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The Effect of Compulsory Schooling Expansion on Mothers' Attitudes Toward Domestic Violence in Turkey

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Abstract

An extensive literature examines the intergenerational spillover effects of education, but evidence on the causal effects of children's education on their parents' outcomes is scarce. This paper estimates the spillover effects of children's schooling on their mothers' attitudes toward domestic violence in Turkey. To identify the causal effect of children's schooling, we take advantage of a reform that took place in Turkey in 1997 and expanded compulsory schooling from five to eight years. Using a regression discontinuity design based on monthly birth cohorts and data from the 2008 and 2013 waves of the Turkey Demographic and Health Surveys, this paper shows that mothers whose eldest daughters were exposed to higher compulsory schooling are by 12 percentage points less likely to find domestic violence justifiable, which represents a decrease by 43 percent. We find no similar effect for boys' schooling. Our findings demonstrate that children's schooling can have impacts on their parents' attitudes, and such effects are likely to vary by the gender of the child.

JEL classification: I21, I28, J12, J16

Keywords: intergenerational spillover effects, spillover effects of education, compulsory schooling, attitudes toward domestic violence

1. Introduction

A growing body of literature demonstrates the effects of parental education on children's outcomes, but few studies have explored the possibility that children's schooling may have spillover effects on their

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parents. This paper provides evidence that a compulsory schooling reform has led to upward generational spillover effects in Turkey. In particular, women whose daughters were exposed to a longer period of compulsory schooling have been less likely to find domestic violence acceptable. 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). While attitudes toward the acceptability of domestic violence are highly correlated with the prevalence of domestic violence (Garcia-Moreno et al. 2005), there is limited evidence on what drives the variation in these attitudes (Alesina, Brioschi, and La Ferrara 2016). Our contribution is to show that education, in particular the schooling level of the younger generation, may be one factor that affects women's attitudes toward domestic violence in a context where the rate of intimate-partner violence is high.[1]

To identify the spillover effects of children's education on the older generation, we take advantage of a change in compulsory schooling in Turkey. In 1997, the Turkish parliament passed a law that increased the duration of compulsory schooling from five to eight years. Students who were starting grade five in the school year 1997/1998 were subjected to the new schooling regime. Hence, the law was retroactive, and neither students nor their parents could manipulate exposure to the reform. The legally required age of starting primary and secondary school in Turkey is 6 and 11 years, respectively. This implies that children born before January 1987 (who should have completed grade five in school year 1996/1997) could drop out of school after five years, while those born afterward were required to complete eight years of schooling. While there may have been imperfect compliance for various reasons, previous studies have shown (and this paper confirms) that, on average, the reform increased years of schooling at the threshold by approximately one additional year for women.[2] We use the 2008 and 2013 waves of the Turkey Demographic and Health Survey (TDHS), which contain information on respondents' children's month and year of birth; all respondents are women. This enables us to implement a regression discontinuity design (RDD) and compare outcomes of respondents whose eldest child was born after January 1987 (and was therefore subjected to longer compulsory schooling) to those who had their first child just before that date. Previous studies have used a similar RDD approach to identify the direct effect of the reform on the treated cohorts (Gulesci and Meyersson 2016; Erten and Keskin 2018) but, as far as we are aware, this paper is the first to use this approach to identify spillover effects on the older generation. We focus on the respondents' first-born child because the timing and sex composition of higher birth orders are less likely to be exogenous.

We find that women whose daughters were exposed to the new compulsory schooling regime (henceforth "treated") are less likely to find domestic violence acceptable. In particular, mothers of treated girls are by 12 percentage points less likely to consider wife-beating justifiable when compared to mothers of untreated girls, which corresponds to a 43 percent decrease in acceptance of domestic violence 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 inclusion of covariates, alternative specifications, and different estimation methods.

1 In this paper, we use the terms "intimate-partner violence" and "domestic violence" interchangeably. In a nationally representative survey, 42 percent of women reported being subject to domestic violence (TRPM 2008). Relative to the rest of the world, this rate places Turkey among the countries with the highest rates of domestic violence (WHO 2013; Devries et al. 2013). More broadly, women's rights and female labor force participation remain low in many majority-Muslim societies, including Turkey (UNDP 2005; Doepke, Tertilt, and Voena 2012).

2 See, for example, Dincer, Kaushal, and Grossman (2014); Kırdar, Dayıoğlu, and Koc (2016); Aydemir and Kırdar (2017).

Our paper contributes to a growing literature on intergenerational spillover effects of education.[3] 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, Currie and Moretti [2003]; Black, Devereux, and Salvanes [2005]; Oreopoulos, Page, and Stevens [2006]; Lundborg, Nilsson, and Rooth [2014] in developed countries; and Glewwe [1999]; Breierova and Duflo [2004]; Chen and Li [2009] in developing countries). The few studies that have explored the possibility of spillover effects of children's schooling on their parents' outcomes focus on developed countries and have found mixed evidence. Berniell, de la Mata, and Valdes (2013) find that health education in primary schools in the United States led to 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 United States, 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, that is, parents lean on their children rather than learn from them. The current paper contributes to this literature by providing evidence on the causal effects of children's education on their mothers' attitudes toward domestic violence in a developing country with high rates of domestic violence.

The rest of the paper is organized as follows: Section 2 offers 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. Section 6 discusses the interpretation of the findings and potential mechanisms, 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, the Turkish education system consisted of three components: five years of primary school (*ilkokul*), three years of junior high school (*ortaokul*), and three 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.[4] The new education law stipulated an extension of compulsory schooling from five to eight years, thus effectively merging primary and junior high school into "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.[5] The law was proposed in February 1997, adopted on August 16, 1997, through an amendment to the Basic Education Law and went into effect as of school year 1997/1998, that is, in September 1997.

According to the Turkish law, compulsory schooling begins in September of the year in which a child turns six years old. As mentioned before, the new law No. 4306 made eight years of primary education compulsory, and it was effective starting with the school year 1997/1998. This implied that students who had completed the fifth grade in the school year 1996/1997 were exempt from the law while those who had completed the fourth grade were required to remain in primary education until they completed eight

3 More generally, our paper is related to research on nonpecuniary effects of education, see Oreopoulos and Salvanes (2011) and Lochner (2011) for recent reviews.

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

5 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.

years of schooling.[6] The combination of these two laws – the law pertaining to the school-starting age and the education reform that made eight years of schooling compulsory starting from September 1997 – implied that children who were born in January 1987 or before would have completed fifth grade in school year 1996/1997 and thus they would have been exempt from the new law. By contrast, children born in January 1987 or later would have completed at most four grades in the school year 1996/1997, and, therefore, they would have been required to stay in school until the eighth grade. Naturally, there may have been factors weakening the link between a child's date of birth and her/his 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 regression discontinuity design (RDD). 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 massive investments in education. These included expenditure on construction of schools, educational materials, and staff. Within just a few years of the implementation of the reform, around 82,000 new classrooms were built, increasing 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, "the 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).

