Breaking the Cycle?
Education and the Intergenerational Transmission of Violence[*]

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Abstract

We estimate the causal effects of education on the intergenerational transmission of violence against children by exploiting an extension of compulsory schooling in Turkey. Using a regression-discontinuity design, we find that the reform increased maternal education by one year, with stronger effects for women raised in rural areas. The increase in education among rural women led to a reduction in the perpetration of child physical abuse but only by mothers who were physically abused by their own families during childhood. Exploring potential channels, we document that these women were also more likely to experience improved mental health outcomes.

JEL Classification: J12, J13, I25

Keywords: Intergenerational transmission of violence, child physical abuse, education, mental health, regression discontinuity

1 Introduction

The cycle of violence—the propensity of parents who were exposed to physical maltreatment in their childhood to maltreat their own children—is an fundamental social problem that is pervasive, costly, and difficult to solve.[1] First, the sheer extent of physical abuse against children by their caregivers in everyday settings is striking: in the developing world, 8 of 10 children aged 2-14 years are routinely subjected to violence at home (Figure A1). The overall prevalence of child physical abuse in developed countries, such as the United States is also startling, with more than 680,000 children (9.2 per 1,000) experiencing maltreatment by their caregivers in a given year (Centers for Disease Control and Prevention 2014) and roughly 25 percent of children experiencing some form of child maltreatment in their lifetimes (Finkelhor et al. 2013). Second, even moderate forms of violence can have important economic and public health consequences.[2] Third, the intergenerational transmission of violent behaviors renders it particularly difficult to design effective policies for preventing child maltreatment.

In this paper, we investigate whether compulsory schooling policy can be an effective means of breaking the cycle of violence by increasing the education of mothers. The impact of female schooling on the perpetration of child abuse is a priori ambiguous. Additional years of schooling may change the attitudes of women toward children by fostering healthy relationships with better role models or improve their mental health by teaching them how to cope with emotional dysfunctions, resulting in a lower future maltreatment risk.[3] However, education may also introduce other stressors into a parent's daily life, including a change in occupation and a decline in the time available for caring for children, which may increase the risk of child maltreatment.

The central contribution of this paper is to present the first causal evaluation of the effect of an exogenous increase in education on the risk of physical abuse against children by mothers in a developing country, Turkey, which has a high prevalence of violence against children and a high approval of using such violence as a disciplinary instrument. Previous studies relying on

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[1]Several studies in the psychology literature document a strong positive correlation between exposure to childhood violence and child physical abuse later in life (Kaufman and Zigler 1987; Heyman and Slep 2001; Newcomb and Locke 2001; Pears and Capaldi 2001; Milner et al. 2010).

[2]In the early years of a child's life, maltreatment is associated with changes in brain functioning (Cicchetti and Rogosch 2001), developmental delays (Veltman and Browne 2001), acute stress (Agnew 2005), and poor academic performance (Kendall-Tackett and Eckenrode 1996). Child physical abuse is also associated with an increased likelihood of interpersonal violence, including peer aggression (Benda and Corwyn 2002), intimate partner violence (Reitzel-Jaffe and Wolfe 2001), and adult sexual assault (Merrill et al. 2001), as well as a higher probability of engaging in criminal activities, such as burglary or armed robbery (Currie and Tekin 2012). Adults who were abused as children are more likely to report depression, suicidal thoughts, alcohol and substance abuse, multiple sexual partners, sexually transmitted diseases, and unintended pregnancies (Dube et al. 2003; Hillis et al. 2004). In these studies, the definition of child physical abuse encompasses a range of actions intended to cause physical pain, including hitting, shaking, slapping, spanking, or beating the child.

[3]The importance of emotional cues on precipitating family violence has also been well-documented in economics literature (Card and Dahl 2011).

raw correlations between education and child physical abuse are likely to suffer from omitted variable bias, as unobservables such as socioeconomic status, upbringing, and ability may influence both educational attainment and the risk of child maltreatment.[4] In contrast to these papers, we use a regression-discontinuity (RD) design to estimate the causal effects of an extension of compulsory schooling from five to eight years on the risk that mothers will perpetrate child physical abuse. Having established the overall impact of increased schooling, we analyze whether the compulsory schooling reform had heterogeneous effects on a mother's behavior based on her experience of physical maltreatment in childhood. In particular, we investigate whether an exogenous increase in mothers' education during adolescence reduces the intergenerational transmission of violence against children.

We then examine several channels through which an increase in mothers' education may affect the intergenerational transmission of child maltreatment. Among these channels, two are particularly pertinent due to their effects on the high-risk group of women with a history of childhood violence. The first entails potential changes in attitudes toward violence. According to social learning theory, individuals with an early childhood experience of maltreatment have a greater tendency to perpetrate violence against their own children because they learn from their parents that such behavior is a legitimate way of resolving disputes (Bandura 1971). Through imitation of violent parental behavior, these individuals develop social norms that legitimize the use violence against children for discipline (Widom 1989).[5] However, if socialization within the family is one environment in which individuals acquire social norms of appropriate behavior, another sphere of early socialization is the school environment, where individuals may be exposed to a different set of attitudes through their teachers and peers (Bisin and Verdier 2011). This exposure to different attitudes and engagement with alternative role models may result in a change in attitudes, including attitudes toward violence. To the extent that corpo-

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[4]Several studies report a negative correlation between education and violence against children. Straus et al. (1980) find that in the United States, physical abuse of children decreases as the educational levels of the parents increase. Using data from the 1992 and 1994 National Longitudinal Survey of Youth (NLSY), Eamon (2001) documents a negative correlation between the mother's education and child physical punishment and argues that the mother's knowledge of alternative child disciplinary practices reduces the probability that she uses physical punishment. Relying on a small sample of 81 married and/or cohabiting two-parent families of preschoolers in the United States, Tucker et al. (2017) finds that lower educational attainment of mothers and fathers is significantly correlated with higher levels of child physical abuse. Using a self-administered survey of violence against children in Turkey, a report by Bernard van Leer Foundation (2014) finds that 32 percent of mothers with a primary school education perpetrate child physical abuse, while this proportion falls to 21 percent for mothers who completed junior high school, 19 percent for those who completed high school, and 14 percent for those who completed university education.

[5]A similar idea is highlighted by Pollak (2004) using a theoretical model that explains the intergenerational transmission of intimate partner violence in which violent behaviors are transmitted from parents to children. By assuming that men raised in violent homes are more likely to be violent as adults and that women raised in such homes are more likely to tolerate violence, Pollak (2004) shows that the marriage market plays a key role in the intergenerational transmission of intimate partner violence.

ral punishment is not an acceptable form of behavior in schools,[6] additional years of female education may result in a higher probability that women with a history of maltreatment will disapprove of violent behavior toward children, resulting in a lower risk of child maltreatment.

The second particularly important channel is potential changes in mental health. Attachment theory, which explains how the parent-child relationship emerges and influences later character development, predicts that physical abuse may lead children to develop internal working models of the world as a threatening place and to fail to encode benign social cues, thus leading them to become hypervigilant toward actions that they misread as threatening (Bowlby 1973; Crittenden and Ainsworth 1989). Similarly, trauma models of violence also focus on traumatic symptoms that are generated by being subjected to violence as a child. A history of childhood maltreatment, among other trauma symptoms, may also compromise children's ability to regulate emotions, make them more impulsive and therefore increase the probability of maltreatment perpetration (Neller et al. 2005; Pomeroy et al. 1995). However, additional years of schooling during adolescence may improve the mental health of traumatized individuals by teaching them to better regulate their emotions, which in turn may reduce the likelihood of maltreatment perpetration.[7] Education may function as a coping resource, facilitate effective problem solving, and reduce the probability of experiencing depression (Kessler 1982; Ross and Mirowsky 1989). A reduction in maternal depression may, in turn, reduce the risk of child physical abuse (Eamon 2001). If schooling allows women to better encode social cues and become less impulsive in their reactions to their children, such improvements in maternal mental health may also reduce the risk of child maltreatment.

While the attitude and mental health channels are particularly important in preventing the transmission of violent parental behaviors across generations, we also explore the impact of other channels through which education may affect child maltreatment, including changes in fertility, labor market outcomes, partner characteristics and marriage market outcomes and exposure to spousal violence. In particular, we examine whether education has a differential impact on these outcomes for the high-risk group of mothers with a history of childhood violence.

Our empirical approach exploits the rollout of the 1997 Basic Education Program in Turkey,

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[6]In the context of Turkey, corporal punishment is legally prohibited, and teachers face disciplinary action if they use corporal punishment against students.

[7]Recent evidence from the neuroscience literature indicates that effects of education on socio-emotional skills are more pronounced during adolescence since the prefrontal cortex of the brain is still in the process of development (Fuster 2013; Egana Del-Sol 2017). Studies in the medical literature also provide some limited evidence that changes in the school environment improve adolescent mental health and reduce violent behaviors by enhancing problem solving in conflict situations (Kidger et al. 2012; Bonell et al. 2013). Recent experimental evidence also shows that adolescent participants in an after-school program had a significant improvement in emotional regulation and socio-emotional skills associated with a reduction in violent behaviors (Dinarte and Egana Del-Sol 2017). Moreover, recent studies find that the impact of exposure to war on maternal health vary with age of exposure, with the largest impacts being observed during adolescence (Akresh et al. 2017).

which increased mandatory school attendance from five to eight years, to study the impacts of increased exposure to education on the intergenerational transmission of child abuse.[8] We use the 2014 National Survey on Domestic Violence against Women in Turkey (NSDVW 2014), which includes information on physical violence against children, history of childhood abuse, mental health, child behavior indicators, attitudes toward violence, fertility decisions, and labor and marriage market indicators. To estimate the causal effect of education on the intergenerational transmission of violence against children, we employ an RD design that allows us to test whether exposure to higher levels of education has an impact on women who have experienced physical abuse during childhood and therefore have a higher risk of perpetrating violence against their children. Given that the required age for beginning junior high school in Turkey is twelve, the expansion of compulsory schooling in 1998 implied that individuals born before January 1987 could drop out after five years, whereas those born after January 1987 had to complete eight years of education (Cesur and Mocan 2014; Dincer et al. 2014). Our identifying assumption is that these two cohorts, born one month apart, display no systematic differences other than whether they were exposed to the compulsory schooling law.

We find that the reform led to an increase of roughly one year of additional schooling for women on average. The main compliers with the reform were women who grew up in rural regions.[9] Our findings reveal that while the reform had no significant impact for the population as a whole, it decreased the likelihood of perpetrating maltreatment for the high-risk group of women who experienced abuse when they were children and were raised in rural areas. After quantifying the impacts of education on the prevalence of child abuse in this high-risk group, we explore the potential mechanisms underlying this effect. We find that women in the treated cohorts and with a history of childhood abuse were more likely to experience an improvement in their mental health outcomes. In contrast, we find no evidence of a differential impact of the reform on attitudes toward violence, labor market outcomes, partner characteristics, spousal violence, or fertility decisions for women who experienced childhood maltreatment.

One potential threat to our identification strategy is that the use of self-reported data on perpetrating child maltreatment may lead to a reporting bias if more educated women are less or more likely to report child abuse. While we cannot rule out this possibility, we find no evidence of a significant impact of increased education on mothers' attitudes toward violence, including attitudes toward violence against children. A total of 29 percent of the

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[8]Our earlier paper (Erten and Keskin 2017) uses the same reform and an older version of the same data source, the 2008 National Survey on Domestic Violence against Women (NSDVW) in Turkey, to quantify the impact of schooling on indicators of intimate partner violence. Combined, these two papers draw a rich picture of the heterogeneous effects of education on different forms of violence at different periods of a woman's life. Unfortunately, we cannot utilize the same dataset in the analysis of this paper since many of the women exposed the the education reform (younger than age 21 in 2008) were still too young to have had children of a certain age.

[9]We find no evidence that the reform had a significant impact on the level of education attained by men or by women who grew up in urban regions of Turkey.

women in our sample believe that it may be necessary to beat children for disciplinary reasons. Similarly, there is a wide acceptance of spousal violence among women, as 38 percent of them agree that men are justified in beating their partners in certain situations. More importantly, almost half of the women in our sample (48 percent) report that they have at least once physically abused their children, and an astonishing 41 percent report that they have used physical violence multiple times or frequently. Moreover, previous studies that have relied on similar national surveys to examine the relationship of child maltreatment to other outcomes investigated the validity of self-reported data on child maltreatment and concluded that these data are valid as long as they are collected properly (Allen et al. 1994; Currie and Tekin 2012).[10] Finally, as explained in detail by Currie and Tekin (2012), there are several problems with using administrative data to capture child maltreatment. Most such data have limited controls for family characteristics and other relevant individual information, and they capture only a fraction of child maltreatment behaviors since not all incidents of abuse are reported to government agencies. These agencies are also likely to have records of a selected group of families, which may constitute an unrepresentative sample (Smith and Thornberry 1995). These issues are exacerbated in developing countries, where only the most extreme cases of child physical abuse are reported to the police or lead to the victims being admitted to a hospital.[11]

1.1 Related Literature

Our work contributes to the growing literature on the causal effects of maternal education on child outcomes. One strand of this literature focuses on the improvements in child health that are induced by an exogenous increase in maternal education. Several studies confirm that an increase in the mother's education results in a reduction in child mortality (Chen and Li 2009; Chou et al. 2010; Grépin and Bharadwaj 2015). Regarding the potential channels underlying these health improvements, previous studies found that additional years of female schooling led to an increase in women's knowledge about health (Glewwe 1999; Agüero and Bharadwaj 2014) and greater use of preventive care services (Grépin and Bharadwaj 2015; Gunes 2016).

A related body of empirical work, mostly in the psychology literature, focuses on documenting the presence of the intergenerational transmission of violence against children and on examining the factors underlying this mechanism. The positive correlation between the experience of child physical abuse and the adult risk of perpetrating such abuse has been widely

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[10]In our study, one woman per household was randomly selected for the interview, and there was no one else in the room when the interview was conducted. The respondents were informed that their answers would be kept confidential, and for sensitive questions, cards with pictures were used to minimize the potential for reporting bias.

[11]Although we have no access to data on official reporting of child abuse, the corresponding figures for intimate partner violence present a bleak picture: our dataset indicates that only approximately 4 percent of women filed a police report or visited a hospital after experiencing spousal physical violence, while roughly 30 percent of women experience physical violence from their partners.

documented with different sources of data, including data from Navy recruits (Merrill et al. 1996), undergraduates (Narang and Contreras 2000), parents (Craig and Sprang 2007), and a combination of nonparents and parents (Crouch et al. 2001). Studies in the psychology literature aimed to examine the factors that explain why physically abused children, as adults, have a higher risk of abusing their own children. Bower-Russa (2005) document a positive association between having a childhood history of experiencing physical discipline and later acceptance of an attitude in favor of using severe parental physical discipline. Wekerle et al. (2001) find that individuals with traumatic childhood experiences are more likely to experience dating violence as adults. In a similar vein, Milner et al. (2010) document that psychological trauma symptoms mediate the transmission of child abuse across generations.

