-0.000	1,106/514/592
	(0.18)	(0.18)	(0.18)	(0.011)	
wastes money	0.20	0.22	0.19	0.032	1,106/514/592
	(0.40)	(0.41)	(0.39)	(0.027)	
does not cook	0.07	0.07	0.07	-0.005	1,106/514/592
	(0.25)	(0.25)	(0.26)	(0.015)	
neglects housework	0.15	0.16	0.14	0.018	1,106/514/592
	(0.36)	(0.37)	(0.35)	(0.025)	

Notes: Narrower estimation sample includes women whose first born child (daughter or son) was born ±36 months around January 1987 in column 1 (2 or 3). Statistical significance in column 4 is based on marked as follows: * 10%, ** 5%, and *** 1% level, with standard errors clustered by the running variable (i.e. month of birth of the child). Column 5 reports the sample size for the corresponding variable in column 1, 2 and 3 respectively.

Table 2: Treatment effects of a child's education on mother's attitudes towards domestic violence

Indicator		Sample	Local Non-parametric T	s.e.	N	h	Sample Mean
			(1)	(2)	(3)	(4)	(5)
Wife beating is acceptable in any of the 7 situations		Child	-0.0161	(0.0578)	1,106	36	0.339
		Girl	-0.1680**	(0.0836)	514	36	0.364
		Boy	0.1266	(0.0822)	561	35	0.316
Wife beating is acceptable if the wife…	neglects children	Child	-0.0522	(0.0488)	1,106	36	0.219
		Girl	-0.1951**	(0.0810)	428	31	0.264
		Boy	0.0466	(0.0653)	524	33	0.179
	argues with husband	Child	-0.0589	(0.0489)	943	31	0.145
		Girl	-0.1702**	(0.0774)	398	28	0.192
		Boy	0.0516	(0.0654)	491	31	0.108
	refuses to have sex	Child	0.0067	(0.0381)	1,065	35	0.108
		Girl	-0.1189*	(0.0634)	437	32	0.132
		Boy	0.1180**	(0.0468)	592	37	0.095
	burns the food	Child	-0.0213	(0.0254)	824	28	0.038
		Girl	-0.0459	(0.0441)	398	29	0.051
		Boy	0.0024	(0.0211)	618	39	0.033
	wastes money	Child	-0.1747***	(0.0567)	943	31	0.249
		Girl	-0.2507***	(0.0854)	380	27	0.298
		Boy	-0.0781	(0.0815)	476	29	0.211
	does not cook	Child	-0.0712*	(0.0364)	893	29	0.081
		Girl	-0.1650***	(0.0608)	350	25	0.112
		Boy	0.0567	(0.0434)	647	41	0.079
	neglects housework	Child	-0.0489	(0.0424)	1,211	41	0.178
		Girl	-0.1890**	(0.0752)	456	32	0.216
		Boy	0.0466	(0.0564)	572	35	0.150

Notes: The table reports the estimates of discontinuity (T) at the cut-off in a non-parametric local linear regression with uniform Kernel function. The forcing variable is the month of birth of the first born child (or daughter or son) of the respondent and the cutoff date is January 1987. The sample (N) is determined by the CCT optimal bandwidth (h). Standard errors are clustered at values of the forcing variable and displayed in parentheses. P-values from a test of joint treatment effect of all components are 0.02 in the overall sample, 0.06 in girls sample, and 0.05 in boys sample. The test is performed in a seemingly unrelated regression (SUR) using linear OLS estimation in a subsample within the CCT optimal bandwidth. Sample mean refers to the average value of the variable for women not affected by the reform whose children were born before January 1987 and within the CCT optimal bandwidth. Statistical significance is marked as follows: * 10%, ** 5%, and *** 1% level.

Table 3: Robustness check, treatment effects of child's (or daughter's) education on mother's attitudes towards domestic violence