3. Data

The data we use come from the 2008 and 2013 waves of the Turkey Demographic and Health Survey. TDHS is a nationally representative household survey in which 10,525 households were interviewed in 2008 and 11,794 households in 2013. The survey consists of a household module and a woman's module. In the household module, basic information on all household members and general household characteristics is collected. The woman's questionnaire was administered to ever-married women aged 15–49 in 2008 (8,003 ever-married women), while the sampling frame included all women aged 15–49 in 2013, independently of their marital status (9,746 women were interviewed). In this module, the respondent is asked extensively about her health, her birth history, and her attitudes, among other things. Importantly for our identification strategy, information on respondents' and their children's 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 toward domestic violence. For our baseline results, we consider the eldest child of the respondent. There are several reasons for this. First, an approach that uses multiple observations (children) derived from the same respondent (mother) would be wrong because mothers are the units of analysis and each mother can therefore be included only once. Second, considering any other child than the firstborn would arguably lead to endogeneity problems such as sample selection based on households' or respondents' (unobserved) characteristics related to fertility preferences. Third, the sex of the first-born child at birth is likely to be

6 Parents who did not comply with the new law faced monetary penalties or in some cases incarceration (Kırdar, Dayıoğlu, and Koc 2016).

reasonably exogenous whereas this is not necessarily the case for later-born children.[7] Working with observations where sex of the child is exogenous is important because as part of our analysis we compare the differences in upward educational spillover effects coming from girls versus those coming from boys. One caveat with our approach is that even though the gender of the first-born child is plausibly exogenous with respect to the treatment, it is likely correlated with ex-post family characteristics (such as household size and the number and gender composition of younger siblings), and these may be in part driving any observed differences in the estimated effects between mothers of the eldest girls and boys. Therefore, we will examine the robustness of our results to controlling for differences in ex-post family characteristics.

Our estimation sample consists of 13,148 women with first-born children. 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 "Identification Strategy" in section 4 for more details), table 1 shows descriptive statistics from a narrower sample of women who are on average 43 years old, and their children are 23 years old.[8] 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 five years of schooling. This corresponds to the majority of respondents (58 percent) having completed primary education. The other half of the sample is split between women with no education (24 percent) and women with secondary or higher education (18 percent). In terms of family background, one in five women (18 percent) comes from a family where Turkish is not the first language (most of these cases are Kurdish families) and had consanguineous parents (19 percent); more than half (56 percent) of respondents spent their childhood in a rural area.

The panel of the table shows descriptive statistics about women's labor market outcomes. More than half (61 percent) of the women in the sample reported ever having had a job. In terms of their current employment status, only 36 percent of them were employed at the time of the survey, and only 19 percent were working in the nonagricultural sector. In terms of their types of occupations, 13 percent were employed as "unpaid family workers," 8 percent were "self-employed," and the rest (15 percent) were "working for a wage." The survey also collected information about their job histories. Using this, we calculate respondents' total duration of employment (throughout their lives, in any type of employment), which is 5 years on average for all women and 10 years on average for those who have ever had a job. The third panel summarizes respondents' fertility-related outcomes. The average interval between the first and second birth is over 3 years (39 months), and the share of girls among later-born children is 48 percent. Overall, respondents have had around three children and the average household size is five.

The bottom panel of table 1 displays summary statistics on women's attitudes toward domestic violence, which will be our main outcomes of interest. Respondents were asked whether wife-beating is acceptable in certain situations. We limit our attention to the four situations that were asked in both the 2008 and 2013 waves of the TDHS.[9] While domestic violence has very low acceptance rates in some situations, other reasons for wife-beating seem to be more widely acceptable. In particular, wife-beating

7 In particular, a patriarchal society with preference for male offspring leads parents to behaviors where they desire at least one son (Bhalotra and Cochrane 2010; Rosenblum 2013). 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 toward boys.

8 Summary statistics are based on a bandwidth of 36 months around the cut-off, date which is close to our estimation sample. Descriptive statistics based on the full sample are shown in table S1.1 in the supplementary online appendix, available with this article at *The World Bank Economic Review* website.

9 Given that respondents had the option of answering "don't know" and this occurred to some extent (3 percent 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, see "Robustness Checks" in section 5.

Table 1. Descriptive Statistics of Women in the Estimation Sample

	(1) Daughter		(2) Son		(3) Treatment effect: Daughter			(4) Treatment effect: Son			(5) Difference (3)–(4)
Characteristic	Mean	s.d.	Mean	s.d.	T	s.e.	N	T	s.e.	N	Diff.
Background characteristics											
Age	43.35	(3.61)	43.59	(3.51)	−0.5454	(0.3814)	1,484	0.0707	(0.4772)	1,645	−0.616
Year of birth	1966.44	(3.20)	1966.30	(3.17)	0.4651	(0.4676)	2,094	0.3784	(0.3177)	2,744	0.0867
Interviewed in 2013	0.37	(0.49)	0.39	(0.49)	−0.0042	(0.0681)	1,548	0.0490	(0.0616)	1,664	−0.0532
Years of education	4.96	(3.37)	4.99	(3.48)	−0.7120*	(0.3838)	2,799	0.7320*	(0.4366)	2,505	−1.444
No education completed	0.24	(0.45)	0.25	(0.46)	0.0353	(0.0603)	2,243	−0.0254	(0.0537)	2,607	0.0607
Completed primary education	0.59	(0.49)	0.56	(0.50)	−0.0330	(0.0666)	2,424	−0.0569	(0.0570)	1,750	0.0238
Completed secondary education	0.17	(0.35)	0.19	(0.37)	−0.0290	(0.0419)	2,504	0.0758	(0.0471)	1,745	−0.105
Not Turkish	0.18	(0.41)	0.17	(0.42)	0.0994*	(0.0560)	2,204	−0.0179	(0.0593)	1,889	0.117
Parents are relatives	0.19	(0.39)	0.19	(0.41)	−0.0303	(0.0535)	2,355	0.0783	(0.0743)	1,657	−0.109
Spent childhood in rural area	0.57	(0.49)	0.56	(0.49)	−0.0859	(0.0579)	1,608	−0.0382	(0.0556)	2,797	−0.0477
Mother has no education	0.69	(0.45)	0.66	(0.45)	0.0428	(0.0673)	2,523	−0.1390*	(0.0798)	2,127	0.182
Mother has primary education	0.09	(0.28)	0.12	(0.30)	−0.0164	(0.0466)	2,112	0.0355	(0.0563)	1,945	−0.0519
Mother has secondary education	0.18	(0.36)	0.20	(0.37)	−0.0021	(0.0580)	1,805	0.0964	(0.0609)	1,842	−0.0985
Mother has higher education	0.03	(0.12)	0.02	(0.13)	−0.0222	(0.0213)	1,816	0.0001	(0.0127)	1,682	−0.0223
Father has no education	0.35	(0.48)	0.34	(0.48)	−0.0013	(0.0512)	2,735	−0.0443	(0.0607)	2,096	0.0430
Father has primary education	0.10	(0.31)	0.08	(0.28)	0.0030	(0.0374)	1,542	0.0989***	(0.0317)	1,685	−0.0959
Father has secondary education	0.44	(0.49)	0.44	(0.49)	0.0126	(0.0592)	2,265	−0.0360	(0.0543)	2,150	0.0486
Father has higher education	0.05	(0.22)	0.07	(0.25)	0.0124	(0.0459)	2,310	0.0245	(0.0340)	1,808	−0.0120
Labor market outcomes											
Ever worked	0.62	(0.49)	0.60	(0.50)	0.0155	(0.0597)	2,001	−0.0744	(0.0680)	2,518	0.0899
Duration of employment	5.15	(7.62)	4.39	(7.26)	0.3894	(1.0294)	1,869	0.2553	(1.1347)	1,627	0.134
Currently employed	0.37	(0.48)	0.35	(0.47)	0.0129	(0.0740)	2,135	−0.1363**	(0.0678)	1,945	0.149
Currently employed in the nonagricultural sector	0.21	(0.37)	0.18	(0.36)	0.0266	(0.0551)	2,034	−0.0465	(0.0726)	2,085	0.0731
Currently employed in a job with social security	0.08	(0.26)	0.07	(0.23)	−0.0085	(0.0324)	1,781	−0.0196	(0.0329)	2,304	0.0111
Employed as an unpaid family worker	0.12	(0.36)	0.13	(0.35)	−0.0419	(0.0368)	1,924	−0.0386	(0.0575)	2,283	−0.00332
Self-employed	0.08	(0.26)	0.08	(0.27)	0.0002	(0.0258)	2,106	−0.0715	(0.0700)	2,233	0.0717