Our paper is also among the first studies to evaluate the role of education in improving mental health outcomes. Kubzansky et al. (1999) document a negative association between educational attainment and long-term stress. Similarly, using a longitudinal dataset, Chevalier and Feinstein (2006) provide limited evidence that an increase in mother's education improves maternal health outcomes and reduces the risk of adult depression. The experimental evidence provided by Dinarte and Egana Del-Sol (2017) is closest to our work in spirit, as it examines the impact of an after-school program on emotional regulation by adolescents that are at risk of using violence against their peers. Their findings indicate that exposure to the program significantly improves the emotional resilience of at-risk adolescents, particularly of girls, by reducing their overreaction to positive stimuli and increasing their belief that one's life can be controlled. Our study complements their findings by showing that exposure to increased education during adolescence improves mental health of at-risk mothers and thereby reduces their probability of child maltreatment.

Finally, our study also relates to the extended literature on the causal effects of compulsory schooling laws on returns to education in the labor market (Angrist and Krueger 1991; Oreopolous 2006), health outcomes (Lleras-Muney 2005; Clark and Royer 2013), fertility behavior (Black et al. 2008; McCrary and Royer 2011) and other outcomes. We contribute to this growing literature by offering the first study to examine the effects of female schooling on the risk of perpetrating child physical abuse and providing detailed evidence from a developing country, Turkey. We acknowledge that previous studies have examined the effects of the same 1997 compulsory schooling reform on other outcomes of interest in Turkey. These studies include, but are not limited to, Cesur and Mocan (2014) and Gulesci and Meyersson (2012), who find a negative effect of the reform on women's religiosity; Dincer et al. (2014) and Gunes (2016), who find a negative effect on fertility and child mortality; and Erten and Keskin (2017), who find an increase in the psychological violence and financial control experienced by women. Although our findings complement these studies, our paper differs significantly through its focus on the intergenerational transmission of child physical abuse and the channels through which education may affect this transmission.

This paper is organized as follows. Section 2 provides an overview of the 1997 compulsory schooling law in Turkey. Section 3 presents the data used in the analysis, the identification strategy used to estimate the causal effects of education on the intergenerational transmission of child abuse perpetration, and preliminary checks for the RD analysis. Section 4 presents the main results, and Section 5 provides a discussion of the evidence regarding potential causal channels. Section 6 concludes the paper.

2 Overview of the 1997 Compulsory Schooling Law in Turkey

Prior to the change in the basic education law in 1997, the education system in Turkey was composed of five years of primary school, three years of junior high school, and three years of high school. Only the first five years of primary school education were mandatory, and the rest were voluntary. In 1997, the parliament of Turkey passed Law No. 4306, which extended compulsory schooling to eight years, combining primary school and junior high school into primary education. This law was referred to as the Basic Education Program, and it applied to all students who did not already have a primary school diploma at the beginning of the 1997-1998 school year.

While the Ministry of National Education (MONE) had already targeted an increase in enrollments in junior high school as a policy goal, the timing of the Basic Education Program was motivated largely by the political events of the late 1990s. Prior to the new policy, students could choose between a secular or a religious junior high school education. The secular government, which came to power in 1997 after the military memorandum aimed at limiting the spread of political Islam, eliminated the option of a religious junior high school education. Compulsory schooling was extended from five to eight years, and it was to be provided only in secular schools. Students began to receive a diploma for successfully completing eighth grade.

The law for the school starting age in Turkey requires that a child begin compulsory schooling in September of the year when he/she turns 6 years old. The 1997 Basic Education Program, which made eight years of primary education compulsory, was effectively implemented in the 1997-1998 school year. If a student had completed fifth grade in 1997, he/she could drop out. However, if a student had completed fourth grade in 1997, he/she was required to continue school through eighth grade. The combination of the school starting age law and the 1997 Basic Education Program implied that children born before January 1987 could drop out after five years, whereas those born after January 1987 had to complete eight years of education. Despite the presence of cases that did not fit this rule, due to either imperfect compliance with the age of starting school or grade repetition, the official requirements were such that students born after January 1987 were more likely to comply with the new compulsory

schooling law than the older cohorts.[12]

The Basic Education Program required substantial investments in schooling infrastructure, which led to an increase in the share of MONE in the public investment budget from 15 percent in 1997 to 37 percent in 1998. Referred to as a 'big bang' approach to education reform, the Basic Education Program necessitated the restoration of old schools and the construction of new schools, the hiring of 103,000 additional teachers (a 41 percent increase) and the construction of 80,000 new classrooms (a 36 percent increase) between 1996 and 2003. The Turkish government also aimed to improve computer literacy by purchasing and distributing more than 56,000 computers to rural primary schools. A standardized bus system was implemented in 2000 to transport students from rural areas to nearby schools, and a program was established to distribute free books and meals to low-income students.

The Basic Education Program was successful in substantially increasing enrollment in primary education. From 1997 to 2000, the net schooling ratio rose from 84.74 percent to 93.54 percent, and the number of students increased from 9,084,635 students to 10,480,721 students. Notably, the enrollment of girls substantially increased, and from 1995 to 2005, the ratio of girls to boys in primary and secondary education rose from 90 percent to 97 percent. Due to the massive investments in schooling infrastructure, the student-to-teacher and student-to-classroom ratios remained fairly constant, implying that the quality of education did not deteriorate over this period.

3 Data and Empirical Methodology

3.1 Data

We use data from Turkey's NSDVW of 2014, a nationally representative household survey that contains information on the presence and intensity of respondents' use of violence against their children, the respondents' history of exposure to violence from their own family members during childhood, their exposure to spousal violence, their mental health indicators, and their children's behavioral indicators as well as indicators of other intrahousehold behavior. The survey, which was conducted among 15,072 households between April and July 2014, includes data on the socioeconomic indicators of households, demographics, labor market and marital histories, mental health indicators, gender role attitudes, and the indicators of violence mentioned above.

The survey targeted women between 15 and 59 years old, including those who do and those who do not have children. One woman per household was randomly selected for the interview.

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[12]Cesur and Mocan (2014) explain in detail that Turkish students who are 72 months old by the end of a calendar year can start school in September of that year (Resmi Gazete, Number 21308). As a result, children born before January 1987 could begin primary school education in 1992 and avoid the 8-year requirement that was adopted on August 18, 1997 and effectively implemented in the 1997-1998 school year.

There was no one else in the room when the interviews were conducted, and the respondents were informed that their answers would be kept confidential. The survey also includes the birth month and year of each respondent, and these data facilitate our use of an RD approach. It also contains information on the type of region in which each woman lived through the age of 12 (e.g., a village, a district, or a province). This information allows us to construct an indicator of pre-reform rural residence, as the age for starting junior high school in Turkey is 12 years old.

The indicators of violence against children include whether the respondent has ever physically abused her children and, if affirmative, how often she has physically abused them, e.g., once, twice, a few times, or many times. Using this information, we construct two indicators of violence against children: (i) an indicator variable of whether the respondent has ever physically abused her children and (ii) an indicator variable of whether the respondent has frequently abused her children, including a few times and many times. The summary statistics presented in Panel B of Table 1 show that 48 percent of women in Turkey have at least once used physical violence against their children. The propensity to frequently abuse children is also rather high, as approximately 41 percent of women have physically abused their children often. A larger proportion of the respondents who grew up in rural regions physically abuse their children in comparison to those who grew up in urban regions, with a difference of 6 percentage points (ppt).

Table 1 also reports summary statistics for other major indicators of women who have children from the 2014 NSDVW survey. We provide summary statistics for women between the ages of 20 and 34 since the estimated bandwidths in our local regression analyses fall into this range. Panel A indicates that the average period of female schooling for this age group was 7.5 years. The junior high school completion rate was 51 percent, the high school completion rate was 31 percent, and 89 percent of the women had completed primary school. Column 4 reports differences between the group means of women raised in rural areas and those of women raised in urban areas. Women raised in rural areas had 1.8 fewer years of schooling, 21 ppt lower rates of junior high school completion, 20 ppt lower rates of high school completion, and 5 ppt lower rates of primary school completion. These results correspond to 21 percent fewer years of schooling, 34 percent lower rates of junior high school completion, and 61 percent lower rates of high school completion than the sample mean.

Panel C of Table 1 presents descriptive statistics of the respondents' attitudes toward violence. Roughly 38 percent of women agree with the statement that men can beat their partners in certain situations, and 29 percent agree with the statement that it may be necessary to beat children for discipline. Hence, about one-third of the women believe that both spousal violence and violence against children can be justified or even necessary under certain conditions. While a greater proportion of the women raised in rural regions tend to approve spousal violence (a 9 ppt difference), there is no evidence of a significant difference between regions regarding the

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attitudes toward violence against children.

Panel D reports summary statistics for mental health outcomes. Following Duflo et al. (2007) and Kling et al. (2007), we aggregate information from 20 indicators of a mother's mental health to construct three summary indices: (i) an overall depression index, which is an average of the z-scores of all 20 indicators; (ii) a somatic depression index, which is an average of 4 indicators that are related to the body and are therefore more objective measures of depression; and (iii) a nonsomatic depression index, which is an average of the remaining 16 indicators that are more related to the mind and thus represent more subjective assessments of depression.[13] This aggregation approach provides greater statistical power to identify effects in the same direction for a group of indicators that captures similar symptoms of psychological well-being. Higher index values indicate higher levels of depression. The raw means indicate that more overall depression, somatic depression, and nonsomatic depression is experienced by women raised in urban areas, but the difference between the rural and urban samples is significant only for the overall depression index.

Panel E provides summary statistics for fertility-related outcomes. On average, the age of the first pregnancy of the respondents is 21, and the average number of children is 1.5. Women raised in rural regions are slightly younger during their first pregnancy (a difference of 0.5 years) and have a higher fertility rate (a difference of 0.4) than those raised in urban regions.

Panel F presents descriptive statistics for labor market outcomes. Only 19 percent of the 20- to 37-year-old women in our sample were employed, and 14 percent of them were employed in the service sector. These results are consistent with the overall pattern in Turkey, where female labor force participation remains rather low.[14] Approximately 11 percent of the respondents worked in a job that had social security benefits. We also construct a personal income index

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[13]Our survey includes 20 questions on several features of an individual's mental health. These questions are designed to identify depression in non-clinical settings and focuses on the classic psychological symptoms of depression, such as poor concentration or thoughts of suicide, and less-known somatic symptoms such as frequent aches and pains or digestion problems (Chevalier and Feinstein (2006)). These indicators have been proven to be good predictors of depression when analyzed together. We created two separate indices to capture the extent of a woman's mental well-being using these questions. The somatic depression index is a z-score calculated by averaging the z-scores from each of the 4 somatic depression indicators, represented by dummy variables that take a value of 1 if the respondent reports that she experienced the following within the previous four weeks: (i) frequent headaches, (ii) trembling hands, (iii) digestion problems, and (iv) heartburn or other stomach problems. The nonsomatic depression index is a z-score calculated by averaging the z-scores from each of the 16 nonsomatic depression indicators, represented by dummy variables that take a value of 1 if the respondent reports that she experienced the following within the previous four weeks: (i) appetite loss, (ii) trouble sleeping, (iii) easily triggered feelings of fright, (iv) anxious or nervous feelings, (v) difficulty thinking clearly, (vi) unhappiness, (vii) frequent crying, (viii) loss of enjoyment in daily activities, (ix) difficulty making decisions, (x) delayed daily activities, (xi) feelings of uselessness, (xii) a loss of interest in activities that she previously enjoyed, (xiii) feelings of worthlessness, (xiv) thoughts about suicide, (xv) constant feelings of tiredness, and (xvi) tiring easily. The overall depression index is a z-score calculated by averaging the z-scores from 20 depression indicators, specifically the 4 somatic and 16 nonsomatic depression indicators listed above.

[14]In our entire survey dataset, the female labor force participation is 22 percent, and the female labor force participation in the service sector is 14 percent.

by averaging the z-scores of indicator variables capturing whether the respondent earned a personal income from various sources.[15] Higher index values indicate greater personal income. The last row in Panel E reports summary statistics for an index of asset ownership, which is constructed by averaging the z-scores of indicator variables on whether the respondent's household owns 25 different assets.[16] Higher index values indicate greater household wealth. On average, compared with women raised in urban areas, women raised in rural areas were 7 ppt less likely to work in the service sector and 5 ppt less likely to have access to social security benefits. They also had a relatively lower personal income and asset ownership.

Panel G provides summary statistics for the partner characteristics and marriage market indicators. On average, the respondents' partners had completed 8.8 years of schooling and were 25 years old. We construct a proxy measure of the partner's religiosity by averaging the z-scores of indicator variables on behaviors prohibited by Islam.[17] The average age of the respondent upon the first marriage was 21 years, and 57 percent had chosen to marry their husbands, whereas the other women had undergone arranged marriages. Six percent had ever been divorced. On average, the partners of the women raised in rural areas have approximately 0.9 fewer years of schooling, are 0.4 years younger, and have more religious attitudes. The average age of marriage for women raised in rural areas is 0.4 years younger that than for women raised in urban areas. Women raised in rural areas are 12 ppt less likely to have chosen to marry their husbands. There is no evidence of a significant difference in divorce rates for women raised in different areas.

Panel H presents descriptive statistics for the spousal violence measures. We aggregate information from different sets of spousal violence measures to create three summary indices: a physical violence index, a psychological violence index, and a financial control index. We construct these indices by averaging the z-scores of each underlying measure of physical vi-

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[15]The sources of personal income include rent from owning land, rent from owning a house, income from owning a company or workplace, income from owning a vehicle, having money in a bank, and income from other asset ownership. We construct a dummy variable for each indicator of personal income that takes the value of 1 if the respondent earns income and 0 otherwise. We use the simple average of the z-scores of these six dummy variables to construct a personal income index for the respondent.

[16]The asset categories include refrigerator, deep freezer, gas/electric oven, microwave oven, dishwasher, garbage disposal, washing machine, clothes dryer, iron, vacuum cleaner, plasma TV (LCD), home theater, television, satellite TV, paid TV service, DVD/VCD player, cell phone, nonmobile telephone, laptop/tablet computer, desktop computer, internet, air conditioner, car, taxi/minibus/bus or other commercial vehicles, and tractor. We construct a dummy variable for each indicator of household wealth that takes the value of 1 if the respondent's household owns an asset and 0 otherwise. We use the simple average of the z-scores of these 25 dummy variables to construct an asset ownership index for the respondent's household.