	Local Linear Non-Parametric Approach												Local Linear Parametric Approach				Global Polynomial Parametric Approach			
	Main estimate		Bias-corrected estimate		Triangular Kernel		Epanechnikov Kernel		Quadratic function		IK bandwidth		No covariates		Controlling for respondent's age		Cubic control function		Quartic control function	
Sample	Child	Daughter	Child	Daughter	Child	Daughter	Child	Daughter	Child	Daughter	Child	Daughter	Child	Daughter	Child	Daughter	Child	Daughter	Child	Daughter
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
T	-0.016	-0.168**	-0.024	-0.197**	-0.037	-0.159*	-0.033	-0.164*	-0.037	-0.206**	-0.036	-0.073	-0.016	-0.168***	-0.024	-0.191***	-0.053	-0.158***	-0.085	-0.163**
s.e.	(0.058)	(0.084)	(0.058)	(0.084)	(0.057)	(0.084)	(0.057)	(0.084)	(0.073)	(0.105)	(0.035)	(0.062)	(0.051)	(0.058)	(0.051)	(0.062)	(0.054)	(0.055)	(0.066)	(0.065)
N	*1,106*	*514*	*1,106*	*514*	*1,367*	*625*	*1,283*	*576*	*1,537*	*744*	*2,885*	*938*	*1,106*	*514*	*1,106*	*514*	*6,258*	*2,995*	*6,258*	*2,995*
h	*36*	*36*	*36*	*36*	*47*	*45*	*43*	*42*	*52*	*55*	*101*	*68*	*36*	*36*	*36*	*36*	*n/a*	*n/a*	*n/a*	*n/a*

Notes: The table shows estimates of discontinuity (T) at the cut-off using alternative estimation techniques and bandwidth. The dependent variable is a dummy variable =1 if the respondent thinks that Wife beating is acceptable in any of the 7 situations she was asked about. Corresponding estimates for the individual scenarios that the respondent was asked about, and for the first-born sons sample are reported in Table A3 in the Appendix. The forcing variable is the month of birth of the first born child (or daughter) of the respondent and the cutoff date is January 1987. Columns 1(2) reports the main estimate for the first-born child (daughter) sample using a non-parametric local linear regression with uniform Kernel function. Column 3(4) reports the bias-corrected estimates as suggested by Calonico et al. (2014) for the first-born child (daughter) sample. In column 5(6), we use triangular Kernel function; in column 7(8) we use Epanechnikov Kernel function to estimate the non-parametric local linear regression for the first-born child (daughter) sample. In column 9(10) we control for a local quadratic function of the forcing variable. In column 11 (12) we use the Imbens and Kalyanamaran (2014) optimal bandwidth algorithm to determine the bandwidth for the non-parametric local linear regression. In column 13(14), we report the estimates of using the local linear parametric approach on the same sample determined by the CCT algorithm in our main estimates (column 1(2)). In column 14(15), we use the local linear parametric approach and control for respondent's year of birth fixed effects. In columns 17-20, we use the entire sample and control for either a cubic (cols 17-18) or quartic (cols. 19-20) function of the forcing variable. Statistical significance is marked as follows: * 10%, ** 5%, and *** 1% level.

Table 4: Two-samples-IV estimation

Two-samples-IV Estimation		Dependent variable in the 1st stage: Completed 8 years of schooling	Years of schooling	Years of schooling (capped at 8 years)	Years of schooling (capped at 13 years)
Reduced form	T	-0.1680***			
	s.e.	(0.0582)			
	h	36			
	N	514			
1st stage	T	0.1851***	1.0011***	0.2300**	0.6495***
	s.e.	(0.0193)	(0.1884)	(0.1162)	(0.2154)
	N	9,743	9,743	9,743	8,217
Two-samples-IV estimate	T	-0.9074***	-0.1678**	-0,7304	-0.2586**
	s.e.	(0.3280)	(0.0661)	(0.4473)	(0.1240)

Notes: First stage estimates are based on a sample of 15-49 year old ever-married women (main respondents) in the 2008 TDHS. The dependent variables in the first stage measuring education are: (1) binary variable equal one if the respondent completed at least 8 years of schooling, (2) years of schooling, (3) years of schooling capped at 8 years (i.e. 0–8), and (4) years of schooling capped at 13 years (i.e. 0–13). For columns 1-3, educational variables are defined for women 15 years and older and for column 4, they are defined for respondents 20 years and older to be sure that each respondent had the opportunity (enough time) to complete 8 and 13 years of schooling, respectively. Standard errors are clustered at values of the forcing variable and displayed in parentheses. Statistical significance is marked as follows: * 10%, ** 5%, and *** 1% level.