Table 1. Continued.

Characteristic	(1) Daughter		(2) Son		(3) Treatment effect: Daughter			(4) Treatment effect: Son			(5) Difference (3)–(4)
	Mean	s.d.	Mean	s.d.	T	s.e.	N	T	s.e.	N	Diff.
Fertility-related outcomes											
Birth interval after 1st birth	37.90	(26.15)	40.78	(27.22)	−2.0885	(3.3409)	1,911	2.4430	(3.8391)	1,515	−4.532
Birth interval (average all births)	48.55	(25.58)	49.72	(25.52)	0.8821	(2.9636)	1,811	3.9870	(2.9095)	2,127	−3.105
Share of girls (excluding the first-born)	0.46	(0.34)	0.50	(0.37)	−0.0259	(0.0450)	1,646	−0.0424	(0.0558)	1,316	0.0164
Fertility (number of children ever born)	3.62	(2.07)	3.36	(2.14)	0.2931	(0.2266)	2,165	−0.0046	(0.2211)	1,723	0.298
Household size	4.82	(2.18)	4.84	(2.24)	0.3200	(0.2115)	2,218	0.0574	(0.2578)	2,101	0.263
Wife-beating is acceptable if the wife:											
does any of these 4 things	0.25	(0.45)	0.22	(0.43)							
neglects children	0.17	(0.40)	0.16	(0.38)							
argues with husband	0.14	(0.36)	0.11	(0.33)							
refuses to have sex	0.08	(0.30)	0.07	(0.29)							
burns the food	0.03	(0.17)	0.03	(0.19)							

Source: Authors' analysis based on data from 2008 and 2013 Turkey Demographic and Health Survey (TDHS).

Notes Displayed are means, standard deviations (s.d.), treatment effects (T), standard errors (s.e.), and sample sizes (N). Sample sizes in estimations of the average values: 887 in panel 1 (daughter sample 36 months around the cut-off date January 1987), 1,047 in panel 2 (son sample 36 months around the cut-off date January 1987). Differing sample sizes: 863 and 1,002 for birth interval and share of girls (due to the child not having siblings), 664 and 820 for duration of employment, 885 and 1,043 for attitudes toward domestic violence (due to missing values) in panels 1 and 2, respectively. Sample sizes based on which optimal bandwidths for the "treatment effects" are estimated: 6,243 in panel 3 (daughter full sample), 6,905 in panel 4 (son full sample). Differing sample sizes: 4,986 and 5,538 for duration of employment (due to missing values), 5,029 and 5,390 for birth interval and share of girls (due to the child not having siblings) in panels 3 and 4, respectively. Averages in panel 1 and 2 are weighted by the individual's inverse sampling probability. Panels 3 and 4 show bias-corrected "treatment effects" with robust standard errors. The difference in "treatment effects" in panel 5 is marked as statistically significant if the confidence intervals of the daughters' and sons' effects do not overlap. Statistical significance is marked as follows: * 10 percent, ** 5 percent, and *** 1 percent level.

for burning food and refusing to have sexual intercourse with husband have relatively lower acceptability rates of 3 percent and 7 percent, respectively. On the other hand, arguing with husband and neglecting children are undesired actions where 13 percent and 17 percent of respondents state that a husband is justified in beating his wife. Based on these four 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; 23 percent of women in the sample consider domestic violence to be acceptable in at least one of these situations.

4. Empirical Strategy

In this section, we first present our identification strategy and then provide a series of validity checks that confirm that the identifying conditions are satisfied.

Identification Strategy

In order to estimate the effect of the expansion of compulsory schooling on treated children's mothers' attitudes toward domestic violence, we implement a regression discontinuity design based on the date of birth of respondents' first-born children. In particular, we compare attitudes of mothers whose first-born child was born in December 1986 or before (who are exposed to the five-year compulsory schooling regime) to attitudes of mothers whose first-born child was born in January 1987 or after (who are exposed to the eight-year compulsory schooling law).

For the implementation of the RDD estimation, we adopt a local nonparametric approach where we use a subsample of observations lying within a certain "optimal" bandwidth around the cut-off and estimate a local linear regression with triangular Kernel density function. We determine the optimal bandwidth using the algorithm proposed by Calonico, Cattaneo, and Titiunik (2014a), and we allow for the optimal bandwidth to vary to the right and to the left of the cut-off (Calonico et al. 2017); we refer to this bandwidth as CCT("Calonico, Cattaneo, and Titiunik"). In the robustness checks, we assess the robustness of our findings to various modifications, including the choice of bandwidth.

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

$$Y_i = \alpha + \tau T_i + f(X_i, T_i) + \varepsilon_i \tag{1}$$

$$\forall \; X_i \, \epsilon (c - h_l, \; c + h_r), T_i \equiv 1(X_i > c)$$

where $\mathbf{Y}_i$ are the attitudes toward domestic violence of respondent $\mathbf{i}$, $\boldsymbol{\alpha}$ is a constant, and $\boldsymbol{\varepsilon}_i$ is an error term. The forcing variable $\mathbf{X}_i$ is respondent's first-born child's month of birth; the cut-off $\mathbf{c}$ is January 1987. Treatment $\mathbf{T}_i$ is a binary variable, which takes the value 1 if the first-born child of the respondent $\mathbf{i}$ was born in January 1987 or afterwards, and 0 otherwise; $\mathbf{h}_r$ and $\mathbf{h}_l$ represent the CCT optimal bandwidths in our local regression to the right and to the left of the cut-off, respectively. Control function $\mathbf{f}$ is linear. The key parameter of interest is $\boldsymbol{\tau}$, which identifies the reduced form treatment effect of the change in the compulsory schooling law on the treated child's mother: that is, it corresponds to the intent-to-treat (ITT) estimate. In all our estimations, we cluster standard errors at the values of our forcing variable $\mathbf{X}_i$ to account for correlation of errors within one 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 $\mathbf{Y}$, and (3) identify problems in the empirical strategy (Imbens and Lemieux 2008). In particular, if the inclusion of covariates 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.