[17]The index is a z-score calculated as an average of the z-scores of the partners' characteristics, including a dummy variable that takes the value of 1 if the partner never drinks alcoholic beverages, a dummy variable that takes the value of 1 if the partner never gambles, a dummy variable that takes the value of 1 if the partner never uses narcotic drugs, and a dummy variable that takes the value of 1 of the partner never had an affair. Since Islam prohibits these behaviors by categorizing them as sins, individuals with strong religious beliefs are very unlikely to exhibit them.

olence, psychological violence, and financial control behavior.[18] Higher index values indicate higher levels of spousal violence. The differences between the rural and urban samples are not statistically significant.

Finally, Panel I of Table 1 reports summary statistics of the predetermined characteristics of the 20- to 37-year-old women in our sample who have children. Fifty-nine percent of the women lived in a rural area until the age of 12, and 18 percent lived in villages. Approximately 1 percent had a non-Turkish primary interview language, typically Kurdish or Arabic. On average, 14 percent of the respondents had experienced violence from a family member during their childhood.[19]

3.2 Identification

The 1997 compulsory schooling law together with the law on school starting age required the completion of 8 years of schooling by individuals born after January 1987, whereas those born earlier could drop out after 5 years, as explained in more detail earlier in Section 2. We use this discontinuity in an RD design to estimate the causal effect of schooling on violence against children. Our identifying assumption is that these two cohorts born one month apart do not exhibit any systematic differences other than whether they were exposed to the compulsory schooling law. As long as this assumption holds, this approach represents a treatment assignment that is as good as random. In our RD design, we assign treatment based on the month and year of birth of the individual, with those born after January 1987 assigned to the treated status.

Following previous research (Oreopolous 2006; Clark and Royer 2013), we employ an RD design by using discontinuity in the birth date and using this discontinuity as an instrument for years of schooling. We provide both reduced-form (RF) estimates (i.e., sharp RD) and two-stage least-squares estimates (i.e., fuzzy RD) for all of the outcome variables of interest.

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[18]The physical violence index is a z-score calculated by averaging the z-scores from each of the 6 physical violence indicators, including dummy variables that take the value of 1 if the respondent reports that she experienced intimate partner violence acts in the form of (i) slapping or throwing an object that would hurt; (ii) pushing, shoving, or pulling hair; (iii) hitting with his fist or in a way that hurts; (iv) kicking, pushing to the ground, or beating; and (v) choking or burning. The psychological violence index is a z-score calculated by averaging the z-scores from each of the following indicators, including dummy variables that take the value of 1 if the respondent reports that she experienced intimate partner violence acts of (i) insulting, (ii) humiliating, (iii) scaring or threatening, (iv) attempting to isolate her from her friends, (v) attempting to prevent contact with her family, (vi) insisting on knowing her location, (vii) ignoring her, (viii) becoming angry if she speaks to other men, (ix) suspecting that she is cheating on him, (x) wanting her to seek his permission before obtaining healthcare, and (xi) intervening in her clothing choices. The financial control index is a z-score constructed by averaging the z-scores from two of the financial control behaviors, including dummy variables that take the value of 1 if the respondent reports that she experienced the following behaviors from her intimate partner: (i) taking income from her despite her disapproval and (ii) refusing to give her money for household spending.

[19]Due to the potential recall problem, the questions in the survey were designed to ask only about violence from parents or other family members after the age of 15. This approach is likely to generate a more conservative estimate of the overall violence faced by an individual as a child.

Our specification follows a basic RD form:

$$y_i = \alpha + \beta t_i + f(x_i) + \epsilon_i \tag{1}$$

$$\forall x_i \in (c - h, c + h)$$

where y_i is the dependent variable, t_i is the treatment status, x_i is the forcing variable, and h is the bandwidth around the cutoff point c. We allow the slope to vary on each side of the cutoff. The control function, $f(x_i)$, is a continuous n-order polynomial function of the forcing variable on each side of the cutoff point. We use local linear regressions in our RD estimations (Imbens and Lemiuex 2008) and conduct optimal bandwidth selection using the Imbens and Kalyanaraman (2009) procedure. This approach implies the selection of an optimal bandwidth for each outcome variable examined.[20] Following Lee and Card (2008), we cluster standard errors at the month-year of birth level to accommodate for specification error in the forcing variable. Since we evaluate the effects of education on a large number of outcomes, we adjust standard errors for multiple-hypothesis testing following Simes (1986). Thus, for each outcome variable, we report results based on both standard p-values and p-values adjusted for multiple-hypothesis testing.

To examine whether the reform had a differential impact on women who were exposed to violence from family members during their own childhood, we estimate the following specification:

$$y_i = \alpha + \beta t_i + \gamma t_i \times v_i + \delta v_i + f(x_i) + u_i \tag{2}$$

$$\forall x_i \in (c - h, c + h)$$

where y_i is the dependent variable, t_i is the treatment status, v_i is exposure to violence from family members during childhood, x_i is the forcing variable, and h is the bandwidth around the cutoff point c.[21] The main coefficients of interest are δ, which captures whether exposure to childhood violence affects the individual's adult behavior toward her own children or other individual outcomes of interest, and γ, which shows whether the education reform had a differential impact on individuals exposed to childhood violence. In other words, the former coefficient captures the intergenerational transmission of violence, and the latter indicates whether receiving more education has an impact on the transmission of violence between generations.

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[20]In addition, we use specifications that adopt the optimal bandwidth from the first-stage results for years of schooling in rural regions of childhood, which is estimated as 85 months around the discontinuity; these are included in the appendix tables. This static bandwidth approach complements the former results for which we use the optimal bandwidth.

[21]We again let the slope vary on each side of the cutoff and use local linear regressions within an optimal bandwidth selected by the Imbens and Kalyanaraman (2009) procedure. We cluster standard errors at the month-year of birth level and adjust them for multiple-hypothesis testing (Simes 1986).

Finally, we include the following control variables in all of our specifications: a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, childhood-region fixed effects, and interactions of childhood-region fixed effects with an indicator of rural childhood regions.[22]

3.3   Preliminary Checks

We provide two standard validity checks for the RD design (Imbens and Lemiuex 2008). First, we investigate whether the density of the forcing variable, the month-year of birth, is continuous at the discontinuity. We perform a McCrary density test on the density of the forcing variable. This test yields an insignificant estimate, as shown in Figure 1.

Second, we examine whether the predetermined covariates are balanced around the discontinuity. In Figure 2, each graph represents local averages of the outcome in one-month bins plotted against the forcing variable, with overlaid smoothed linear regression lines using raw data on each side of the cutoff. The gray lines represent 95 percent confidence intervals. The predetermined characteristics that we plot are regional dummy variables capturing whether the respondent's childhood region is western, southern, central, northern, or eastern Turkey and whether the respondent's interview language is not Turkish. The graphs do not indicate any significant jumps at the cutoff point. Overall, we conclude that the predetermined covariates appear to be balanced around the threshold.

Because all of the violence against children-related questions are relevant only to women who have children, our RD analysis is based primarily on the sample of women who have children. One concern is the extent to which the treatment had an effect on having children or on the number of children women had and therefore on selection into the main sample of the analysis. To address this concern, we test whether the reform had a significant effect on having children and on the number of children that women had. Table A2 shows no evidence of a significant effect of the reform on having children or on number of children. Hence, there is no reason to expect that the reform affected the probability of selection into the sample of women who have children, and this sample will therefore be our focus of analysis throughout the remainder of the paper.

Since our main focus in the paper is to examine the differential effects of the reform on women who experienced violence from their own family members, we also test whether the reform had any effect on the probability of facing childhood violence. This might be a particular concern, if, for instance, additional hours or days spent in school by the individuals exposed to the compulsory schooling law make them spend less time with their parents at home and mechanically decrease their chances of being physically abused. The RD estimates in the first

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[22]We use fixed effects for 12 regions where the respondents lived until the age of 12, when they were subjected to the education reform.

row of Table A2 indicate no evidence of a significant impact on this predetermined outcome, as expected. Moreover, we also find no evidence of a significant effect on the intensity of childhood violence, which is an indicator variable of whether the respondent faced childhood violence often. Finally, we test whether the reform had any impact on the probability of experiencing violence from family members or others (e.g., teachers, strangers, etc.), which we refer to as overall childhood violence. We find no evidence of a significant effect on overall childhood violence or on its intensity.

4   Effects of the Compulsory Schooling Law

4.1   Schooling Outcomes

We begin by testing the effect of the compulsory schooling reform on educational outcomes. Since the 2014 NSDVW data for Turkey do not have month-of-birth information for men, we show the RD treatment effects of the reform on the junior high school completion of men and women using the 2014 Household Labor Force Survey (HLFS) data. Figure 3 plots the local averages of female and male rates of junior high school completion in month-of-birth bins around the cutoff point, January 1987. The graph on the left shows evidence of a clear jump in the junior high school completion of women, whereas the right-side graph shows no evidence of a significant jump in the same outcome for men. This result implies that the reform had a much smaller effect on men, possibly because the junior high school completion rate for males was already close to 90 percent prior to the reform.

Focusing on the sample of women, Figure 4 provides a graphical illustration of the RD design by comparing the treatment and placebo effects using the 2014 and 2008 NSDVW surveys. The left-side graph plots the average junior high school completion rates in monthly bins against the month and year of birth, with a cutoff of January 1987 using the 2014 NSDVW survey. As described in Section 2, the education reform required those born after this date to complete junior high school, whereas the older cohorts had the option of dropping out after completing primary school. Local linear smoothers on each side of the cutoff are overlaid on the graph, which shows a clear jump at the discontinuity with an approximately 15-20 ppt increase in the probability of completing junior high school. We use data from the 2008 NSDVW to conduct a placebo test to examine the validity of the RD design. The right-side graph of Figure 4 shows the same relationship using the 2008 HLFS survey, in which the age cutoff is the same, comparing 27- and 28-year-old women. The age cutoff corresponds to being born before or after January 1981. The right graph shows no evidence of a jump in completing junior high school for women of the same age in the 2008 NSDVW data. Thus, the jump that we observe around the discontinuity of the reform implementation in the 2014 survey is not likely to be driven by some underlying relationship between age and school completion but is rather an

outcome of the reform.

While these graphs reveal a positive RD treatment effect of being exposed to the compulsory schooling reform, the results could be further refined with regression analysis. Table 2 reports the RD treatment effects on years of schooling and the completion of different levels of education for all women surveyed in the 2014 NSDVW. In each row, the last column reports outcome means for the relevant sample. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, childhood-region fixed effects, and interactions of childhood-region fixed effects with an indicator of rural childhood regions. Column 4 displays the optimal bandwidth estimated by the Imbens and Kalyanaraman algorithm in months on each side of the cutoff.

The first row of Table 2 presents estimates of the RD treatment effects on the years of schooling obtained by all women. The optimal bandwidth, calculated using the Imbens and Kalyanaraman (2009) algorithm, yields a bandwidth of 89 months around the discontinuity. Based on a local linear specification, column 1 presents an RD estimate of 0.70 years for the treatment effect on years of schooling, which is statistically significant at the 5 percent level. In terms of magnitude, an increase of 0.70 years in the years of schooling corresponds to an 8.3 percent increase relative to the mean. For robustness, we include alternative specifications by allowing the bandwidth to vary and report the linear RD estimates with 0.75 and 1.5 times the optimal bandwidth in columns 2 and 3, respectively. The estimated effects remain significant within the approximate range of 0.7 to 1 year. The remaining rows of Table 2 present the RD treatment effects on different levels of school completion. The second row displays the estimated RD treatment effects for the outcome variable that captures whether the respondent completed junior high school or higher. Column 1, based on the local linear specification, reports an RD estimate of 19 ppt, corresponding to 32 percent relative to the mean. In alternative specifications, the estimate remains significant. The third row shows that the linear RD estimate of the treatment effect on completing high school is 13 ppt, and it remains significant in alternative specifications. This finding implies that the reform had long-term effects in enabling some women to continue beyond junior high school. As expected, all RD estimates for whether the respondent completed primary school are insignificant. These results for primary school completion constitute a robustness check showing that the reform did not influence the likelihood of completing primary school, which was already compulsory prior to 1997.[23]

In Table 3, we examine whether the reform had heterogeneous effects based on region of childhood. Because the reform affected children who were 12 years old when the reform was implemented, we expect the reform to have heterogeneous effects as a result of regional

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[23]Table A4 in Online Appendix B reports the local RD estimates using a quadratic control function with an optimal bandwidth selection method in column 1. The results are in line with those reported in Table 2.

disparities in constraints on access to female education in Turkey. Whereas some of these constraints result from insufficient schooling infrastructure in rural areas, some are related to the more conservative attitudes toward sending girls to school that are prevalent in rural areas (Dulger 2004). The linear RD estimate in the first row of Panel A and column 1 shows that the reform had a positive effect of 1.1 years on the schooling of women raised in rural areas. This effect corresponds to a 15 percent increase relative to the mean. The RD estimates in the alternative specifications in columns 2 and 3 remain highly significant, ranging from 1.1 to 2.3 years. In contrast, the linear RD estimate in column 4 of Panel A reveals no significant impact of the reform on years of schooling for women who spent their childhood in urban regions. The RD estimates in the other columns remain insignificant except for the linear RD estimate with one and a half times the optimal bandwidth, which is likely the result of an artificially large bandwidth that covers observations with much lower values from the left side of the discontinuity.

Panel B of Table 3 focuses on the RD treatment effects on women who have children, who constitute our sample of interest in testing violence against children in the subsequent step. In the subsample of women raised in rural areas, the linear RD treatment effect is 1.1 years of schooling, which corresponds to a 16 percent increase relative to the mean. In alternative specifications, the RD estimates for the sample of women who have children and grew up in rural areas remain highly significant and close to the magnitude of RD estimates for the entire sample (slightly larger in terms of percentage change relative to the mean). A comparison of the means of the two samples shows that women who have children had lower schooling outcomes relative to the full sample before the reform, and they were more likely to comply when the reform was implemented. Like the full-sample RD effects, columns 4-6 of Panel B in Table 3 indicate no evidence of a significant effect of the reform on the years of schooling completed by women with children who were raised in urban regions. In short, the compulsory schooling law had a positive effect on the years of schooling of approximately 0.7 years for all women and slightly more than one year (approximately 1.1 years) for women raised in rural regions and women with children who were raised in rural regions. The estimates are robust to using alternative functional forms and bandwidths. This implies that the fuzzy RD estimates in the two-stage least-squares specification will be slightly smaller than the sharp RD estimates, as we use the sample of women who have children. In our results, however, we report both of these estimates for comparison.