Table 5: Treatment effects of own education on respondent's attitudes towards domestic violence

Indicator		Local Non-parametric				Sample Mean
		T	s.e.	N	h	
Wife beating is acceptable in any of the 7 situations		-0.0966	(0.0742)	584	21	0.30
Wife beating is acceptable if she…	neglects children	-0.0536	(0.0588)	584	21	0.17
	argues with husband	-0.1085*	(0.0569)	556	20	0.16
	refuses to have sex	-0.0357	(0.0344)	628	23	0.05
	burns the food	-0.0494	(0.0343)	356	24	0.03
	wastes money	-0.1280*	(0.0717)	495	18	0.21
	does not cook	0.0428	(0.0377)	447	16	0.05
	neglects housework	-0.0931	(0.0633)	447	16	0.14

Notes: The table shows the treatment effects of the reform on respondent's attitudes towards domestic violence, using as the forcing variable the month of birth of the respondent herself. As such, the estimates of regression discontinuity (T) at the cut-off correspond to the effect of the reform on treated women's own attitudes towards domestic violence. The respondents are "ever-married women" who were selected for interview in the main module of the 2008 TDHS. We use a non-parametric local linear regression with uniform Kernel function. Sample (N) is determined by the CCT optimal bandwidth (h). Standard errors are clustered at values of the forcing variable and displayed in parentheses. Statistical significance is marked as follows: * 10%, ** 5%, and *** 1% level.

Table 6: Treatment effects of child's education on respondent's labor market outcomes

Indicator		Local Non-parametric				Sample Mean
		T	s.e.	N	h	
Currently employed	Child	0.0348	(0.0530)	1329	40	0.33
	Girl	0.0586	(0.0789)	561	36	0.32
Currently employed in the non-agricultural sector	Child	0.0241	(0.0396)	1361	42	0.16
	Girl	0.0697	(0.0602)	533	34	0.16
Employed as an unpaid family worker	Child	0.0245	(0.0382)	1361	41	0.13
	Girl	-0.0145	(0.0560)	644	42	0.14
Self-employed	Child	-0.0100	(0.0320)	1168	35	0.07
	Girl	0.0296	(0.0424)	494	32	0.07
Duration of employment	Child	1.2194	(0.9677)	655	26	3.89
	Girl	1.8284	(13.529)	286	26	3.96
Ever had to quit a job for childcare	Child	0.0205	(0.0191)	1578	48	0.04
	Girl	0.0019	(0.0263)	810	53	0.04

Notes: The table reports the estimates of discontinuity (T) at the cut-off in a non-parametric local linear regression with uniform Kernel function. The forcing variable is the month of birth of the first born child (or daughter) of the respondent and the cutoff date is January 1987. The sample (N) is determined by the CCT optimal bandwidth (h). Standard errors are clustered at values of the forcing variable and displayed in parentheses. Sample mean refers to the average value of the variable for women not affected by the reform whose children were born before January 1987 and within the CCT optimal bandwidth. The dependent variables are labor market outcomes of the respondent. “Currently employed” is a dummy variable =1 if the respondent was employed at the time of the survey. “Currently employed in the non-agricultural sector” is a dummy variable =1 if the respondent was working in the non-agricultural sector at the time of the survey. “Employed as an unpaid family worker” is a dummy variable =1 if the respondent was working as an unpaid worker in a family business at the time of the survey. “Self-employed” is a dummy variable =1 if the respondent was self-employed at the time of the survey. “Duration of employment” is the cumulative number of years that the respondent has worked in any job throughout her life until the time of the survey. “Ever had to quit a job for childcare” is a dummy variable =1 if the respondent reported having quit any job at any point of her life in order to care for her children or other children in the household. Statistical significance is marked as follows: * 10%, ** 5%, and *** 1% level.

Table 7: Heterogeneity analysis

	Panel A			
	Respondent lives in a rural area		Daughter is married and living with the respondent; or she lives elsewhere	
	Yes	*Yes*	*Yes*	*No*
	(1)	(2)	(3)	(4)
T	-0.3739**	-0.1323*	-0.2546**	-0.1529
s.e.	(0.1582)	(0.0699)	(0.1059)	(0.1041)
N	112	402	270	242
h	36	36	36	36

	Panel B			
	Respondent lives in a rural area			
	Yes		*No*	
	Daughter is married and living with the respondent; or she lives elsewhere			
	Yes	*No*	*Yes*	*No*
	(1)	(2)	(3)	(4)
T	-0.4102**	0.1395	-0.1949	-0.1482
s.e.	(0.2000)	(0.3779)	(0.1493)	(0.1152)
N	82	29	188	213
h	36	36	36	36

Notes: Estimations on subsamples by respondent's characteristics. Presented are estimates of discontinuity (T) at the cut-off in a parametric local linear regression controlling for respondent's year of birth fixed effects. The bandwidth is fixed at 36 months (the CCT optimal bandwidth for the entire sample). **Panel A:** In column (1), the sample is restricted to respondents who live in a rural area. In column (2), the sample is restricted to respondents who don't live in a rural area. In column (3), the sample is restricted to respondents whose eldest daughters are either married and living with the respondent or are living elsewhere (whether married or not). In column (4), the sample includes respondents whose eldest daughters never married and are living with them. **Panel B:** In columns (1) and (2), the sample is restricted to respondents who live in a rural area; while in columns (3) and (4) the sample includes respondents who live in a non-rural area. In columns (1) and (3) the sample is further restricted to respondents whose eldest daughters are either married and living with them or are living elsewhere (whether married or not); while in columns (2) and (4) the sample is further restricted to respondents whose eldest daughters never married and are living with the respondent.