As mentioned above, equation (1) provides us with an estimate of the reduced form effect of the compulsory schooling reform. Ideally, we would also like to estimate the effect of children's schooling on their mothers' attitudes using an instrumental variable (IV) approach. However, information on schooling attainment of children is unavailable for a substantial portion of the sample, and it is missing in a nonrandom way. In particular, 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 at the time of the survey, the schooling information was not collected. We find that the data on educational attainment are systematically missing along two dimensions: child's gender (daughters are over-represented among children with missing schooling information) and family's socio-economic background (girls from lower socio-economic background are missing in our data disproportionately).[10] Hence, if we were to restrict the analysis only to children residing in the same household, this would induce sample selection bias in our estimates.[11] Instead, in "Two-Sample-2SLS Estimation" in section 5, we will estimate the effect of girls' schooling on their mothers' attitudes toward domestic violence by using a two-sample two-stage-least-squares (2SLS) methodology based on Inoue and Solon (2010).

Validity of the Identification Strategy

In order to show that the 1997 education reform in Turkey led to a discontinuous increase in schooling, we look at educational attainment of all household members in the 2008 and 2013 TDHS. In this representative sample, information on date of birth is available only on an annual basis. As shown by the dashed and solid lines in fig. 1, there is a clear jump in completion rates of eight-year schooling between cohorts born in 1986 and 1987. In particular, we see that the completion rates are gradually rising for cohorts born up to 1986, and then they increase discontinuously between 1986 and 1987.[12] Figure 1 also plots completion rates of five-year schooling (long dashed and short dashed lines), and shows 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 fig. 1, other studies have documented a sharp increase in schooling after the 1997 education reform in Turkey (see Cesur and Mocan 2014; Dincer, Kaushal, and Grossman 2014; Günes 2015, 2016; Gulesci and Meyersson 2016; Kırdar, Dayıoğlu, and Koc 2016; Erten and Keskin 2018).

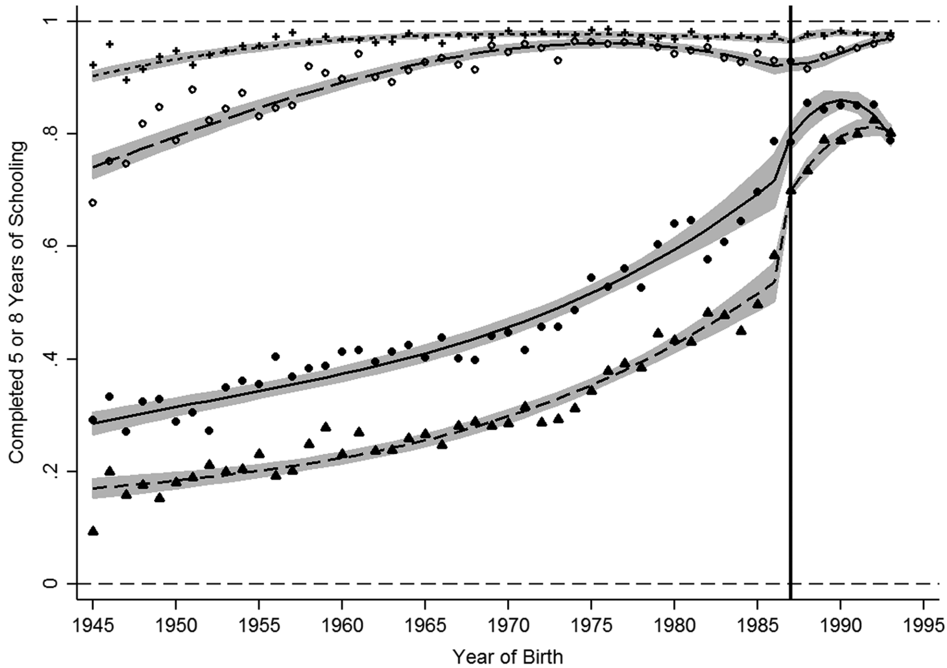
Having shown that the education reform in Turkey led to a substantial rise in education, the next question is whether individuals could have manipulated their "treatment status." We do not expect any manipulation because (1) the new law was suggested in February and adopted in August 1997, (2) it applied 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 children upon the announcement of the reform. To ascertain this expectation, we test for a discontinuity in the density of the forcing variable at the cut-off (Cattaneo, Jansson, and

10 The underlying reasons are that daughters traditionally move away from home upon marriage, age at first marriage is typically lower for women relative to men in Turkey, and girls from poorer families are more likely to marry and to leave home early.

11 This is because girls from poorer families are more likely to drop out of school at an earlier age, and therefore they are the ones who are expected to be the most affected by a reform that expands compulsory education.

12 Figure 1 also shows that while the eight-year completion rates are rising sharply between the 1986 and 1987 cohorts, they do not reach 100 percent even in cohorts fully affected by the new law. This is likely caused by insufficient or ineffective enforcement of the new compulsory schooling regime. Another noteworthy aspect is that the discontinuity seems to be spreading over cohorts born in 1987–1989. This may have been caused by an imperfect compliance with the school-starting-age rule in early 1990s when the concerned cohorts started attending primary school. Two other potentially contributing factors are grade repetition and a (suspected) delay in implementation of the new law in remote areas of Turkey. What is also visible in fig. 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 noncompliance with school-starting-age rules or due to grade repetition.

Figure 1. Proportion of Household Members Who Have Completed at Least 5 or 8 Years of Education, by Gender and Year of Birth



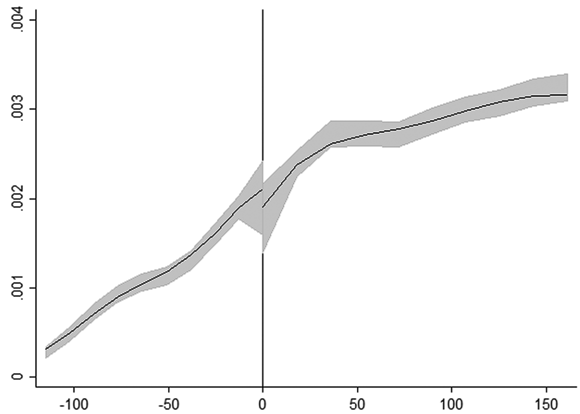
Source: Authors' analysis based on data from 2008 and 2013 Turkey Demographic and Health Survey (TDHS).
Note: Dashed (triangle) and solid (circle) lines represent women and men with at least eight years of education, respectively. Long dashed (hollow circle) and short dashed (plus) lines denote women and men with at least five years of education, respectively. Points represent fractions of individuals who went to school for eight or five years; fractions are calculated annually by individual's year of birth. Lines are predicted probabilities to have completed at least eight or five years of schooling stemming from a simple linear probability model; the OLS regression is segmented into (1) years up to 1986 and (2) years 1987 and later; the cubic control function for year of birth is allowed to differ by segment and by gender. Grey areas represent 95 percent confidence intervals corresponding to the predicted probabilities. Standard errors are clustered at the year level. Sample covers all 26,504 and 24,799 household members in the 2008 and 2013 TDHS, respectively, who were born before 1994. Year of birth is calculated as the difference between survey year and individual's reported age; interviews were conducted between October and December 2008, and between September 2013 and January 2014. For presentational reasons, birth years 1913–1944 are not shown in the graph.