As a robustness check, Tables A3 and A5 and column 2 of Table A4 in Online Appendix B report the RD estimates using a static bandwidth of 85 months around the cutoff, which is the optimal bandwidth estimated for the years of schooling in rural regions during childhood.[24] The

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[24]This bandwidth also corresponds to the optimal bandwidth estimated for years of schooling for the sample of women who have children. Since the rest of the analysis will focus on the sample of women with children, and particularly those who grew up in rural regions, we use this bandwidth in all tables with static bandwidth results. The results do not change qualitatively if we use the optimal bandwidth

findings in these tables are very similar to those in Tables 2 and 3. Whereas the RD treatment effects on the years of schooling of women raised in rural regions are statistically significant and large, those on the years of schooling of women from urban regions are insignificant and much smaller. Since we find no evidence of a significant impact of the reform on women raised in urban areas, in the following sections, we will report the results for the overall and rural samples.[25]

4.2   Education and Violence against Children

In this section, we test whether the reform had a significant impact on violence against children. Table 4 presents the results. In Panel A, the OLS estimates in columns 1 and 4 indicate the presence of a negative correlation between years of schooling and child abuse and years of schooling and frequent child abuse. The magnitudes of the correlations suggest that one additional year of schooling corresponds to a 2.1 (2.4) ppt lower probability of physically abusing children in the overall (rural) sample and a 2.0 (2.1) ppt lower probability of frequently physically abusing children in the overall (rural) sample. Remarkably, the RD estimates in the first two rows of Panel A show no evidence of a significant effect of the reform on ever abusing children or frequently abusing them in the overall or rural samples.

In Panel B of Table 4, we examine whether the reform had a differential impact on women who were exposed to violence when they were children themselves. The coefficient estimates for being exposed to childhood violence are significant and positive, indicating that women with exposure to childhood violence are more likely to exert physical violence against their own children. This finding implies that there is a strong intergenerational transmission of violence against children. Next, we examine whether the reform had a differential impact on these women. In other words, could the reform break the intergenerational cycle of violence against children? The RD estimates in columns 5 and 6 show that the reform had a negative impact on physical child abuse by mothers who were exposed to childhood violence and were raised in rural regions. Hence, in rural regions, where the reform had the largest impact, the RD estimates show that the reform led to a significant decline in the probability of ever physically abusing children and of frequently abusing them for mothers with exposure to childhood violence. Thus, the reform provided a strong impetus to break the cycle of intergenerational violence against children.

The magnitude of the RF estimates in column 5 of Panel B in Table 4 indicate that women raised in rural areas are 34 ppt more likely to abuse their children if they experienced physical maltreatment in childhood themselves. Being exposed to the reform reduces this probability by 22 ppt. These are sizable effects, given the outcome mean of 51 percent. The sum of the two coefficients is not statistically different from zero. The IV estimates in column 6

estimated for the years of schooling for the full sample, which is 89 months.

[25]The results for the urban sample are available from the authors upon request.

are consistent with the RF estimates; however, it should be noted that the sum of the two coefficients is different from zero, indicating that while the reform reduced the probability of abusing children, it did not completely eliminate it. However, such intergenerational effects may snowball over time and lead to a larger reduction across generations if exposure to more education reduces the transmission from one generation to the next. The magnitudes of the estimates are slightly larger for the outcome of frequent child abuse. This implies that the reform has reduced not only the probability of violence against children but also its intensity.

As a robustness check, Table A6 in Online Appendix B reports the RD estimates using a static bandwidth of 85 months around the cutoff, which is the optimal bandwidth estimated for years of schooling of women who grew up in rural regions. The findings are quite similar to those shown in Table 4. Panel A shows that there is no evidence of a significant effect of the reform on ever abusing children or frequently abusing them. Panel B indicates that in the subsample of rural childhood regions, the reform had a negative impact on the probability of child abuse and frequent child abuse for women who experienced childhood physical abuse, meaning that it reduced the intergenerational transmission of violence. The RD estimates are precisely estimated for both RF and IV specifications, and the magnitudes of the effects are very similar.

As an additional robustness check, Table A7 in Online Appendix B reports the RD treatment effects of the reform by exposure to alternative forms of violence during childhood. In Panel A, we examine whether the reform had differential effects on women exposed to overall childhood violence, i.e., violence from family members or others, including teachers and strangers. The RD estimates in columns 5 and 6 indicate that the reform had a significant negative impact on child abuse or frequent child abuse for women who experienced overall childhood violence. The magnitudes of the estimates are similar to, but slightly smaller than, those shown in Table 4.

In Panel B of Table A7, we examine whether the reform resulted in differential effects on women who witnessed domestic violence against their own mother while growing up in a violent home. The RD estimates show no evidence of a significant impact on violence against children by women who witnessed violence, but were not necessarily physically abused themselves, at home as a young child. This indicates that the reform is effective only for the subset of women who themselves experienced violence during childhood as opposed to women who witnessed violence against their mother.

In Table A1 in Online Appendix B, we investigate whether the reform had a differential impact on the schooling outcomes of women who were exposed to childhood maltreatment. We find no evidence of a significant differential impact of the reform on years of schooling or on the completion of junior high school of women who experienced childhood violence in the overall or rural samples. However, the reform had a significant impact on the schooling outcomes of women regardless of their history of childhood maltreatment. Thus, the differential impact

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we found in child maltreatment by women who were physically abused themselves does not necessarily arise from the fact that on the margin this reform made them more likely to attend school compared to their non-abused peers. In contrast, it is possible that similar amounts of increase in the years of schooling had a differential impact on these more vulnerable women for the reasons we will explore in Section 5.

Finally, we check the robustness of our results by using an alternative optimal bandwidth selection method proposed by Calonico et al. (2014). Table A14 in Online Appendix B reports in columns 1-3 the results for the rural sample using the Calonico et al. (2014) (CCT) optimal bandwidth selection method and compares them with our original results using Imbens and Kalyanaraman (2009) (IK) optimal bandwidth selection in columns 4-6. The coefficient estimates using the CCT bandwidth selectors reported in columns 2 and 3 are similar in magnitude to those reported in columns 5 and 6 using the IK bandwidths, although some are less precisely estimated due to the smaller number of observations included in the narrower CCT bandwidths.[26]

Altogether, our results indicate that the reform reduced the intergenerational transmission of violence against children. While we find no evidence of a significant impact of the reform on child physical abuse for the whole sample, we find that the reform had a significant negative impact on child physical maltreatment by mothers who were exposed to childhood violence. In the next section, we will examine whether there is any evidence of a potential channel that could explain how the reform may have reduced the intergenerational transmission of violence against children for the main compliers with the reform (i.e., women raised in rural areas).

5 Examining Causal Channels

In this section, we proceed with an examination of the potential channels underlying our finding of diminished violence against children by mothers who experienced childhood violence from their own family members, who were affected by the 1997 education reform, and who were raised in rural regions of Turkey. We divide our analysis into six subsections by focusing on the effects of the compulsory schooling reform on the following characteristics: (i) attitudes toward violence, (ii) mental health, (iii) fertility outcomes, (iii) labor market outcomes, (iv) partner characteristics and marriage market outcomes, and (v) spousal violence.

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[26]Following Card et al. (2015), we omit the regularization term in the bandwidth selectors, since regularized selectors provide bandwidths that are too small for our empirical setting. According to Card et al. (2015), omitting the regularization term does not affect the asymptotic properties of the bandwidth selector.

5.1 Changes in Attitudes toward Violence

One of the potential mechanisms underlying the effects that we observe is the reform-induced changes in the violence-related attitudes of the mothers who experienced childhood violence. If additional years of schooling change the beliefs of these women regarding violence against women or children, the change might make them less prone to use physical violence against their children when disciplining them. The empirical evidence on the effects of compulsory schooling on violence-related attitudes is mixed. Some studies find that increased female schooling improves young women's attitudes toward domestic violence (Friedman et al. 2011), whereas others fail to find any evidence of a significant change in violence-related attitudes (Dincer et al. 2014; Erten and Keskin 2017; Gulesci and Meyersson 2012). However, none of these studies examined whether education has a differential effect on the violence-related attitudes of mothers who experienced physical violence during childhood. If socialization in the school environment exposes women to a different set of attitudes through interactions with teachers and peers as alternative role models, it may have a particularly strong effect on women who experienced childhood violence as they emulate and acquire norms that disapprove of violence toward children. Given that corporal punishment in schools is legally prohibited and constitutes unacceptable behavior in Turkey, exposure to increased education may differentially improve the attitudes of women with a history of childhood violence.

We explore this mechanism by testing whether the reform had a differential effect on the attitudes of mothers who experienced childhood abuse from their own family. Table 5 reports our findings, focusing on the probability that the respondent agrees with the following statements: (i) men can beat their partners in certain situations, and (ii) it may be necessary to beat children for discipline. The correlations reported in columns 1 and 4 of Table 5 show that the years of mother's schooling is negatively correlated with the probability of agreeing with these statements indicating approval of the use of intrahousehold violence. For the sample of the main compliers in rural regions, column 4 indicates that one additional year of schooling corresponds to a 2.3 and 2.4 ppt decline in approval of the use of domestic violence against women and children, respectively.

The RD estimates for the treatment effects on violence-related attitudes are presented in columns 2-3 and 5-6 of Table 5. We find no evidence that the reform had a differential impact on the attitudes of mothers who experienced childhood violence. All of the RD treatment effects on the interaction terms of being affected by the education reform and exposed to childhood violence are insignificant. For the mothers raised in rural regions (i.e., the main compliers with the reform), the RD treatment effects on attitudes toward violence against children–the statement that it may be necessary to beat children for discipline–are zero and insignificant.

As a robustness check, Table A8 in Online Appendix B reports the RD estimates of the effects of the reform on violence-related attitudes using an optimal bandwidth in Panel A and the differential effects of the reform on these attitudes for women with experience of childhood

maltreatment using a static bandwidth of 85 months around the cutoff. The RD estimates in Panel A indicate that the reform had no overall effect on violence-related attitudes, confirming the findings of other studies (Dincer et al. 2014; Erten and Keskin 2017; Gulesci and Meyersson 2012). In Panel B, the RD estimates using the static bandwidth show that the results in Table 5 are robust to using alternative bandwidths in the estimation. We find no evidence of a differential impact of the reform on the attitudes of mothers with experience of childhood violence. Overall, we conclude that the attitude channel does not seem to explain our main results.

5.2 Changes in Mental Health Outcomes

As an alternative potential channel, we examine whether the reform affected mental health outcomes. Additional years of schooling may allow women to learn how to cope with emotional dysfunction and directly change their mental reactions to upsetting events, which may in turn reduce their probability of experiencing depression or anxiety. Hence, if increased educational attainment enables the mother to become less depressed, anxious, and aggressive, she will be less likely to perpetrate physical maltreatment against her children. This argument is consistent with the evidence in the recent literature. Using a cluster-randomized control trial that provided cognitive behavioral therapy to women with depression during pregnancy, Baranov et al. (2016) find that treated mothers displayed better parenting behaviors, providing a better home environment and investing more in their children's education.

It is important to highlight that the mental health channel is likely to play a crucial role in reducing child physical abuse, especially by mothers with a history of childhood maltreatment. First, if exposure to physical maltreatment in childhood causes a woman to suffer from trauma, this is likely to compromise her later-life ability to regulate her emotions and to render her more impulsive and violent toward her children (Neller et al. 2005; Pomeroy et al. 1995). Additional years of schooling may improve the mental health of such traumatized individuals by teaching them to better control their emotions, which may in turn reduce the likelihood of maltreatment perpetration. Second, a history of childhood maltreatment may compromise a woman's attachment to her own parents and alter her reading of social cues such that she perceives certain cues as threatening even in situations where they are benign (Crittenden and Ainsworth 1989). If additional years of schooling allow women to better encode social cues and become less hypervigilant in their reactions to their children, such improvements in maternal mental health may reduce the risk of perpetrating maltreatment.

We examine this mechanism by testing whether the reform had a differential effect on the mental health outcomes of women with a history of childhood maltreatment. The OLS estimates in columns 1 and 4 of Table 6, which reports the results, indicate that female schooling is negatively correlated with depression measures, including the overall, somatic, and nonsomatic depression indices. The RD estimates in columns 2-3 and 5-6 show no evidence of a significant

effect of the reform on the mental health outcomes for the full sample of women. However, the interaction terms in columns 5-6 indicate that the reform led to a significant reduction in the depression indicators of women who experienced childhood violence and were raised in rural regions. Both the RF estimates in column 5 and the IV estimates in column 6 are significant and negative for all measures of depression. It is reassuring to find that the effect is significant for the more objective measure of depression, the somatic index, which includes only physical symptoms of depression. The magnitudes of the RD treatment effects are large. For the RF estimates, the sum of the coefficient for childhood violence—which is significant and positive— and the coefficient for the interaction term is not statistically different from zero, while the sum of the IV estimates of these coefficients is not statistically different from zero. These results imply that additional years of female schooling significantly reduce the probability of experiencing depression for the group of women who experienced childhood violence and have a high risk of experiencing mental illness.

As a robustness check, Table A9 in Online Appendix B reports the RD treatment effects of the reform without interaction terms in Panel A and the RD treatment effects of the reform by exposure to childhood violence using a static bandwidth in Panel B. The results are consistent with those shown in Table 6. Panel A shows no evidence of a significant RD treatment effect of the reform on the mental health of women. Panel B indicates that the reform had a differential impact on improving the mental health of women who were raised in rural areas and exposed to childhood abuse.

Altogether, these results together with those in Table 6 indicate that the mental health channel can explain our main results. In particular, our findings indicate that the reform-induced increase in female schooling led to a significant improvement in the mental health of women with a history of childhood violence, which in turn substantially reduced the probability that these women would use physical violence against their children.

5.3 Changes in Fertility Outcomes

Another potential channel through which maternal education may affect child physical mal-treatment is that additional years of female schooling may result in a decline in fertility by increasing the time spent in school and raising the opportunity costs of having children. Since parents with more children have relatively less time to reason with each child, they have a greater tendency to use physical violence as a quick method of disciplining children. If additional years of female schooling lead to a decline in the number of children that women have, then additional schooling is likely to improve mothers' parenting behavior by increasing the time available per child and reducing stress through a lesser childcare burden. Similarly, if additional years of schooling increase the age of women at their first pregnancy either through incarceration or human capital effects, this increase could lead to a change in how women experience motherhood and may reduce violence against children.

An extensive literature has examined the effects of education on fertility outcomes. Some studies found evidence that increased female schooling reduces the number of children women have in their teenage years and increases the age of first pregnancy (Black et al. 2008; Silles 2011; DeCicca and Krashinsky 2015), whereas others found no significant impact of schooling on the probability of having children or the age of first pregnancy (McCrary and Royer 2011) or found evidence of a decline in the number of very early births (up to age 15) with no evidence of a decline in fertility for later ages (Breierova and Duflo 2004). However, none of these studies analyzed whether education differentially affects the fertility decisions of mothers who experienced childhood physical maltreatment. If women with a history of childhood violence face a high risk of teenage pregnancy or of having a large number of children, one could expect that exposure to additional years of schooling may have particularly strong effects on this subpopulation.