11 Online Appendix

Table A1: Descriptive statistics in the full sample

	Child	Daughter	Son	Difference (3)-(2)	Observations Child/Daughter/Son
	(1)	(2)	(3)	(4)	(5)
Background Characteristics					
Age	34.60	34.55	34.65	-0.099	6,277/3,003/3,274
	(8.06)	(8.13)	(8.00)	(0.211)	
Years of education	5.55	5.54	5.57	-0.028	6,277/3,003/3,274
	(3.80)	(3.80)	(3.81)	(0.092)	
No education	0.22	0.22	0.22	-0.000	6,277/3,003/3,274
	(0.42)	(0.42)	(0.42)	(0.011)	
Primary education	0.52	0.52	0.52	0.003	6,277/3,003/3,274
	(0.50)	(0.50)	(0.50)	(0.013)	
Secondary or higher education	0.26	0.26	0.26	-0.003	6,277/3,003/3,274
	(0.44)	(0.44)	(0.44)	(0.010)	
Not Turkish	0.25	0.26	0.25	0.013	6,276/3,003/3,273
	(0.44)	(0.44)	(0.43)	(0.011)	
Parents are relatives	0.22	0.23	0.22	0.008	6,273/3,000/3,273
	(0.42)	(0.42)	(0.41)	(0.011)	
Spent childhood in rural area	0.48	0.48	0.48	-0.002	6,269/2,998/3,271
	(0.50)	(0.50)	(0.50)	(0.013)	
Labor Market Outcomes					
Ever worked	0.55	0.54	0.56	-0.019	6,277/3,003/3,274
	(0.50)	(0.50)	(0.50)	(0.012)	
Ever worked in non-agriculture	0.33	0.32	0.34	-0.021*	6,277/3,003/3,274
	(0.47)	(0.47)	(0.47)	(0.012)	
Currently employed	0.29	0.29	0.30	-0.013	6,277/3,003/3,274
	(0.46)	(0.45)	(0.46)	(0.012)	
Currently employed in non-agriculture	0.15	0.15	0.16	-0.008	6,277/3,003/3,274
	(0.36)	(0.36)	(0.36)	(0.009)	
Employed as unpaid family worker	0.11	0.11	0.11	0.002	6,277/3,003/3,274
	(0.32)	(0.32)	(0.32)	(0.008)	
Self-employed	0.06	0.06	0.06	-0.007	6,277/3,003/3,274
	(0.24)	(0.23)	(0.24)	(0.005)	
Duration of employment	6.53	6.60	6.46	0.133	6,259/2,997/3,262
	(9.51)	(9.71)	(9.33)	(0.241)	
Ever had to quit a job for childcare	0.05	0.04	0.05	-0.008	6,277/3,003/3,274
	(0.21)	(0.21)	(0.22)	(0.005)	
Wife beating is acceptable if the wife…					
does any of these 7 things	0.28	0.28	0.28	0.001	6,258/2,995/3,263
	(0.45)	(0.45)	(0.45)	(0.011)	
neglects children	0.17	0.18	0.17	0.007	6,258/2,995/3,263
	(0.38)	(0.38)	(0.37)	(0.009)	
argues with husband	0.14	0.14	0.13	0.006	6,258/2,995/3,263
	(0.34)	(0.35)	(0.34)	(0.008)	
refuses to have sex	0.08	0.08	0.08	-0.001	6,258/2,995/3,263
	(0.27)	(0.26)	(0.27)	(0.006)	
burns the food	0.03	0.03	0.03	0.001	6,258/2,995/3,263
	(0.18)	(0.18)	(0.18)	(0.005)	
wastes money	0.18	0.18	0.18	-0.006	6,258/2,995/3,263
	(0.38)	(0.38)	(0.39)	(0.010)	
does not cook	0.06	0.06	0.06	-0.004	6,258/2,995/3,263
	(0.24)	(0.24)	(0.24)	(0.006)	
neglects housework	0.13	0.13	0.13	0.004	6,258/2,995/3,263
does any of these 7 things	(0.34)	(0.34)	(0.34)	(0.008)	

Notes: The sample includes all respondents for the ever-married women module in the 2008 TDHS. Statistical significance in column 4 is based on marked as follows: * 10%, ** 5%, and *** 1% level, with standard errors clustered by the running variable (i.e. month of birth of the child).