Ma 2017).[13] Figure 2 shows no evidence of such a discontinuity; with a p-value of 0.30 we cannot reject the null hypothesis that there is no discontinuity in the forcing variable at the cut-off. Figure S1.1 in the supplementary online appendix shows that there are no such discontinuities in the daughter and son samples either. 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 and ex-post family characteristics exhibit a discontinuity at the cut-off. Table 1 shows the results of estimating equation (1) on respondents' background characteristics, separately for the daughter and the son samples. For the daughter (son) sample, only 2 (4) out of the 30 background characteristics we examine display a statistically significant difference around the cut-off. Moreover, none of the differences in the "treatment effects" in table 1 are statistically significantly different in the daughter and the son samples (table S1.2 in the supplementary online appendix shows that the 95 percent confidence intervals are overlapping in all cases; the 90 percent confidence intervals overlap as well, results not shown). This implies that the samples are well balanced and the respondents in the daughter and son samples are comparable in terms of their observable characteristics around the cut-off. Nevertheless, in our robustness checks, we will control for

13 The test proposed by Cattaneo, Jansson, and Ma (2017) is similar to the McCrary (2008) test, but it requires choosing fewer tuning parameters.

Figure 2. Test of Discontinuity in the Forcing Variable at the Cut-Off



Source: Authors' analysis based on data from 2008 and 2013 Turkey Demographic and Health Survey (TDHS)
Notes: The figure displays the density function (y-axis) of the forcing variable (x-axis) with 95 percent robust confidence intervals. The forcing variable is the difference between individual's month of birth and January 1987, measured in months. With a *p*-value of 0.30, we cannot reject the null hypothesis that there is no discontinuity in the forcing variable at the cut-off. Estimation based on 13,148 observations.

respondent's predetermined and ex-post (i.e., after the birth of her first child) family characteristics. As discussed above, this will provide an additional test of the internal validity of our estimates.

5. Results

This section presents first the main RDD results, followed by a number of robustness checks and results stemming from a two-sample-2SLS estimation.

Main Results

First, we present our results regarding the causal impact of children's exposure to education reform on their mothers' attitudes toward 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 situations presented to her – from now on referred to as the aggregate indicator. Table 2 shows results from our baseline specification.

The first row of table 2 shows that the effect of a child's exposure to the reform on her/his mothers' acceptance of wife-beating is – 4.4 percentage points (ppt) and statistically insignificant. This average effect masks substantial heterogeneity by the gender of the child: There is a large and statistically significant effect of girls' exposure to higher compulsory schooling on their mothers' attitudes, while the effect for boys is imprecise. In particular, the acceptability of any type of domestic violence decreases by 12.4 ppt if the daughter was exposed to the compulsory schooling reform. This constitutes a 43 percent drop with respect to the average acceptability rate of domestic violence in the sample.[14] In the son sample, the effect is opposite in sign, but the point estimate is small (1.2 ppt) and imprecisely estimated at conventional levels. Figure 3 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 in table 2. The figures confirm the patterns observed in table 2 – there is no significant jump around the cut-off date in the full sample nor in the son sample, while in the daughter sample there is a large and significant decrease at the cut-off.

14 See the last column of table 2 for means of the outcome variable among women in the control group whose first child was born up to 36 months before January 1987.

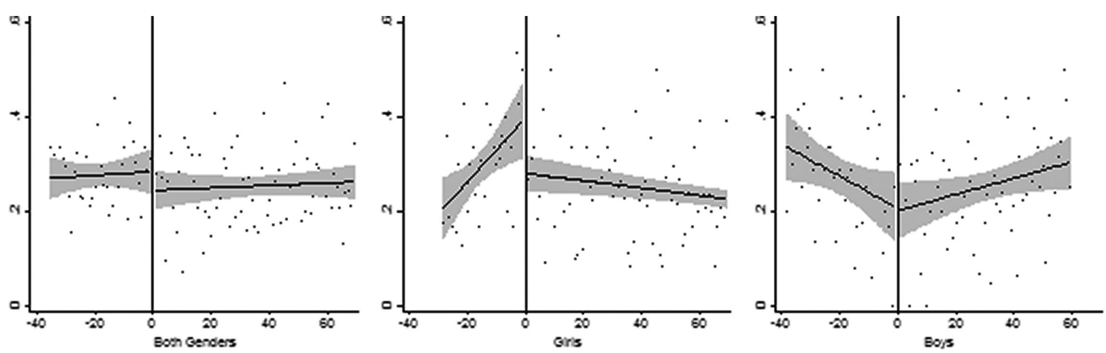
Table 2. Treatment Effects of a Child's Exposure to Education Reform on Mother's Attitudes Toward Domestic Violence

Indicator		Sample	Local linear approach T	s.e.	N	N left	N right	h left	h right	Sample mean
Wife-beating is acceptable in any of the 4 situations		Child	−0.0444	(0.0326)	3,156	817	2,339	35	69	0.277
		Daughter	−0.1242**	(0.0555)	2,373	333	2,040	28	121	0.289
		Son	0.0123	(0.0494)	1,516	454	1,062	38	59	0.265
Wife-beating is acceptable if the wife…	neglects children	Child	−0.0586*	(0.0348)	2,898	845	2,053	38	61	0.204
		Daughter	−0.1132*	(0.0608)	2,487	339	2,148	29	127	0.214
		Son	−0.0069	(0.0442)	1,275	374	901	30	52	0.195
	argues with husband	Child	−0.0283	(0.0313)	3,600	837	2,763	36	82	0.146
		Daughter	−0.0800*	(0.0435)	3,259	367	2,892	34	163	0.163
		Son	0.0085	(0.0356)	1,720	502	1,218	45	68	0.130
	refuses to have sex	Child	−0.0041	(0.0240)	4,837	946	3,891	44	111	0.100
		Daughter	−0.0517	(0.0391)	2,493	397	2,096	37	124	0.101
		Son	0.0342	(0.0373)	2,178	440	1,738	37	97	0.100
	burns the food	Child	−0.0088	(0.0165)	3,188	966	2,222	45	67	0.038
		Daughter	−0.0307	(0.0238)	2,331	357	1,974	32	116	0.031
		Son	−0.0100	(0.0194)	1,153	483	670	43	39	0.044

Source: Authors' analysis based on data from 2008 and 2013 Turkey Demographic and Health Survey (TDHS).