We examine this channel by testing whether the reform had a significant impact on the fertility outcomes of women with experience of childhood violence. The correlations shown in columns 1 and 4 of Table 7, which reports the results, indicate that more educated women have higher ages of first pregnancy and a lower number of children. For example, the results shown in column 4 indicate that one additional year of schooling corresponds to a higher age of first pregnancy, an additional 0.36 years, and a 0.15 decline in the number of children.

The RD treatment effects on fertility outcomes are reported in columns 2-3 and 5-6 of Table 7. None of the RD estimates for the interaction terms of exposure to the reform and childhood violence are significant except the one for the overall sample in the RF specification. For the sample of the main compliers in rural regions, we find no significant impact of the reform for women exposed to childhood violence on age at first pregnancy or number of children. The RD estimates shown in Panel A of Table A10 in Online Appendix B indicate that the reform led to a significant increase in the age at first pregnancy for the main compliers in rural regions. In particular, the IV estimate in column 6 shows that an additional year of schooling increased the age of first pregnancy by 0.7 years. However, we find no evidence of a significant impact of the reform on the number of children that women had at the age cutoff of 27 years. This lack of impact is consistent with evidence from previous studies that the number of births may decline only at younger ages, and the effect may disappear at older ages as completed fertility catches up over time (Breierova and Duflo 2004).[27] In Panel B of Table A10, we replicate the results in Table 7 using a static bandwidth of 85 months around the discontinuity. The results are similar to those reported in Table 7. We also find no evidence of a significant relationship between being exposed to childhood maltreatment and fertility outcomes based on results in

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[27]Erten and Keskin (2017) found that the same reform induced a decline in the number of children for women exposed to the reform being younger than 21 years old in 2008. The combination of our findings in these two papers implies that while compulsory schooling may initially lead to a reduction in the number of early births, as women grow older, they tend to catch up in terms of completed fertility outcomes.

Tables 7 and A10. Hence, the fertility channel does not appear to explain our main results.

5.4   Changes in Labor Market Outcomes

An increase in maternal educational improvement may also result in better labor market outcomes for mothers, including a higher probability of finding a job and having a personal income.[28] In turn, mothers' increased economic empowerment and access to resources may allow them to more effectively respond to children's needs, resulting in a lower propensity to resort to violence for discipline (Paxson and Waldfogel 2002). Low-income mothers may face higher levels of stress due to the scarcity of resources to which they can obtain access, which in turn may lead to harsher parenting practices (Straus and Mathur 1996; Dietz 2000; Eamon 2001). On the one hand, additional years of schooling may result in higher returns in the labor market, relaxing mothers' budgetary constraints and reducing the financial stress that they face. On the other hand, if women's working conditions are harsh, being employed may act as an additional stressor and may therefore induce more physical abuse against their children. Moreover, if women with a history of childhood violence have lower cognitive ability or non-cognitive traits that disqualify them from attaining certain skills, exposure to increased education may reduce these skill gaps and enable them to experience a stronger improvement in labor market outcomes.

We examine this mechanism by testing whether the reform-induced increase in female schooling had a significant impact on the labor market outcomes of women exposed to childhood violence.[29] Table 8 presents the results. The OLS estimates in columns 1 and 4 indicate the presence of a positive correlation between female years of schooling and labor market outcomes. For example, for the sample of the main compliers in rural regions, one additional year of schooling is associated with a 1.4 ppt increase in being employed, a 2.1 ppt increase in working in the service sector, and a 2.1 ppt increase in having a job with social security benefits.

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[28]The empirical evidence on the effects of compulsory schooling on labor market outcomes is mixed. While some studies find a positive impact of increased female schooling on the probability of being employed or of having a personal income (Erten and Keskin 2017), others find no evidence of a significant impact on labor market outcomes (Gulesci and Meyersson 2012). It is important to note that age of the women in question may affect whether we observe a significant impact on labor market participation and other labor market outcomes. In the context of Turkey and other developing countries, it is well documented that women who complete their education participate in the labor market at younger ages; however, they tend to drop out after they marry and have children (Dayıoğlu and Kırdar 2010). Hence, although the reform is likely to have a significant impact on women's employment by providing them with better skills at younger ages (e.g., 20-21 years old), the effects are likely to disappear once the women have children and begin to drop out of the labor market (e.g., 27-28 years old). This finding suggests that the impact of education reforms may vary over the lifetime of women, particularly in countries that lack a social infrastructure for childcare. If public childcare facilities are not common and private childcare is difficult to afford at lower income levels, many women may opt to be stay-at-home mothers and assume childcare responsibilities.

[29]Unfortunately, the labor market outcomes are all measured only for the seven days prior to the survey date, while our measures of violence against children capture a much longer time span. Therefore, our findings in this subsection can be regarded as evidence suggestive of this potential channel.

The RD estimates in columns 2-3 and 5-6 of Table 8 indicate no evidence of a significant impact of the reform on labor market outcomes. The RD treatment effects on interaction terms of exposure to the reform and to childhood violence also show no evidence of a significant differential impact on the labor market outcomes of women who experienced childhood abuse. As a robustness check, Table A11 in Online Appendix B provides the RD treatment effects without interaction terms in Panel A and the RD treatment effects with interaction terms using a static bandwidth of 85 months around the discontinuity in Panel B. The estimates in Panel A confirm that we find no evidence of a significant effect of the reform on labor market outcomes.[30] The RD estimates in Panel B use a static bandwidth to reestimate the results in Table 8 as a robustness check. We find no evidence of a significant impact of the reform on the labor market outcomes of women exposed to childhood maltreatment. Thus, the labor market channel does not seem to explain our main results.

5.5 Changes in Partner Characteristics and Marriage Market Outcomes

An additional channel through which mothers' education may impact child physical abuse is that an increase in female education may result in a better match with a 'higher-quality' partner. If additional years of schooling allow women to have a more educated or less violence-prone partner, this assortative matching may result in a decline in child physical abuse by the mother to the extent that the male partner may oppose it. In addition, if increased female education allows women to freely choose their own partners, it may also lead to a reduction in marital conflict, inducing mothers to less frequently resort to physical child maltreatment (Gulesci and Meyersson 2012). Moreover, if women with a history of childhood violence have a lower probability of choosing their spouses, or marrying a less educated partner, an increase in female schooling may allow women to act outside of the traditional norms and increase their possibilities of matching with a higher-quality partner.

We explore this channel by testing whether the additional years of female schooling induced by the reform had a differential effect on the partner characteristics and marriage market outcomes of women exposed to childhood violence. In Table 9, the OLS estimates in columns 1 and 4 indicate that female years of schooling are positively correlated with a woman's partner's years of schooling, partner's age, her marriage age, and her marriage decision. In particular, the correlations show that one additional year of schooling for a woman raised in rural regions

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[30]Erten and Keskin (2017) found that the same reform induced an improvement in the labor market outcomes of women for the age cutoff of 21 years. Our results show that for an age cutoff of 27 years, the previously estimated effects may no longer be present since a much larger fraction of women have children by age 27, which reduces their potential to participate in the labor market due to childcare responsibilities. This finding implies that while compulsory schooling may lead to an improvement in labor market outcomes, as women grow older and have children, they tend to drop out of the labor market.

is associated with a half-year increase in her partner's schooling, a 0.27-year increase in her partner's age, a 0.29-year increase in her marriage age, and a 3.8 ppt increase in her probability of deciding whom she will marry.

The RD estimates reported in columns 2-3 and 5-6 of Table 9 indicate that there is no evidence of a significant impact on the interaction terms of exposure to the reform and childhood violence, with the exception of only two of twelve RD estimates.[31] We also find no evidence of a significant impact of the reform on partner's age, partner's religiosity index, marriage age, and ever having been divorced, while we find some evidence of a significant positive impact on the partner's years of schooling and marriage decision for women raised in rural regions. Table A12 in Online Appendix B confirms these RD treatment effects without interaction terms in Panel A. In Panel B of Table A12, we reestimate the results in Table 9 using a static bandwidth of 85 months as a robustness check. The results are similar to those reported in Table 9. Overall, we conclude that the marriage market channel does not appear to explain our main results.

5.6   Changes in Spousal Violence

An improvement in female education may also affect the probability that a woman experiences spousal violence. If additional years of schooling economically empower women and improve their bargaining position within the household, these factors may lead to a decline in the probability of facing spousal violence and in turn result in lower levels of stress and child abuse. However, if such economic empowerment creates incentives for male partners to extract rents from women, it may lead to an increase in violence or threats of violence as an instrument of control (Erten and Keskin 2017). This situation may in turn create a higher risk of perpetrating maltreatment of children if abused women divert their anger toward their children (O'Keefe 1995). If women with a history of childhood violence are also at a higher risk of experiencing spousal violence, exposure to additional years of schooling may alter this risk and produce a differential impact on how they behave toward their children.

Earlier work has shown that the same education reform led to an improvement in the labor market outcomes of women (i.e. those younger than 21 years old in 2008), as measured by the indicators of work within the last week, and the increase in women's personal income generated incentives for male partners to use violence to extract rents from women. Since the exit options from marriage are highly stigmatized in rural regions of Turkey, the instrumental

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[31]The interaction term for the partner's years of schooling in the overall sample is significant only in the RF specification. The IV estimate for the overall sample and the RF and IV estimates for the rural sample are insignificant. In addition, the interaction term for marriage decision in the rural sample is significant only in the RF specification. The IV estimate for the rural sample and the RF and IV estimates for the overall samples are insignificant. Thus, we find no robust evidence of a significant differential impact on the partner's years of schooling or marriage decision for women exposed to childhood violence.

use of violence dominated the bargaining channel as the threat of exiting a marriage was not credible. As a result, the increased female schooling resulted in more psychological violence and financial control behavior being experienced by women (Erten and Keskin 2017). However, we have now shown that for the age cutoff of 27 years, there is no evidence of a change in the labor market outcomes of women, most likely due to the completed fertility outcomes and increase in childcare responsibilities. As a result, the underlying mechanism for the increase in psychological violence and financial control behavior is no longer present for this age group.

We examine this channel by testing whether the reform had a differential impact on the spousal violence indicators of women exposed to childhood violence. In Table 10, the correlations reported in columns 1 and 4 indicate that female education is negatively correlated with experiencing physical and psychological violence from spouses. The RD estimates reported in columns 2-3 and 5-6 show that we find no evidence of a significant impact of the reform on any of the spousal violence indicators, including the physical violence, psychological violence, and financial control indices. This result is consistent with the prediction we derived from earlier work. Since the reform does not lead to an improvement of women's income, male partners do not have an incentive to use instruments of violence to extract resources from women. Moreover, none of the interaction terms of being exposed to the reform and childhood maltreatment are significant, indicating that the reform did not have a differential impact on women with a history of childhood maltreatment. To check for robustness, Table A13 in Online Appendix B reports the RD treatment effects of the reform without the interaction terms in Panel A and the RD treatment effects of the reform including interaction terms and using a static bandwidth in Panel B. The findings are very similar to those reported in Table 10. Hence, we find no evidence for spousal violence as a potential channel to explain our main results.

As an additional robustness check, we use an alternative optimal bandwidth selection method proposed by Calonico et al. (2014) to test whether the main outcome variables used in the analysis of channels are sensitive to the use of this alternative method. Table A14 in Online Appendix B shows that the RD treatment effect estimates using the CCT bandwidth selectors reported in columns 2 and 3 are similar in magnitude and statistical significance to those reported in columns 5 and 6 using the IK bandwidths.

6 Concluding Remarks

In this paper, we exploit the extension of compulsory schooling in Turkey from five to eight years to examine whether exposure to increased education may mitigate the risk of the intergenerational transmission of violence against children. In particular, our paper is the first to causally examine whether education has any impact on a woman's risk of perpetrating child physical abuse and whether this varies by her own history of childhood violence. We find that the reform led to an average increase of one year of additional schooling for women, and the

main compliers were women who grew up in rural regions. Our findings reveal that the reform had heterogeneous effects on the risk of perpetrating the maltreatment of children. It led to a decrease in the likelihood of physical child abuse only for women who were raised in rural areas and experienced abuse when they were children. This finding implies that increasing the years of education of women reduces the intergenerational transmission of violence by altering the behavior of violence-exposed mothers toward their children.

After quantifying the impact of education on the prevalence of child abuse for this high-risk group, we explore the potential mechanisms underlying this effect. We find no evidence of a differential impact of the reform on attitudes toward violence, labor market outcomes, partner characteristics, spousal violence, or the fertility decisions of women who experienced childhood maltreatment compared to nonmaltreated mothers. However, women in the treated cohorts and with a history of childhood abuse are more likely to experience an improvement in their mental health outcomes.

Our results may be interpreted as evidence for the role of education in improving the ability to regulate emotions and address the negative effects of emotional dysfunction on mental health, which in turn reduce violent behaviors toward children. The mental health channel offers an effective explanation of the reduction in the intergenerational transmission of violence through two mechanisms. First, if a mother's exposure to childhood maltreatment has traumatized her and compromised her capacity to regulate emotions, a reform-induced improvement in mental health (e.g., a lower probability of experiencing both the somatic and nonsomatic symptoms of depression) may make her less impulsive in reacting to children, reducing the probability of maltreatment perpetration. Second, if being exposed to childhood violence has compromised the attachment of the mother to her own family and altered her reading of social cues so that she perceives them as threatening, an improvement in her mental health due to increased education may result in a reduction in her sensitivity or hypervigilance to the behavior of children that she may perceive as threatening. Such an improvement in encoding social cues may reduce the risk that she will physically abuse her children. Overall, our results may be interpreted as showing a reduction in the intergenerational transmission of violence by mitigating the risks of child maltreatment that emerges from trauma symptoms and attachment issues, which are greatly reduced through the positive effects of education on maternal mental health.

In contrast, we find no evidence that increased maternal education improves the violence-related attitudes of women by allowing them to interact with better role models in the school environment. This finding might imply that education does not necessarily reduce the intergenerational transmission of violence by mitigating the probability of child maltreatment resulting from social learning effects, i.e., the imitation of violent behavior learned from the family environment. This could result from the fact either that teachers and peers do not have significantly different attitudes or that the transfer of alternative attitudes is limited in this context.

Overall, our findings indicate that the extension of compulsory schooling in Turkey had a significant impact in reducing the intergenerational transmission of violence against children. Given that such intergenerational transmission plays a crucial role in explaining child maltreatment perpetration, one of the policy implications of our study is that improving the educational attainment of women through devising educational programs can be an effective means of breaking the cycle of violence across generations. Moreover, our study also reveals that the underlying channel for this effect is a differential improvement in the mental health of women in the treated cohorts who have experienced childhood abuse. This mechanism underscores the importance of education in regulating emotional dysfunction and reducing child maltreatment as a result. Given the recent scientific evidence on the crucial role of the adolescent years for brain development and emotional regulation, our findings also have important implications for the design of schooling reforms that target this vulnerable age group to improve their behavioral outcomes later in life.