Table A2: Individual tests of discontinuity of covariates at the cut-off

Characteristic	Local Non-parametric				Tests	
	T	s.e.	N	h	KS	Bartlett
Age	-0.78**	(0.35)	1,216	40	0.00	0.00
Years of education	0.17	(0.44)	1,070	36	0.84	0.37
No education	-0.00	(0.06)	1,054	34	0.84	0.32
Primary education	0.01	(0.06)	1,264	43	0.97	0.89
Secondary or higher education	0.01	(0.04)	1,480	51	1.00	0.69
Not Turkish	0.03	(0.05)	1,264	42	1.00	0.59
Parents are relatives	0.04	(0.05)	1,216	40	1.00	0.50
Spent childhood in rural area	0.00	(0.06)	1,069	36	0.71	0.93
Ever worked in non-agricultural sector	0.06	(0.06)	1,027	34	0.88	0.45

Notes: Left panel presents estimates of discontinuity (T) at the cut-off in a non-parametric local linear regression with uniform Kernel function. Sample (N) is determined by the CCT optimal bandwidth (h). P-values from a test of joint significance of all respondent's background characteristics are 0.43 in the overall sample, 0.13 in girls sample, and 0.15 in boys sample. The test is performed in a seemingly unrelated regression (SUR) using linear OLS estimation in a subsample within the CCT optimal bandwidth. Right panel lists combined p-values from a Kolmogorov-Smirnov (KS) test of equal distributions before and after the cut-off, and p-values from a Bartlett test of equal variances before and after the cut-off. Both tests are performed within a bandwidth of 36 months. Statistical significance is marked as follows: * 10%, ** 5%, and *** 1% level.

Table A3 (part 1): Robustness checks, Treatment effects on individual components of domestic violence attitudes