Notes: The table reports the estimates of discontinuity (T) at the cut-off in a nonparametric local linear regression with triangular Kernel function. The forcing variable is the month of birth of the first-born child (or daughter or son) of the respondent, and the cut-off date is January 1987. The sample sizes (N, N left, N right) are determined by Calonico et al. (2017) optimal bandwidths (h left, h right). Standard errors (s.e.) 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 1 to 36 months before January 1987. Statistical significance is marked as follows: * 10 percent, ** 5 percent, and *** 1 percent level.

Figure 3. Treatment Effects of Child's Exposure to Education Reform on Mothers' Attitudes Towards Domestic Violence



Source: Authors' analysis based on data from 2008 and 2013 Turkey Demographic and Health Survey (TDHS)

Note: Dots represent fractions of respondents who deem domestic violence acceptable in at least one of the four 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 local linear regression. Grey areas represent 95 percent confidence intervals corresponding to the predicted probabilities. Standard errors are clustered at month of birth level. Graphs correspond to estimations shown in table 2. The forcing variable is the difference between child's month of birth and January 1987, measured in months.

Next, we examine which particular components or situations are driving the result. The lower panel of table 2 displays the ITT treatment effects on the different components of the aggregate indicator. We see that all four components of the indicator display negative treatment effects in the daughter sample, but only two of them are statistically significant: neglecting children and arguing with husband. The remaining two components (refusing to have sex, burning food) also show a negative treatment effect, but these are imprecisely estimated at conventional levels. Interestingly, the effect sizes are extremely similar for all dependent variables (roughly 40–50 percent of the sample mean) in the daughter sample.[15] Finally, acceptability of domestic violence also decreases significantly in the overall "child" sample in one of the situations – if wife neglects children – which is the situation with the highest baseline acceptance rate. The estimated effect is 5.9 ppt (or 29 percent of the sample mean).

To summarize, we find that girls' exposure to the education reform had a large and significant effect on their mothers' attitudes toward domestic violence. The effects are found for both the aggregate measure and individual components; the effect sizes are around 40–50 percent of the sample mean. The effects of boys' exposure to the education reform are insignificant.

Robustness Checks

As a first robustness check, we examine the impact of the bandwidth choice on estimated treatment effects in a local linear regression. In fig. S1.2 in the supplementary online appendix, we plot estimates stemming from 37 different regressions where the bandwidth on one side of the cut-off is held constant (optimal CCT bandwidth) and the bandwidth on the other side varies between 12 and 120 months. The figure also shows the optimal bandwidths from the main specification (vertical line) and the sample sizes (dashed line). Across the board, the point estimates of the treatment effect do not vary dramatically with the bandwidth size. For girls, the estimates are consistently negative whereas for boys, the sign of the estimate changes, and the effects are always insignificant.

The second set of robustness checks is presented in table S1.3 in the supplementary online appendix. We present a number of variations in our regression approach:

(a) adding predetermined and ex-post (i.e., after the child's birth) covariates (Calonico, Cattaneo, and Titiunik 2018);
(b) calculating bias-corrected estimates with robust standard errors (Calonico, Cattaneo, and Titiunik 2014a,b);
(c) imposing the same optimal bandwidth on both sides of the cut-off;
(d) calculating optimal bandwidth with a different algorithm (coverage-error-rate optimal bandwidth instead of mean-squared-error optimal bandwidth (Calonico et al. 2017);
(e) applying different Kernel functions (uniform and Epanechnikov);
(f) controlling for a local quadratic function of the forcing variable;
(g) estimating a parametric higher-order polynomial OLS regression 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).[16]

Across the board, the treatment effects for the aggregate indicator are fairly stable. The point estimates vary between 0.09 and 0.16 and remain statistically significant with only one exception.[17] Importantly, the inclusion of covariates does not decrease the estimated coefficient, but on the contrary the point estimate

15 Situations that involve burning food are an exception, but the reported baseline acceptability of domestic violence in this situation is extremely low (3 percent).

16 According to Gelman and Imbens (2018), the higher-order polynomial approach in the full sample is inferior to the local linear approach because (1) it gives too much weight to observations far away from the cut-off, which leads to sensitivity of this technique to remote observations; (2) the method is sensitive to the order of the polynomial chosen, and (3) confidence intervals are estimated too narrowly, thus leading to over-rejections of null hypotheses when testing statistical significance of coefficients.

17 The exception is that when we impose the same bandwidth on both sides of the cut-off, the estimate is no longer significant at conventional levels. We argue that in our specific case, this is a suboptimal approach and it is preferable to allow for different bandwidths on each side of the cut-off for two reasons. First, we have many fewer observations to the left of the cut-off than to the right. Therefore, allowing the bandwidth to be wider on the right-hand side, where more observations are, improves the precision of the estimates considerably. Second, the CCT algorithm selects a bandwidth that minimizes the mean squared error (MSE) which in turn is defined as the sum of the variance of the treatment effect

for the treatment effect increases slightly (the difference is statically insignificant). Similarly, all four of the components of the aggregate indicator have robust effects to the different specifications. In particular, all four components consistently display negative treatment effects in the daughter sample, but only two of them are statistically significant across the different specifications: neglecting children and arguing with husband.

Third, we examine whether our choice to treat “don’t know” answers as acceptability of domestic violence (rather than omitting these observations from the analysis) has any influence on the results, and we find no such evidence (see table S1.4 in the supplementary online appendix). Finally, we examine the sensitivity of our estimates to the inclusion of inverse sampling weights. Table S1.5 in the supplementary online appendix shows that weighting by the individual’s inverse sampling probability doesn’t change our estimates qualitatively, and it marginally increases the magnitudes of our point estimates.

Two-Sample-2SLS Estimation

The results thus far correspond to the reduced form effects of the reform. As discussed in “Identification Strategy” in section 4, we cannot estimate the effect of children’s schooling on their mother’s attitudes using a standard IV approach because the information on children’s schooling is available only for a selected sample of children still living with the respondent. As an alternative, we use schooling data of the respondents (i.e., ever-married women in 2008 and all women in 2013 TDHS) to estimate the first stage parameter for the daughter sample, i.e., the effect of the reform on schooling level of the treated cohorts of women. Then, we apply the two-sample-2SLS estimation method (Inoue and Solon 2010) to estimate the structural parameter. This 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 (i.e., structural equation parameter) 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.