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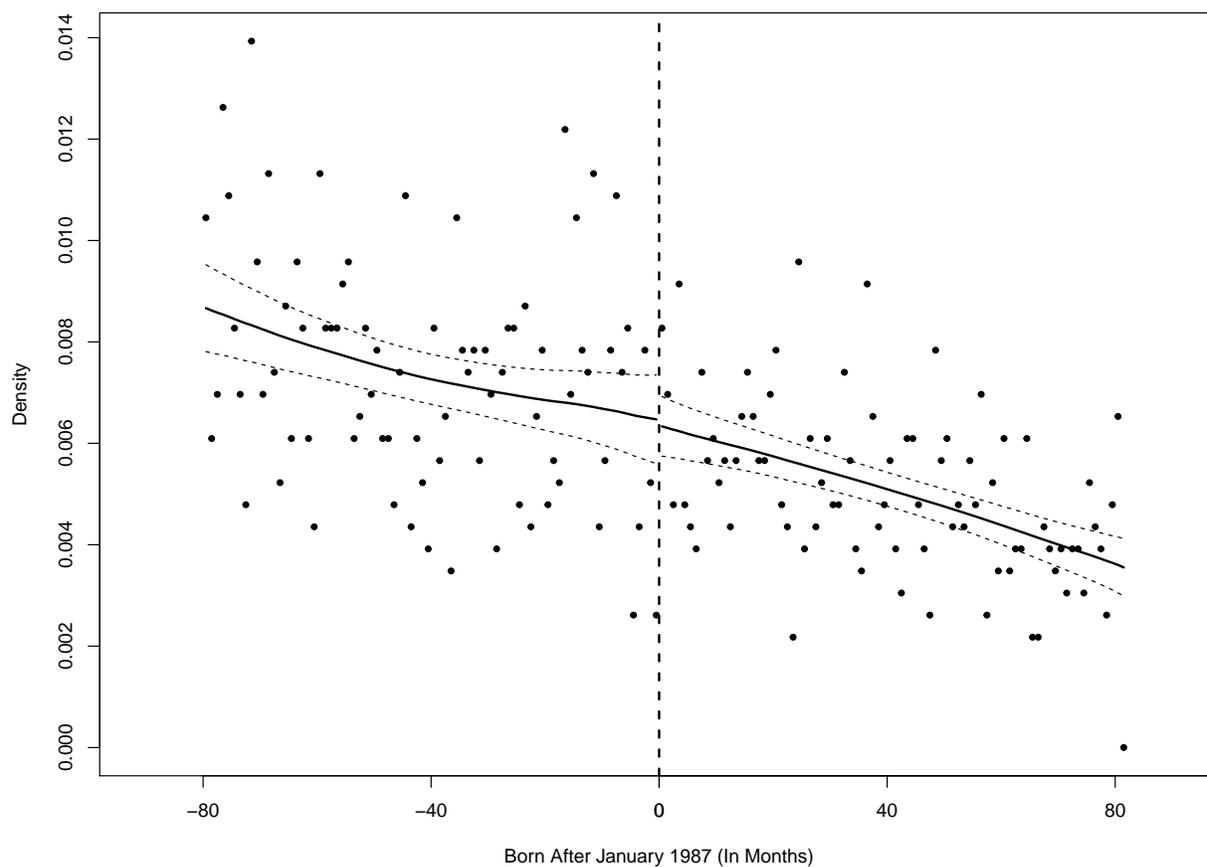
Note: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. The graph shows the results of the McCrary test of whether there is a discontinuity in the density of the forcing variable, the month of birth.

FIGURE 2: BALANCED COVARIATES
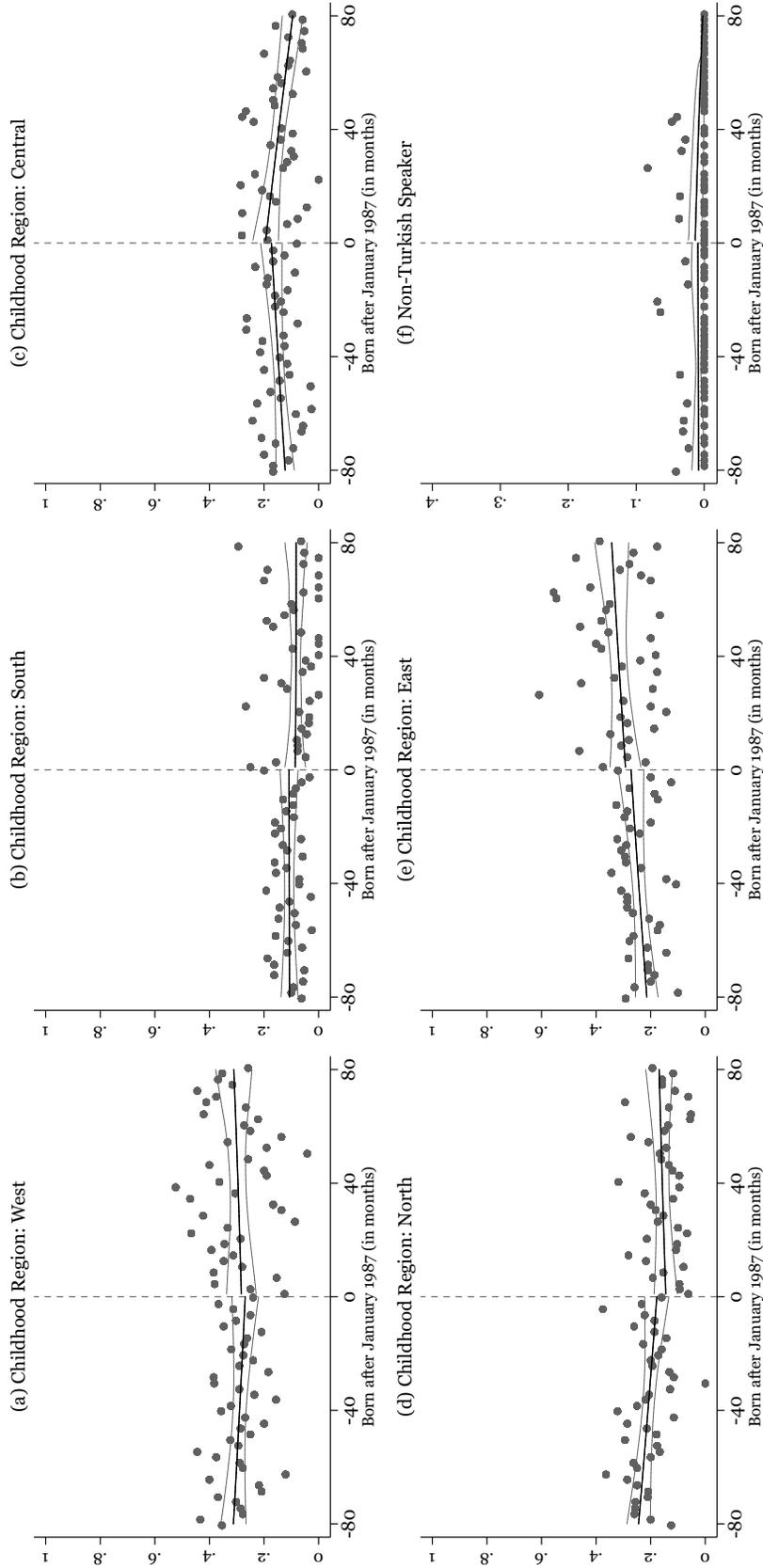
(a) Childhood Region: West

(b) Childhood Region: South

(c) Childhood Region: Central

(d) Childhood Region: North

(e) Childhood Region: East

(f) Non-Turkish Speaker

Note: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. The figures plot predetermined covariates in monthly bins against the month-year of birth of being born in January 1987. The vertical line in each graph represents the cut-off point, January 1987. Gray lines show 95 percent confidence intervals around the mean level. Variable definitions are listed in Online Appendix A.

FIGURE 3: RD TREATMENT EFFECTS ON JUNIOR HIGH SCHOOL COMPLETION

Women with Completed Junior High School

Men with Completed Junior High School
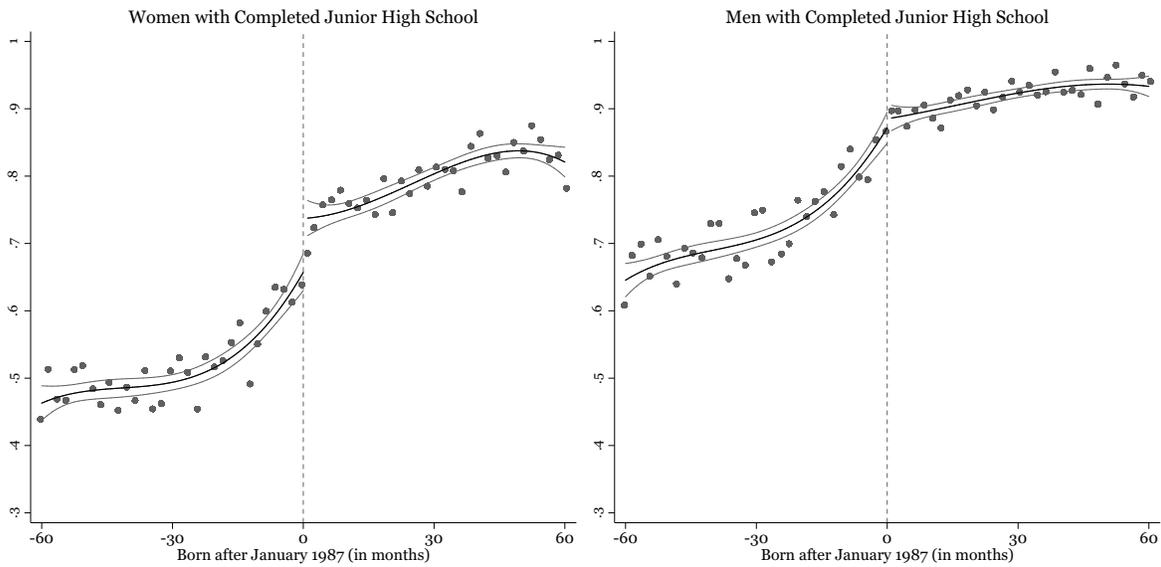
Note: Data are from the 2014 Household Labor Force Survey. Figures plot junior high school completion rates in monthly bins for women on the left and men on the right. Gray lines show 95 percent confidence intervals around the mean level.

FIGURE 4: TREATMENT AND PLACEBO

2014 TNSDVW Survey

2008 TNSDVW Survey
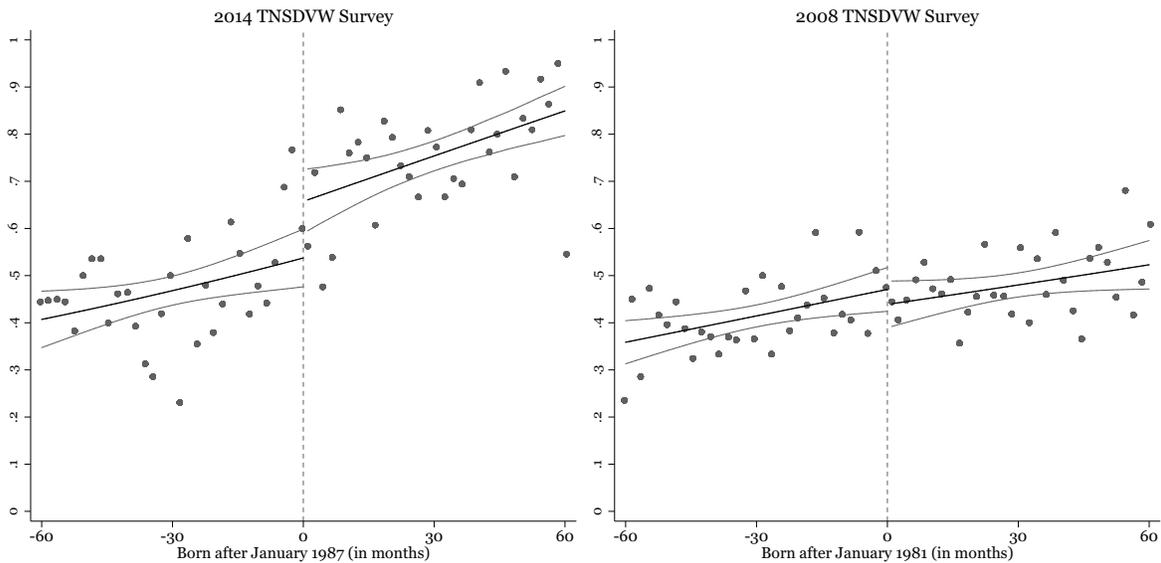
Note: Data are from the 2014 and 2008 National Surveys on Domestic Violence against Women in Turkey, respectively. The figures plot a dummy variable equal to one of the respondent completed junior high school in monthly bins. Gray lines show 95 percent confidence intervals around the mean level.