Robustness Check		Wife beating is acceptable in any of the 7 situations			Wife beating is acceptable if the wife…													
					neglects children		argues with husband		refuses to have sex		burns the food		wastes money		does not cook		neglects housework	
		Child	Daughter	Son	Child	Daughter	Child	Daughter	Child	Daughter	Child	Daughter	Child	Daughter	Child	Daughter	Child	Daughter
Main estimate	T	-0.0161	-0.1680**	0.1266	-0.0522	-0.1951**	-0.0589	-0.1702**	0.0067	-0.1189*	-0.0213	-0.0459	-0.1747***	-0.2507***	-0.0712*	-0.1650***	-0.0489	-0.1890**
	s.e.	(0.0578)	(0.0836)	(0.0822)	(0.0488)	(0.0810)	(0.0489)	(0.0774)	(0.0381)	(0.0634)	(0.0254)	(0.0441)	(0.0567)	(0.0854)	(0.0364)	(0.0608)	(0.0424)	(0.0752)
Bias-corrected estimate	T	-0.0238	-0.1970**	0.1473*	-0.0601	-0.2260***	-0.0662	-0.2033***	0.0097	-0.1261**	-0.0314	-0.0529	-0.1756***	-0.2802***	-0.0811**	-0.1804***	-0.0607	-0.2200***
	s.e.	(0.0578)	(0.0836)	(0.0822)	(0.0488)	(0.0810)	(0.0489)	(0.0774)	(0.0381)	(0.0634)	(0.0254)	(0.0441)	(0.0567)	(0.0854)	(0.0364)	(0.0608)	(0.0424)	(0.0752)
	N	*1,106*	*514*	*561*	*1,106*	*428*	*943*	*398*	*1,065*	*437*	*824*	*398*	*943*	*380*	*893*	*350*	*1,211*	*456*
	h	*36*	*36*	*35*	*36*	*31*	*31*	*28*	*35*	*32*	*28*	*29*	*31*	*27*	*29*	*25*	*41*	*32*
Triangular Kernel	T	-0.0373	-0.1588*	0.0847	-0.0687	-0.1912**	-0.0435	-0.1527**	0.0105	-0.0950	-0.0287	-0.0557	-0.1621***	-0.2806***	-0.0664*	-0.1562***	-0.0704*	-0.1783**
	s.e.	(0.0569)	(0.0839)	(0.0805)	(0.0486)	(0.0769)	(0.0476)	(0.0738)	(0.0376)	(0.0611)	(0.0237)	(0.0439)	(0.0560)	(0.0834)	(0.0363)	(0.0587)	(0.0405)	(0.0706)
	N	*1,367*	*625*	*703*	*1,339*	*576*	*1,236*	*550*	*1,316*	*564*	*1,127*	*550*	*1,186*	*493*	*1,127*	*488*	*1,563*	*625*
	h	*47*	*45*	*45*	*45*	*42*	*42*	*40*	*45*	*41*	*38*	*39*	*39*	*36*	*37*	*35*	*54*	*45*
Epanechnikov Kernel	T	-0.0327	-0.1642*	0.0937	-0.0650	-0.1875**	-0.0452	-0.1547**	0.0079	-0.1047*	-0.0287	-0.0541	-0.1659***	-0.2853***	-0.0697*	-0.1579***	-0.0718*	-0.1792**
	s.e.	(0.0570)	(0.0841)	(0.0813)	(0.0488)	(0.0764)	(0.0473)	(0.0728)	(0.0374)	(0.0613)	(0.0236)	(0.0423)	(0.0562)	(0.0831)	(0.0361)	(0.0577)	(0.0409)	(0.0706)
	N	*1,283*	*576*	*647*	*1,259*	*550*	*1,157*	*524*	*1,236*	*524*	*1,049*	*524*	*1,106*	*474*	*1,049*	*456*	*1,454*	*576*
	h	*43*	*42*	*40*	*42*	*40*	*39*	*37*	*42*	*38*	*35*	*37*	*36*	*33*	*35*	*32*	*49*	*42*
Quadratic function	T	-0.0372	-0.2055**	0.1191	-0.0615	-0.2054**	-0.0611	-0.1768*	0.0104	-0.1009	-0.0282	-0.0641	-0.1553**	-0.2993***	-0.0706*	-0.1772***	-0.0651	-0.2070**
	s.e.	(0.0734)	(0.1045)	(0.0948)	(0.0583)	(0.0995)	(0.0557)	(0.0928)	(0.0543)	(0.0767)	(0.0283)	(0.0554)	(0.0652)	(0.0936)	(0.0422)	(0.0657)	(0.0523)	(0.0828)
	N	*1,537*	*744*	*942*	*1,730*	*637*	*1,666*	*637*	*1,211*	*637*	*1,563*	*637*	*1,594*	*735*	*1,563*	*712*	*1,777*	*827*
	h	*52*	*55*	*60*	*58*	*47*	*56*	*46*	*41*	*46*	*53*	*46*	*54*	*54*	*53*	*52*	*61*	*60*
IK bandwidth	T	-0.0359	-0.0727	0.0144	-0.0477	-0.1244**	0.0063	-0.0514	-0.0195	-0.0428	-0.0060	-0.0296	-0.0503*	-0.1354**	-0.0066	-0.0800**	-0.0416	-0.1492***
	s.e.	(0.0347)	(0.0617)	(0.0558)	(0.0345)	(0.0576)	(0.0255)	(0.0495)	(0.0236)	(0.0391)	(0.0156)	(0.0230)	(0.0264)	(0.0548)	(0.0203)	(0.0363)	(0.0312)	(0.0532)
	N	*2,885*	*938*	*1,117*	*2,153*	*844*	*3,121*	*924*	*2,511*	*1,045*	*2,080*	*1,260*	*4,166*	*909*	*2,800*	*880*	*2,186*	*835*
	h	*101*	*68*	*73*	*74*	*61*	*112*	*67*	*88*	*76*	*72*	*91*	*161*	*65*	*98*	*63*	*76*	*60*

Notes: The table shows estimates of discontinuity (T) at the cut-off using alternative estimation techniques and bandwidth. The dependent variables are dummy variables =1 if the respondent thinks that wife beating is acceptable in one of the given situations. The forcing variable is the month of birth of the first born child (or daughter or son) of the respondent and the cutoff date is January 1987. The first 2 rows of the table report the main estimate for the relevant sample (either first-born child, daughter or son) using the non-parametric local linear regression with uniform Kernel function. The following rows show, in descending order: (i) the bias-corrected estimates as suggested by Calonico et al. (2014); (iii) non-parametric local linear regression using the triangular Kernel function; (iii) non-parametric local linear regression using the Epanechnikov Kernel function; (iv) non-parametric local linear regression with a local quadratic function of the forcing variable; (v) non-parametric local linear regression using the the Imbens and Kalyanamaran to determine the optimal bandwidth; (vi) local linear *parametric* approach on the same sample determined by the CCT algorithm in our main estimates; (vii) local linear parametric approach controlling for the respondent's year of birth fixed effects (viii) global parametric approach where we use the entire sample and control for either a cubic or quartic function of the forcing variable. Statistical significance is marked as follows: * 10%, ** 5%, and *** 1% level.