Table 3 displays the results. The reduced form point estimate in the first panel corresponds to the main effect from table 2 (“first sample”). The second panel shows the first stage estimates stemming from the “second sample.”[18] In these regressions, we estimated the effect of the treatment variable (respondent was born in January 1987 or later) on three measures of schooling: (1) years of schooling, (2) years of schooling capped at 8 years (i.e., 0–8), and (3) years of schooling capped at 13 years (i.e., 0–13).[19] Given that the instrument (respondent’s date of birth) is arguably exogenous and has a positive and highly

estimator and its squared bias (Calonico, Cattaneo, and Titiunik 2014b). The bias stems from the fact that a potentially nonlinear relationship between Xi and Yi is approximated by a linear control function within the chosen bandwidth; see equation (1). The more curvature there is in the relationship between the forcing variable and the outcome variable, the smaller the bandwidth that should be selected. We find that in our case, the relationship is best captured by a cubic polynomial on the left-hand side, while on the right-hand side of the cut-off it is closer to a linear one. This is the main reason why the CCT optimal bandwidth algorithm selects a wider optimal bandwidth to the right-hand side (less smoothing bias) than to the left of the cut-off (more misspecification error due to a nonlinear underlying relationship).

18 One caveat with this analysis is that the respondents in the 2008 TDHS are ever-married women (as opposed to the 2013 TDHS where the sample includes all women). Given that respondents born around the cut-off date (January 1987) were approximately 21 years old at the time of the survey in 2008, our first-stage estimation is partly based on a sample of women who married earlier than the average woman in the country: the average age at first marriage in the full sample is 19.8 years while in the estimation sample (of ever-married women born 36 months around the cut-off date) it is 17.9 years in 2008 TDHS. As women who are married early tend to come from poorer, typically rural communities, the effect of the reform is likely to be larger on them. Nevertheless, 2013 TDHS mitigates this problem through inclusion of all women.

19 The second and third measure of education caps years of schooling at 8 and 13 years, respectively, in order to take into account that younger respondents did not have enough time to complete as many years of education as their older counterparts. For the same reason, the first two measures are defined only for respondents 15 years or older

Table 3. Two-Sample-2SLS Estimation

Estimation method		T	s.e.	N	h left	h right
Reduced form (main sample)		−0.1242**	(0.0555)	2,373	28	121
First stage (different sample)	Years of schooling	0.9313***	(0.2665)	17,130	93	39
	Years of schooling (capped at 8 years)	0.5117***	(0.1862)	17,130	72	52
	Years of schooling (capped at 13 years)	0.7915**	(0.3236)	15,398	93	26
Two-sample-2SLS estimate	Years of schooling	−0.1333*	(0.0707)			
	Years of schooling (capped at 8 years)	−0.2427*	(0.1398)			
	Years of schooling (capped at 13 years)	−0.1569*	(0.0950)			

Source: Authors' analysis based on data from 2008 and 2013 Turkey Demographic and Health Survey (TDHS).
Notes: Displayed are treatment effects (T), standard errors (s.e.), sample sizes (N), and Calonico et al. (2017) optimal bandwidths (h left, h right). Reduced form estimates correspond to the main results from table 2. First-stage estimates are based on the sample of 15–49-year-old ever-married women in the 2008 TDHS and 15–49-year-old women in 2013 TDHS. The dependent variables in the first stage measuring education are: (1) years of schooling, (2) years of schooling capped at 8 years (i.e., 0–8), and (3) years of schooling capped at 13 years (i.e., 0–13). The first two measures are defined only for respondents 15 years or older (who were able to complete 8 years of schooling in 2008) and the last measure is defined only for women 20 years or older (who were able to complete 13 years of schooling in 2008). Standard errors are clustered at values of the forcing variable and displayed in parentheses. Statistical significance is marked as follows: * 10 percent, ** 5 percent, and *** 1 percent level.

significant effect on all three measures of education, the IV approach is valid. The resulting two-sample-2SLS estimates in the third panel display the expected signs, are statistically significant, and in terms of magnitudes they are very similar to those estimated in our main local linear specification in table 2. Thus, the two-sample-2SLS regression results show that the effect of daughters' exposure to the compulsory schooling reform is very similar to the effect of daughters' education on their mothers' attitudes toward domestic violence.

6. Discussion

In this section, we first discuss a number of issues that are important to consider when interpreting our findings. Second, we reflect on potential mechanisms through which girls' longer exposure to compulsory schooling in Turkey may have affected their mothers' attitudes toward domestic violence.

Interpretation of the Findings

First, we reflect on the decision to include only first-born children in our analysis. 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 on how far the first-born child's birth date is from January 1987, how big the birth intervals between the first-born and later-born children are, and how large the bandwidth is. If the untreated first-born children were born close enough to the cut-off and/or if the subsequent birth interval was sufficiently large, then all of their younger siblings were treated. Under this assumption, our discontinuity coefficient, which is estimated precisely at the cut-off, represents a marginal effect of having *one less child* educated for eight years. For example, when looking at a treated and untreated family with four children each, we are effectively comparing the effect of having 100 percent of children exposed to eight-year compulsory schooling as opposed to having 75 percent of children exposed. Hence, any treatment effect that we detect comes from the 25 percent exposure rate. In this sense, we might be estimating a lower bound of the true treatment effect in all families except for

(who were able to complete 8 years of schooling in 2008), and the last measure is defined only for women 20 years or older (who were able to complete 13 years of schooling in 2008).

those with one child.[20] 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 percent. If we assumed that the treatment effect is linear in the number of children exposed, the "full" treatment effect should be 2.8 times larger than what we estimated. However, it is not straightforward to assume a linearity in this respect as firstborns may have a larger impact on their parents' lives than later-born children.[21]

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 eight years of schooling. And, in fact, this is what we saw in fig. 1 – completion rates increased discontinuously at the cut-off, but they did not reach 100 percent. Additionally, due to the fact that staying in school for eight years was also possible (and fairly common) on a voluntary basis 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 2SLS approach, and we have shown in "Two-Sample-2SLS Estimation" in section 5 that our reduced form estimate using the RDD is a valid approximation of the treatment effect.

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 the potential for extrapolation are limited. In terms of internal validity, we are not aware of any parallel policy changes that would differently impact individuals born in December 1986 and January 1987. Additionally, due to the "retroactive" nature of the education reform, we do not expect any manipulation of the treatment status. Nevertheless, we have seen that the compliance was not 100 percent. In terms of external validity, it is important to keep in mind the specific context in which the reform was implemented. On the one hand, Turkey is a majority-Muslim country, and traditional gender norms might be more prevalent there than in other contexts. 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 documented effects of girls' education on their mothers' attitudes toward domestic violence may not be generalizable to other settings.

Finally, the estimated effects could be interpreted as an indication of a more general shift in gender-related attitudes. In order to explore this possibility, we estimated the treatment effects of girls' schooling on their mothers' opinions about gender roles. In particular, the 2008 and 2013 TDHS contained five statements related to gender norms, and the respondents were asked if they agreed or disagreed with these statements.[22] We tested if mothers of treated girls were less likely to agree with gender-biased statements compared to mothers of nontreated girls. Table 4 shows the results. Out of the five individual statements, we find no significant treatment effect in any dimension. This implies that the increase in girls' schooling caused by the reform did not have a considerable effect on their mothers' attitudes toward gender norms in general. As such, there is no evidence of a general shift in the girls' mothers' opinions about gender. The effect is specific to their opinions regarding domestic violence.

20 We attempted to estimate the "exact" treatment effect in one-child families only. Despite the fact that 22 percent 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 the cut-off, i.e., only 26 women in our sample had an only child that was born prior to January 1987.