TABLE 1: SUMMARY STATISTICS FOR 20- TO 34-YEAR-OLD WOMEN WHO HAVE CHILDREN

| | Region of Childhood | | | Difference | |
	All (1) Mean (S.D.)	Rural (2) Mean (S.D.)	Urban (3) Mean (S.D.)	(2) – (3) (4) Est. (S.E.)	(5) Observations (All/Rural/Urban)
Panel A: Education					
Years of schooling	7.52 (3.86)	6.73 (3.72)	8.55 (3.78)	-1.82*** (0.21)	1,807/1,101/686
Completed junior high school	0.51 (0.50)	0.42 (0.49)	0.63 (0.48)	-0.21*** (0.03)	1,808/1,101/686
Completed high school	0.31 (0.46)	0.22 (0.41)	0.42 (0.49)	-0.20*** (0.03)	1,808/1,101/686
Completed primary school	0.89 (0.31)	0.87 (0.33)	0.92 (0.27)	-0.05*** (0.02)	1,808/1,101/686
Panel B: Violence against children					
Child abuse	0.48 (0.50)	0.51 (0.50)	0.45 (0.50)	0.06** (0.03)	1,800/1,096/683
Frequent child abuse	0.41 (0.49)	0.44 (0.50)	0.38 (0.48)	0.06** (0.03)	1,800/1,096/683
Panel C: Attitudes against violence					
Men can beat their partners in certain situations.	0.38 (0.48)	0.41 (0.49)	0.32 (0.47)	0.09*** (0.03)	1,712/1,040/651
It may be necessary to beat children for discipline.	0.29 (0.45)	0.30 (0.46)	0.27 (0.45)	0.03 (0.03)	1,801/1,097/683
Panel D: Mental health outcomes					
Overall depression index	0.02 (0.52)	0.04 (0.52)	-0.01 (0.53)	0.05* (0.03)	1,808/1,101/686
Somatic depression index	0.00 (0.65)	0.02 (0.65)	-0.03 (0.65)	0.06 (0.04)	1,808/1,101/686
Nonsomatic depression index	0.02 (0.52)	0.04 (0.52)	-0.01 (0.53)	0.05 (0.03)	1,808/1,101/686
Panel E: Fertility outcomes					
Age at first pregnancy †	21.34 (3.68)	21.08 (3.55)	21.60 (3.77)	-0.52*** (0.20)	1,905/1,143/737
Number of children †	1.50 (1.18)	1.70 (1.21)	1.27 (1.08)	0.43*** (0.05)	2,425/1,387/1,006

TABLE 1: SUMMARY STATISTICS FOR 20- TO 34-YEAR-OLD WOMEN WHO HAVE CHILDREN, CONT'D

	Region of Childhood			Difference	
	All	Rural	Urban	(2) − (3)	
	(1)	(2)	(3)	(4)	(5)
	Mean	Mean	Mean	Est.	Observations
	(S.D.)	(S.D.)	(S.D.)	(S.E.)	(All/Rural/Urban)
Panel F: Labor market outcomes					
Employed	0.19	0.18	0.21	-0.03	1,808/1,101/686
	(0.39)	(0.38)	(0.41)	(0.02)	
Employed in services	0.14	0.11	0.18	-0.07***	1,808/1,101/686
	(0.34)	(0.31)	(0.38)	(0.02)	
Social security	0.11	0.09	0.14	-0.05***	1,808/1,101/686
	(0.31)	(0.29)	(0.35)	(0.02)	
Personal income index	-0.08	-0.10	-0.04	-0.05**	1,808/1,101/686
	(0.45)	(0.44)	(0.47)	(0.03)	
Asset ownership index	0.08	0.03	0.17	-0.14***	1,808/1,101/686
	(0.35)	(0.34)	(0.34)	(0.02)	
Panel G: Marriage market outcomes					
Partner's years of schooling	8.81	8.43	9.31	-0.88***	1,792/1,088/684
	(3.61)	(3.55)	(3.62)	(0.21)	
Partner's age	24.81	24.60	25.04	-0.44*	1,805/1,099/685
	(4.25)	(4.31)	(4.11)	(0.24)	
Partner's religiosity index	0.01	0.06	-0.07	0.12**	1,808/1,101/686
	(0.64)	(0.53)	(0.78)	(0.04)	
Marriage age	21.28	21.09	21.48	-0.39**	1,805/1,099/685
	(3.37)	(3.36)	(3.37)	(0.19)	
Marriage decision	0.57	0.52	0.64	-0.12***	1,808/1,101/686
	(0.49)	(0.50)	(0.48)	(0.03)	
Divorced	0.06	0.05	0.06	-0.02	1,808/1,101/686
	(0.23)	(0.21)	(0.25)	(0.01)	
Panel H: Spousal violence outcomes					
Physical violence index	0.00	0.00	0.00	0.00	1,808/1,101/686
	(0.79)	(0.79)	(0.80)	(0.05)	
Psychological violence index	0.05	0.05	0.04	0.01	1,808/1,101/686
	(0.55)	(0.54)	(0.58)	(0.03)	
Financial control index	-0.04	-0.05	-0.05	0.00	1,801/1,096/684
	(0.80)	(0.78)	(0.81)	(0.04)	
Panel I: Covariates					
Rural childhood region	0.59	1.00	0.00	1.00***	1,787/1,101/686
	(0.49)	(0.00)	(0.00)	(0.00)	
Non-Turkish speaker	0.01	0.01	0.00	0.01***	1,808/1,101/686
	(0.09)	(0.09)	(0.03)	(0.00)	
Childhood violence	0.14	0.14	0.14	-0.01	1,742/1,057/664
	(0.35)	(0.34)	(0.35)	(0.02)	

Notes: The table presents the means, standard deviations, and number of observations from the 2014 National Survey on Domestic Violence against Women in Turkey. The sample includes women who have children and who were born within 85 months before or after January 1987. † denotes that age at first pregnancy is reported for women who have ever been pregnant, and number of children is reported for all women, born within 85 months around the discontinuity. Columns 1 - 3 report means and standard deviations in parentheses. Column 4 reports differences in the group means between columns 2 and 3 with standard errors in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively. The sum of rural and urban samples is less than the full sample due to missing observations in the region of childhood variable. The variables are described in Appendix A.

TABLE 2: RD TREATMENT EFFECTS ON SCHOOLING OUTCOMES

	(1) Linear RD $\hat{h}$ bandwidth	(2) Linear RD 0.75$\hat{h}$ bandwidth	(3) Linear RD 1.5$\hat{h}$ bandwidth	(4) Bandwidth	(5) N	(6) Mean
Years of schooling	0.704** (0.303)††	0.769** (0.349)†	1.031*** (0.249)†††	89	2,492	8.48
Completed education:						
Junior high school	0.192*** (0.031)†††	0.186*** (0.037)†††	0.186*** (0.027)†††	118	3,308	0.60
High school	0.125*** (0.044)††	0.081* (0.048)	0.078** (0.038)†	65	1,837	0.40
Primary school	-0.020 (0.024)	-0.031 (0.028)	-0.020 (0.020)	93	2,630	0.91

Notes: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. Columns 1 – 3 report local RD regressions with linear polynomials in the month-year of birth using the optimal bandwidth $\hat{h}$, 0.75 $\hat{h}$ and 1.5 $\hat{h}$, respectively. The optimal bandwidth, reported in column 4, is estimated by using the Imbens and Kalyanaraman (2009) algorithm. Column 5 reports the number of observations used in estimations with the optimal bandwidth $\hat{h}$, and column 6 reports the outcome mean within the optimal bandwidth $\hat{h}$. All results are reported for the full sample of women. The variables are described in Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively (based on p-values unadjusted for multiple-hypothesis testing). †††, ††, and † denote significance at the 1, 5, and 10 percent levels, respectively (based on p-values adjusted for multiple-hypothesis testing using Simes adjustment).

TABLE 3: RD TREATMENT EFFECTS ON SCHOOLING BY REGION OF CHILDHOOD

	Rural childhood region			Urban childhood region		
	(1) $\hat{h}$	(2) 0.75 $\hat{h}$	(3) 1.5 $\hat{h}$	(4) $\hat{h}$	(5) 0.75 $\hat{h}$	(6) 1.5 $\hat{h}$
Bandwidth:						
Panel A: Sample of All Women						
Years of schooling	1.134** (0.451)	1.080** (0.513)	1.283*** (0.364)	0.413 (0.454)	0.560 (0.474)	0.763* (0.391)
Mean	7.42	7.40	7.47	9.68	9.73	9.51
Bandwidth	85	64	128	98	74	147
Observations	1,396	1,052	2,038	1,147	866	1,710
Panel B: Sample of Women Who Have Children						
Years of schooling	1.115** (0.539)	1.039* (0.605)	1.103** (0.460)	-0.141 (0.514)	-0.136 (0.552)	-0.395 (0.481)
Mean	6.81	6.80	6.68	8.52	8.32	8.55
Bandwidth	80	60	120	72	54	143
Observations	1,032	779	1,455	596	458	825

Notes: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. Columns 1 – 3, and 4 – 6 report local RD regressions with linear polynomials in the month-year of birth using the optimal bandwidth $\hat{h}$, 0.75 $\hat{h}$, and 1.5 $\hat{h}$, respectively. The outcome mean, optimal bandwidth estimated by the Imbens and Kalyanaraman (2009) algorithm, and observation numbers are reported in the rows under the dependent variables. Columns 1 – 3 report the results for the sample of women who grew up in a rural region, and columns 4 – 6 report them for the sample of women who grew up in an urban region. Panel A reports the results for the sample of all women, and Panel B reports them for the sample of women who have children. The variables are described in Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

TABLE 4: EFFECTS OF EDUCATION ON VIOLENCE AGAINST CHILDREN

		Overall sample			Rural sample		
		(1) OLS	(2) RF	(3) IV	(4) OLS	(5) RF	(6) IV
Panel A: RD Treatment Effects							
Child abuse	Schooling	-0.021***	0.007	0.033	-0.024***	0.024	0.025
		(0.003)†††	(0.046)	(0.226)	(0.005)†††	(0.067)	(0.068)
	Mean	0.49	0.49	0.49	0.51	0.51	0.51
	Bandwidth	94	94	94	89	89	89
	Observations	1,932	1,932	1,932	1,140	1,140	1,140
Frequent child abuse	Schooling	-0.020***	0.056	0.196	-0.021***	0.053	0.058
		(0.003)†††	(0.043)	(0.269)	(0.005)†††	(0.072)	(0.085)
	Mean	0.42	0.42	0.42	0.44	0.44	0.44
	Bandwidth	106	106	106	92	92	92
	Observations	2,131	2,131	2,131	1,164	1,164	1,164
Panel B: RD Treatment Effects by Exposure to Childhood Violence							
Child abuse	Schooling	-0.019***	0.032	0.746	-0.023***	0.083	0.097
		(0.004)†††	(0.047)	(5.790)	(0.005)†††	(0.068)	(0.096)
	Schooling × Childhood violence	-0.000	-0.060	-0.623	0.002	-0.224**	-0.148**
		(0.011)	(0.084)	(4.547)	(0.018)	(0.103)††	(0.074)††
	Childhood violence	0.198**	0.224***	4.979	0.238*	0.341***	1.308**
		(0.086)††	(0.049)†††	(34.975)	(0.123)	(0.056)†††	(0.531)††
	Mean	0.49	0.49	0.49	0.51	0.51	0.51
	Bandwidth	94	94	94	89	89	89
	Observations	1,864	1,864	1,864	1,096	1,096	1,096
Frequent child abuse	Schooling	-0.018***	0.080*	1.065	-0.020***	0.108	0.134
		(0.003)†††	(0.044)	(4.903)	(0.005)†††	(0.073)	(0.122)
	Schooling × Childhood violence	-0.011	-0.060	-0.957	-0.001	-0.268**	-0.185**
		(0.011)	(0.081)	(4.309)	(0.018)	(0.103)††	(0.084)††
	Childhood violence	0.257***	0.204***	7.475	0.250*	0.344***	1.557**
		(0.095)††	(0.046)†††	(32.864)	(0.134)	(0.062)†††	(0.613)††
	Mean	0.42	0.42	0.42	0.44	0.44	0.44
	Bandwidth	106	106	106	92	92	92
	Observations	2,055	2,055	2,055	1,119	1,119	1,119

Notes: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. The sample includes women who have children. The optimal bandwidth is estimated by using the Imbens and Kalyanaraman (2009) algorithm. Panel A reports the RD treatment effects of the reform, and Panel B reports them by exposure to childhood violence, i.e., whether the respondent experienced violence from her own family members during her childhood. Columns 1 reports OLS results using years of schooling as the independent variable for an optimal bandwidth $\hat{h}$ estimated by the Imbens and Kalyanaraman algorithm. Columns 2 – 3 report reduced-form RD treatment effects and two-stage least-squares RD treatment effects (by using treatment as an instrument for years of schooling) of being born after January 1987 with a linear control function in the month-year of birth on each side of the discontinuity. Columns 1 – 3 report these results for the overall sample, and columns 4 – 6 report results from same specifications for the subsample of respondents whose childhood region of residence was rural. The variables are described in Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level.

*** Significant at the 1 percent level.
 ** Significant at the 5 percent level.
  * Significant at the 10 percent level.
(Based on p-values unadjusted for multiple-hypothesis testing.)

††† Significant at the 1 percent level.
 †† Significant at the 5 percent level.
  † Significant at the 10 percent level.
(Based on p-values adjusted for multiple-hypothesis testing using Simes adjustment.)

TABLE 5: EFFECTS OF EDUCATION ON ATTITUDES TOWARD VIOLENCE

		Overall sample			Rural sample		
		(1) OLS	(2) RF	(3) IV	(4) OLS	(5) RF	(6) IV
Men can beat their partners in certain situations.	Schooling	-0.024***	0.046	0.154	-0.023***	0.053	0.052
		(0.004)†††	(0.055)	(0.233)	(0.005)†††	(0.068)	(0.070)
	Schooling × Childhood violence	-0.003	-0.066	-0.162	-0.010	-0.067	-0.056
		(0.011)	(0.092)	(0.206)	(0.013)	(0.110)	(0.065)
	Childhood violence	0.114	0.113**	1.323	0.169	0.120**	0.490
		(0.091)	(0.048)††	(1.561)	(0.103)	(0.056)†	(0.446)
	Mean	0.38	0.38	0.38	0.41	0.41	0.41
	Bandwidth	83	83	83	88	88	88
	Observations	1,589	1,589	1,589	1,039	1,039	1,039
It may be necessary to beat children for discipline.	Schooling	-0.018***	0.035	0.357	-0.024***	0.025	0.027
		(0.003)†††	(0.042)	(1.522)	(0.004)†††	(0.049)	(0.055)
	Schooling × Childhood violence	-0.005	0.017	-0.297	-0.008	0.009	-0.009
		(0.010)	(0.065)	(1.388)	(0.013)	(0.084)	(0.048)
	Childhood violence	0.091	0.058	2.329	0.119	0.075	0.147
		(0.088)	(0.039)	(10.558)	(0.104)	(0.052)	(0.344)
	Mean	0.28	0.28	0.28	0.29	0.29	0.29
	Bandwidth	107	107	107	99	99	99
	Observations	2,056	2,056	2,056	1,189	1,189	1,189

Notes: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. The sample includes women who have children. The optimal bandwidth is estimated by using the Imbens and Kalyanaraman (2009) algorithm. The RD treatment effects of the reform are reported by exposure to childhood violence, i.e., whether the respondent experienced violence from her own family members during her childhood. Columns 1 reports OLS results using years of schooling as the independent variable for an optimal bandwidth $\hat{h}$ estimated by the Imbens and Kalyanaraman algorithm. Columns 2 – 3 report reduced-form RD treatment effects and two-stage least-squares RD treatment effects (by using treatment as an instrument for years of schooling) of being born after January 1987 with a linear control function in the month-year of birth on each side of the discontinuity. Columns 1 – 3 report these results for the overall sample, and columns 4 – 6 report results from the same specifications for the subsample of respondents whose childhood region of residence was rural. The variables are described in Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.
(Based on p-values unadjusted for multiple-hypothesis testing.)

††† Significant at the 1 percent level.
†† Significant at the 5 percent level.
† Significant at the 10 percent level.
(Based on p-values adjusted for multiple-hypothesis testing using Simes adjustment.)