Table A3 (part 2): Robustness checks, Treatment effects on individual components of domestic violence attitudes

Robustness Check		Wife beating is acceptable in any of the 7 situations			Wife beating is acceptable if the wife…													
					neglects children		argues with husband		refuses to have sex		burns the food		wastes money		does not cook		neglects housework	
		Child	Daughter	Son	Child	Daughter	Child	Daughter	Child	Daughter	Child	Daughter	Child	Daughter	Child	Daughter	Child	Daughter
Local Linear Parametric Approach																		
No covariates (Main est. h)	T	-0.0161	-0.1680***	0.1266	-0.0522	-0.1951***	-0.0589	-0.1702**	0.0067	-0.1189**	-0.0213	-0.0459	-0.1747**	-0.2507***	-0.0712**	-0.1650***	-0.0489	-0.1890***
	s.e.	(0.0505)	(0.0582)	(0.0918)	(0.0417)	(0.0701)	(0.0451)	(0.0834)	(0.0363)	(0.0526)	(0.0224)	(0.0393)	(0.0699)	(0.0601)	(0.0331)	(0.0565)	(0.0333)	(0.0662)
Age dummies (Main est. h)	T	-0.0239	-0.1906***	0.1221	-0.0531	-0.2035***	-0.0691	-0.1706**	0.0049	-0.1292**	-0.0204	-0.0410	-0.1776**	-0.2404***	-0.0746**	-0.1624***	-0.0511	-0.2005***
	s.e.	(0.0514)	(0.0620)	(0.0944)	(0.0413)	(0.0695)	(0.0449)	(0.0805)	(0.0372)	(0.0515)	(0.0231)	(0.0403)	(0.0712)	(0.0631)	(0.0341)	(0.0553)	(0.0328)	(0.0676)
	N	*6,258*	*2,995*	*3,263*	*6,258*	*2,995*	*6,258*	*2,995*	*6,258*	*2,995*	*6,258*	*2,995*	*6,258*	*2,995*	*6,258*	*2,995*	*6,258*	*2,995*
Global Polynomial Parametric Approach																		
P3 No covariates	T	-0.0528	-0.1576***	0.0472	-0.0561	-0.1742***	-0.0445	-0.1322**	-0.0025	-0.0745	-0.0101	-0.0568	-0.1502**	-0.2437***	-0.0315	-0.1120**	-0.0687*	-0.2018***
	s.e.	(0.0535)	(0.0547)	(0.0902)	(0.0380)	(0.0644)	(0.0401)	(0.0645)	(0.0319)	(0.0490)	(0.0209)	(0.0351)	(0.0611)	(0.0541)	(0.0294)	(0.0498)	(0.0354)	(0.0595)
P4 No covariates	T	-0.0852	-0.1632**	-0.0115	-0.0749	-0.1839**	-0.0983*	-0.1695**	-0.0232	-0.1259**	-0.0320	-0.0769*	-0.2173***	-0.2951***	-0.0826**	-0.1800***	-0.0850**	-0.2217***
	s.e.	(0.0661)	(0.0651)	(0.1116)	(0.0466)	(0.0779)	(0.0529)	(0.0845)	(0.0417)	(0.0605)	(0.0261)	(0.0429)	(0.0779)	(0.0615)	(0.0363)	(0.0602)	(0.0407)	(0.0711)
P5 No covariates	T	-0.0918	-0.2085***	0.0226	-0.0988*	-0.2603***	-0.0898	-0.1705	0.0235	-0.0730	-0.0265	-0.0544	-0.2254**	-0.3044***	-0.0887**	-0.1939***	-0.0809*	-0.2221***
	s.e.	(0.0771)	(0.0740)	(0.1312)	(0.0550)	(0.0856)	(0.0660)	(0.1034)	(0.0476)	(0.0668)	(0.0332)	(0.0562)	(0.0926)	(0.0691)	(0.0432)	(0.0734)	(0.0467)	(0.0788)
	N	*6,258*	*2,995*	*3,263*	*6,258*	*2,995*	*6,258*	*2,995*	*6,258*	*2,995*	*6,258*	*2,995*	*6,258*	*2,995*	*6,258*	*2,995*	*6,258*	*2,995*