21 As discussed in section 3, limiting the analysis to first-born children has the advantage of avoiding potential sample selection bias, which is likely to be higher for higher-order births. Nevertheless, we can estimate the treatment effects for parities two and three. The results are reported in table S1.6 in the supplementary online appendix, and they show that there are no significant effects for these higher birth orders. While this could be due to the selection issue discussed, it may also be driven by parents reacting more to the experiences of their first-born children relative to higher birth orders.

22 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." •"It is better to educate a son than a daughter." •"Women should be more involved in politics." •"Women should be virgins when they get married."

Table 4. Treatment Effects of Girls' Exposure to Education Reform on Their Mothers' Opinions about Gender Roles

Statement	Local linear approach							Sample
	T	s.e.	N	N left	N right	h left	h right	mean
Men should also do the housework like cooking, washing, ironing, and cleaning. (Yes = 1)	0.0376	(0.0645)	2,251	339	1,912	29	114	0.85
The important decisions in the family should be made only by men of the family. (No = 1)	−0.0054	(0.0609)	2,000	357	1,643	32	100	0.67
It is better to educate a son than a daughter. (No = 1)	0.0524	(0.0523)	2,869	339	2,530	30	145	0.91
Women should be more involved in politics. (Yes = 1)	0.0665	(0.0591)	1,565	242	1,323	21	83	0.72
Women should be virgins when they get married. (No = 1)	0.0175	(0.0339)	1,706	500	1,206	48	77	0.18

Source: Authors' analysis based on data from 2008 and 2013 Turkey Demographic and Health Survey (TDHS).

Notes: The table reports estimates of discontinuity (T) at the cut-off in a nonparametric local linear regression with triangular Kernel function. The forcing variable is the month of birth of the first-born daughter of the respondent and the cut-off date is January 1987. The sample sizes (N, N left, N right) are determined by the Calonico et al. (2017) optimal bandwidths (h left, h right). Standard errors (s.e.) 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 1 to 36 months before January 1987. 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. Statistical significance is marked as follows: * 10 percent, ** 5 percent, and *** 1 percent level.

Potential Mechanisms

There are a number of potential mechanisms through which an increase in girls' exposure to compulsory schooling in Turkey may have led to a reduction in their mothers' likelihood to find domestic violence acceptable. First, teenage children may affect the life of their families.[23] To the extent that additional schooling acquired by daughters due to the reform changed their own attitudes toward domestic violence (Friedman et al. 2015), they may actively engage with their mothers in order to try and amend their positions. We call this the "active persuasion" channel. In the TDHS data, we find only a small and insignificant effect of the reform on girls' own attitudes toward domestic violence (see table S1.7 in the supplementary online appendix).[24] Therefore, we conclude that, in our context, this channel is unlikely to be the one driving the effect.

Second, a growing literature on the role of domestic violence as an instrument of intrahousehold 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, Witte, and Long 1991; Bloch and Rao 2002; Eswaran and Malhotra 2011; Anderson and Genicot 2015).[25] Thus, if the reform had any spillover effects on the labor market

23 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). Washington (2008) shows that U.S. legislators with daughters vote more women-friendly on reproductive rights, and Warner (1991) and Warner and Steel (1999) find that U.S. and Canadian parents with only daughters are more likely to hold feminist views.

24 Using a different dataset, Erten and Keskin (2018) also find that the reform had no discernable effect on girls' attitudes toward domestic violence in Turkey.

25 Empirical literature testing these predictions studied how employment or earning opportunities of women may influence the incidence of domestic violence across a variety of settings (Aizer 2010; Alesina, Brioschi, and La Ferrara 2016; Andenberg et al. 2016; Chin 2012; Heath 2014; Angelucci 2008; Bobonis, Gonzalez-Brenes, and Castro 2013; Amaral, Bandyopadhyay, and Sensarma 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.

outcomes of the treated girls' mothers, [26] it may have also affected mothers' own exposure to and attitudes toward domestic violence. We call this the "economic empowerment" channel. When we test for the effect of the reform on the labor market outcomes of the girls' mothers, we find no significant effect in any dimension.[27] 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 their own exposure to domestic violence, this may influence their mothers' attitudes toward wife-beating through parental empathy.[28] Erten and Keskin (2018) show that the increase in women's education caused by the reform increased the likelihood that they experience psychological abuse and financial controlling behaviors by their spouses. To the extent that parents react to their daughters' distress and perceive physical abuse to be at least as distressing as psychological violence, this may have led to parents changing their attitudes toward domestic violence. Our findings that the effect is coming only from girls' and not from boys' compulsory schooling is in line with this, and so is the fact that the change in mothers' attitudes is specific to domestic violence, and not to gender norms (table 4). This is merely suggestive evidence, and our data do not allow us to test directly for the parental empathy mechanism. Exploring the role of this and other potential mechanisms through which children's schooling may affect their parents' attitudes can be a promising avenue for future research.[29]

7. Conclusion

In this study, we examined the question of whether education of the younger generation has spillover effects on their parents' attitudes. In particular, we exploited a compulsory schooling reform in Turkey that increased the legal requirement of schooling from five to eight years, and we tested if mothers whose first-born child was exposed to higher compulsory schooling because of the reform have different attitudes toward domestic violence. We find 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.

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,

26 For example, an increase in compulsory schooling may have reduced the need for child care 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), or it would allow mothers who were not employed (e.g., due to child care duties with younger children) to join the labor force once the younger children start schooling and the older children stay in school for longer.

27 The "treatment effects" on labor market outcomes estimated in table 1 showed that mothers of treated girls had similar employment rates, occupations, and cumulative employment durations as those in the control group.

28 A large literature in psychology shows that parents, in particular mothers, may be significantly distressed when they observe their children in painful situations (Guttman and Laporte 2000; Stern, Borelli, and Smiley 2014; Goubert et al. 2006; Caes et al. 2012) 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 (Voors et al. 2012; Bauer et al. 2016). 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.

29 There may be other mechanisms that could explain our main results. However, any such mechanisms would need to explain (1) why we see an effect through girls' and not boys' exposure to higher compulsory schooling, (2) the fact that the effect is only on attitudes toward domestic violence and not on gender norms in general. For example, one such channel could be that if children have to go to school longer, this may lead to financial difficulties for the households. This effect might be particularly strong for families with daughters since older daughters typically help with child care by, for example, taking care of their younger siblings. If older daughters go to school for a longer period of time, the reduction in child care that they would otherwise provide might bring additional financial burden on these families, which may then lead to an increase in domestic violence. This mechanism assumes that mothers experiencing wife-beating change their attitudes toward domestic violence. We thank an anonymous referee for suggesting this potential mechanism.

and they may affect the older generation as well. Previous literature has demonstrated spillover effects of girls' education on younger generations (e.g., child health) but there is limited causal evidence of upward intergenerational spillover effects of girls' schooling. 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 intergenerational spillover effects should study this mechanism more carefully, and explore other channels through which children's schooling may affect their parents' outcomes.

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