Notes: The table shows estimates of discontinuity (T) at the cut-off using alternative estimation techniques and bandwidth. The dependent variables are dummy variables =1 if the respondent thinks that wife beating is acceptable in one of the given situations. The forcing variable is the month of birth of the first born child (or daughter or son) of the respondent and the cutoff date is January 1987. The first 2 rows of the table report the main estimate for the relevant sample (either first-born child, daughter or son) using the non-parametric local linear regression with uniform Kernel function. The following rows show, in descending order: (i) the bias-corrected estimates as suggested by Calonico et al. (2014); (iii) non-parametric local linear regression using the triangular Kernel function; (iii) non-parametric local linear regression using the Epanechnikov Kernel function; (iv) non-parametric local linear regression with a local quadratic function of the forcing variable; (v) non-parametric local linear regression using the the Imbens and Kalyanamaran to determine the optimal bandwidth; (vi) local linear *parametric* approach on the same sample determined by the CCT algorithm in our main estimates; (vii) local linear parametric approach controlling for the respondent's year of birth fixed effects (viii) global parametric approach where we use the entire sample and control for either a cubic or quartic function of the forcing variable. Statistical significance is marked as follows: * 10%, ** 5%, and *** 1% level.

Table A4: Treatment effects on respondent's likelihood to have migrated in recent past

	Respondent migrated in the last 10 years		Respondent migrated in the last 5 years	
	Non-parametric approach	Local linear parametric approach	Non-parametric approach	Local linear parametric approach
	(1)	(2)	(3)	(4)
T	-0.0437	-0.0185	-0.0342	-0.0195
s.e.	(0.0541)	(0.0637)	(0.0553)	(0.0445)
N	698	515	489	515
h	50	36	34	36

Notes: The table shows the treatment effects of the reform on whether the respondent migrated in the last 10 (5) years before the survey. The sample includes respondents whose first born child is a girl. In columns (1) and (3), we use a non-parametric local linear regression with uniform Kernel function. Sample (N) is determined by the CCT optimal bandwidth (h). In columns (2) and (4) we use a parametric local linear regression, controlling for respondent year of birth fixed effects. Sample (N) is is fixed at 36 months around the cutoff, which is the CCT optimal bandwidth for the main outcome of interest (respondent's domestic violence attitudes). Standard errors are clustered at values of the forcing variable and displayed in parentheses. Statistical significance is marked as follows: * 10%, ** 5%, and *** 1% level.

Table A5: Treatment effects of girls' schooling on their mothers' opinions about gender roles

	Indicator	Local Non-parametric T	s.e.	N	h	Sample Mean
1	Men should also do the housework like cooking, washing, ironing, and cleaning. (No=1)	-0.0068	(0.0829)	548	39	0.345
2	A married woman should work outside the home if she wants to. (No=1)	0.0655	(0.0521)	397	28	0.108
3	A woman may go anywhere she wants without her husband's permission. (No=1)	0.0102	(0.0731)	522	37	0.724
4	Men are wiser. (Yes=1)	-0.1540**	(0.0700)	487	34	0.218
5	Women should be more involved in politics. (No=1)	0.0377	(0.0835)	472	33	0.282
6	Women should be virgins when they get married. (Yes=1)	-0.0316	(0.0469)	513	36	0.914
7	The important decisions in the family should be made only by men of the family. (Yes=1)	-0.0781	(0.0839)	416	29	0.238
8	A woman shouldn't argue with her husband even if she disagrees with him. (Yes=1)	-0.1401	(0.0870)	522	37	0.473
9	It is better to educate a son than a daughter. (Yes=1)	0.0072	(0.0774)	349	25	0.158
Respondent agreed with at least one gender-biased statement		-0.0245	(0.0376)	397	28	0.967

Notes: The table reports the estimates of discontinuity (T) at the cut-off in a non-parametric local linear regression with uniform Kernel function. The forcing variable is the month of birth of the first born daughter of the respondent and the cutoff date is January 1987. The sample (N) is determined by the CCT optimal bandwidth (h). Standard errors are clustered at values of the forcing variable and displayed in parentheses. Sample mean refers to the average value of the variable for women not affected by the reform whose children were born before January 1987 and within the CCT optimal bandwidth. The dependent variables are dummy variables indicating whether the respondent stated she agreed or disagreed with statements about gender roles in the household and in the society. The variables are coded such that 1 indicates the respondent has a gender-biased opinion, and 0 is otherwise. P-value from a test of joint significance of all statements (1-9) is 0.304. The test is performed in a seemingly unrelated regression (SUR) using linear OLS estimation in a subsample within the CCT optimal bandwidth. Statistical significance is marked as follows: * 10%, ** 5%, and *** 1% level.