TABLE 6: EFFECTS OF EDUCATION ON MENTAL HEALTH OUTCOMES

		Overall sample			Rural sample		
		(1) OLS	(2) RF	(3) IV	(4) OLS	(5) RF	(6) IV
Overall depression index	Schooling	-0.023***	0.001	0.114	-0.022***	0.017	0.039
		(0.005)†††	(0.050)	(0.277)	(0.005)†††	(0.067)	(0.088)
	Schooling × Childhood violence	0.005	-0.126	-0.203	0.004	-0.286**	-0.169**
		(0.012)	(0.096)	(0.270)	(0.015)	(0.115)††	(0.081)†
	Childhood violence	0.284***	0.364***	1.852	0.257**	0.368***	1.449**
		(0.100)†††	(0.044)†††	(2.055)	(0.111)††	(0.056)†††	(0.575)††
	Mean	0.02	0.02	0.02	0.03	0.03	0.03
	Bandwidth	77	77	77	115	115	115
	Observations	1,584	1,584	1,584	1,360	1,360	1,360
Somatic depression index	Schooling	-0.037***	0.025	0.410	-0.033***	0.054	0.071
		(0.006)†††	(0.059)	(1.264)	(0.007)†††	(0.075)	(0.112)
	Schooling × Childhood violence	0.029**	-0.249**	-0.583	0.013	-0.340**	-0.192*
		(0.014)	(0.111)†	(1.185)	(0.025)	(0.143)††	(0.103)†
	Childhood violence	0.062	0.350***	4.657	0.122	0.333***	1.543**
		(0.106)	(0.054)†††	(9.023)	(0.170)	(0.081)†††	(0.736)††
	Mean	0.00	0.00	0.00	0.03	0.03	0.03
	Bandwidth	106	106	106	96	96	96
	Observations	2,045	2,045	2,045	1,167	1,167	1,167
Nonsomatic depression index	Schooling	-0.022***	0.002	0.077	-0.019***	0.040	0.054
		(0.005)†††	(0.055)	(0.228)	(0.005)†††	(0.077)	(0.106)
	Schooling × Childhood violence	-0.001	-0.098	-0.147	-0.011	-0.290**	-0.160**
		(0.013)	(0.102)	(0.245)	(0.017)	(0.127)††	(0.081)†
	Childhood violence	0.342***	0.370***	1.443	0.378***	0.407***	1.414**
		(0.112)†††	(0.049)†††	(1.857)	(0.120)†††	(0.063)†††	(0.575)††
	Mean	0.03	0.03	0.03	0.04	0.04	0.04
	Bandwidth	75	75	75	95	95	95
	Observations	1,518	1,518	1,518	1,156	1,156	1,156

Notes: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. The sample includes women who have children. The optimal bandwidth is estimated by using the Imbens and Kalyanaraman (2009) algorithm. The RD treatment effects of the reform are reported by exposure to childhood violence, i.e., whether the respondent experienced violence from her own family members during her childhood. Columns 1 reports OLS results using years of schooling as the independent variable for an optimal bandwidth $\hat{h}$ estimated by the Imbens and Kalyanaraman algorithm. Columns 2 – 3 report reduced-form RD treatment effects and two-stage least-squares RD treatment effects (by using treatment as an instrument for years of schooling) of being born after January 1987 with a linear control function in the month-year of birth on each side of the discontinuity. Columns 1 – 3 report these results for the overall sample, and columns 4 – 6 report results from the same specifications for the subsample of respondents whose childhood region of residence was rural. The variables are described in Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.
(Based on p-values unadjusted for multiple-hypothesis testing.)

††† Significant at the 1 percent level.
†† Significant at the 5 percent level.
† Significant at the 10 percent level.
(Based on p-values adjusted for multiple-hypothesis testing using Simes adjustment.)

TABLE 7: EFFECTS OF EDUCATION ON FERTILITY OUTCOMES

		Overall sample			Rural sample		
		(1) OLS	(2) RF	(3) IV	(4) OLS	(5) RF	(6) IV
Age at first pregnancy	Schooling	0.442***	0.147	0.016	0.360***	0.650*	0.672
		(0.027)†††	(0.259)	(1.519)	(0.037)†††	(0.359)	(0.437)
	Schooling × Childhood violence	-0.096	0.823*	0.920	-0.095	0.871	0.161
		(0.078)	(0.448)	(1.862)	(0.083)	(0.547)	(0.401)
	Childhood violence	0.514	-0.407	-7.147	0.767	0.036	-0.778
		(0.601)	(0.325)	(14.394)	(0.599)	(0.411)	(2.922)
	Mean	21.48	21.48	21.48	21.14	21.14	21.14
	Bandwidth	121	121	121	106	106	106
	Observations	2,385	2,385	2,385	1,336	1,336	1,336
Number of children	Schooling	-0.141***	-0.124	-0.149	-0.146***	-0.135	-0.139
		(0.007)†††	(0.102)	(0.130)	(0.010)†††	(0.113)	(0.120)
	Schooling × Childhood violence	-0.023	-0.084	-0.014	-0.014	-0.109	0.006
		(0.016)	(0.150)	(0.075)	(0.026)	(0.198)	(0.077)
	Childhood violence	0.065	-0.038	0.023	-0.059	-0.045	-0.164
		(0.173)	(0.108)	(0.664)	(0.242)	(0.125)	(0.616)
	Mean	1.51	1.51	1.51	1.69	1.69	1.69
	Bandwidth	73	73	73	88	88	88
	Observations	1,963	1,963	1,963	1,382	1,382	1,382

Notes: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. The sample includes all women. The optimal bandwidth is estimated by using the Imbens and Kalyanaraman (2009) algorithm. The RD treatment effects of the reform are reported by exposure to childhood violence, i.e., whether the respondent experienced violence from her own family members during her childhood. Columns 1 reports OLS results using years of schooling as the independent variable for an optimal bandwidth $\hat{h}$ estimated by the Imbens and Kalyanaraman algorithm. Columns 2 – 3 report reduced-form RD treatment effects and two-stage least-squares RD treatment effects (by using treatment as an instrument for years of schooling) of being born after January 1987 with a linear control function in the month-year of birth on each side of the discontinuity. Columns 1 – 3 report these results for the overall sample, and columns 4 – 6 report results from the same specifications for the subsample of respondents whose childhood region of residence was rural. The variables are described in Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level.

*** Significant at the 1 percent level.
 ** Significant at the 5 percent level.
  * Significant at the 10 percent level.
(Based on p-values unadjusted for multiple-hypothesis testing.)

††† Significant at the 1 percent level.
 †† Significant at the 5 percent level.
  † Significant at the 10 percent level.
(Based on p-values adjusted for multiple-hypothesis testing using Simes adjustment.)

		Overall sample			Rural sample		
		(1) OLS	(2) RF	(3) IV	(4) OLS	(5) RF	(6) IV
Employed	Schooling	0.019***	0.024	0.478	0.014***	0.027	0.034
		(0.004)†††	(0.032)	(3.048)	(0.004)†††	(0.037)	(0.046)
	Schooling × Childhood violence	-0.001	-0.037	-0.400	-0.003	-0.061	-0.044
		(0.009)	(0.064)	(2.417)	(0.014)	(0.079)	(0.049)
	Childhood violence	0.052	0.053	3.102	0.087	0.088	0.378
		(0.067)	(0.042)	(18.546)	(0.100)	(0.055)	(0.360)
	Mean	0.19	0.19	0.19	0.18	0.18	0.18
	Bandwidth	96	96	96	93	93	93
	Observations	1,891	1,891	1,891	1,139	1,139	1,139
Employed in services	Schooling	0.024***	0.004	0.114	0.021***	-0.001	0.001
		(0.004)†††	(0.027)	(0.592)	(0.004)†††	(0.031)	(0.032)
	Schooling × Childhood violence	-0.003	-0.070	-0.146	-0.006	-0.059	-0.029
		(0.008)	(0.052)	(0.479)	(0.014)	(0.058)	(0.037)
	Childhood violence	0.045	0.040	1.117	0.063	0.046	0.224
		(0.055)	(0.038)	(3.682)	(0.093)	(0.046)	(0.279)
	Mean	0.14	0.14	0.14	0.11	0.11	0.11
	Bandwidth	94	94	94	88	88	88
	Observations	1,871	1,871	1,871	1,097	1,097	1,097
Social security	Schooling	0.024***	0.017	0.113	0.021***	-0.003	-0.002
		(0.003)†††	(0.026)	(0.235)	(0.004)†††	(0.028)	(0.030)
	Schooling × Childhood violence	-0.005	-0.042	-0.119	-0.007	-0.042	-0.018
		(0.009)	(0.043)	(0.195)	(0.012)	(0.051)	(0.032)
	Childhood violence	0.036	0.009	0.898	0.054	0.021	0.132
		(0.057)	(0.035)	(1.507)	(0.072)	(0.046)	(0.250)
	Mean	0.11	0.11	0.11	0.10	0.10	0.10
	Bandwidth	81	81	81	97	97	97
	Observations	1,658	1,658	1,658	1,186	1,186	1,186
Personal income index	Schooling	0.023***	-0.040	-1.089	0.013***	-0.052	-0.053
		(0.003)†††	(0.040)	(14.771)	(0.005)†††	(0.058)	(0.074)
	Schooling × Childhood violence	0.003	-0.061	0.927	0.026	-0.069	-0.011
		(0.013)	(0.055)	(13.656)	(0.033)	(0.091)	(0.069)
	Childhood violence	-0.028	0.001	-7.134	-0.218	-0.020	0.019
		(0.087)	(0.041)	(104.576)	(0.190)	(0.077)	(0.528)
	Mean	-0.05	-0.05	-0.05	-0.10	-0.10	-0.10
	Bandwidth	143	143	143	89	89	89
	Observations	2,532	2,532	2,532	1,101	1,101	1,101
Asset ownership index	Schooling	0.030***	-0.011	-0.087	0.031***	0.019	0.017
		(0.002)†††	(0.021)	(0.213)	(0.002)†††	(0.028)	(0.024)
	Schooling × Childhood violence	-0.012**	0.054	0.109	-0.006	0.046	0.015
		(0.006)	(0.038)	(0.174)	(0.008)	(0.046)	(0.026)
	Childhood violence	0.036	-0.074***	-0.877	-0.015	-0.070**	-0.151
		(0.039)	(0.023)†††	(1.332)	(0.054)	(0.030)	(0.184)
	Mean	0.03	0.03	0.03	0.00	0.00	0.00
	Bandwidth	84	84	84	87	87	87
	Observations	1,697	1,697	1,697	1,076	1,076	1,076

Notes: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. The sample includes women who have children. The optimal bandwidth is estimated by using the Imbens and Kalyanaraman (2009) algorithm. The RD treatment effects of the reform are reported by exposure to childhood violence, i.e., whether the respondent experienced violence from her own family members during her childhood. Columns 1 reports OLS results using years of schooling as the independent variable for an optimal bandwidth $\hat{h}$ estimated by the Imbens and Kalyanaraman algorithm. Columns 2 – 3 report reduced-form RD treatment effects and two-stage least-squares RD treatment effects (by using treatment as an instrument for years of schooling) of being born after January 1987 with a linear control function in the month-year of birth on each side of the discontinuity. Columns 1 – 3 report these results for the overall sample, and columns 4 – 6 report results from the same specifications for the subsample of respondents whose childhood region of residence was rural. The variables are described in Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively (based on p-values unadjusted for multiple-hypothesis testing). †††, ††, and † denote significance at the 1, 5, and 10 percent levels, respectively (based on p-values adjusted for multiple-hypothesis testing using Simes adjustment).

TABLE 9: EFFECT OF EDUCATION ON PARTNER CHARACTERISTICS AND MARRIAGE MARKET OUTCOMES

		Overall sample			Rural sample		
		(1) OLS	(2) RF	(3) IV	(4) OLS	(5) RF	(6) IV
Partner's years of schooling	Schooling	0.515***	0.494	2.371	0.502***	1.043**	1.149*
		(0.028)†††	(0.351)	(5.235)	(0.034)†††	(0.440)†	(0.669)
	Schooling × Childhood violence	-0.046	1.081**	-1.095	-0.150	0.647	-0.301
		(0.066)	(0.537)	(4.608)	(0.095)	(0.726)	(0.566)
	Childhood violence	-0.034	-0.885***	8.326	0.632	-0.730*	1.901
		(0.542)	(0.322)††	(35.514)	(0.765)	(0.420)	(4.052)
	Mean	8.80	8.80	8.80	8.47	8.47	8.47
	Bandwidth	87	87	87	79	79	79
	Observations	1,748	1,748	1,748	979	979	979
Partner's age	Schooling	0.302***	0.138	0.250	0.272***	0.281	0.287
		(0.034)†††	(0.398)	(3.792)	(0.043)†††	(0.549)	(0.611)
	Schooling × Childhood violence	-0.172	0.965	0.664	0.122	0.481	0.134
		(0.136)	(0.869)	(3.705)	(0.132)	(0.889)	(0.585)
	Childhood violence	1.985*	0.419	-4.102	-0.174	0.579	-0.152
		(1.099)	(0.380)	(28.070)	(0.851)	(0.550)	(4.186)
	Mean	24.89	24.89	24.89	24.71	24.71	24.71
	Bandwidth	106	106	106	111	111	111
	Observations	2,059	2,059	2,059	1,323	1,323	1,323
Partner's religiosity index	Schooling	0.004	0.001	0.023	-0.001	-0.019	-0.037
		(0.005)	(0.068)	(0.292)	(0.004)	(0.069)	(0.078)
	Schooling × Childhood violence	0.023	-0.028	-0.043	-0.017	0.217	0.129
		(0.032)	(0.169)	(0.303)	(0.032)	(0.160)	(0.110)
	Childhood violence	-0.457*	-0.282***	0.033	-0.084	-0.279**	-1.112
		(0.265)	(0.106)††	(2.323)	(0.242)	(0.124)	(0.824)
	Mean	0.01	0.01	0.01	0.06	0.06	0.06
	Bandwidth	84	84	84	82	82	82
	Observations	1,718	1,718	1,718	1,022	1,022	1,022
Marriage age	Schooling	0.325***	0.006	-0.253	0.286***	-0.308	-7.786
		(0.030)†††	(0.445)	(8.375)	(0.036)†††	(0.556)	(152.057)
	Schooling × Childhood violence	-0.254**	-0.130	0.055	-0.236*	-0.213	3.345
		(0.110)†	(0.660)	(4.236)	(0.127)	(0.669)	(68.125)
	Childhood violence	1.547**	-0.265	-0.814	1.766**	0.062	-25.265
		(0.714)	(0.376)	(32.998)	(0.779)†	(0.345)	(512.013)
	Mean	20.94	20.94	20.94	20.74	20.74	20.74
	Bandwidth	37	37	37	40	40	40
	Observations	820	820	820	539	539	539
Marriage decision	Schooling	0.035***	0.131**	0.346	0.038***	0.162***	0.146*
		(0.004)†††	(0.051)†	(0.560)	(0.005)†††	(0.057)††	(0.086)
	Schooling × Childhood violence	0.040***	0.100	-0.108	0.041***	0.265***	0.064
		(0.010)†††	(0.081)	(0.430)	(0.011)†††	(0.085)††	(0.072)
	Childhood violence	-0.352***	-0.094*	0.795	-0.292***	-0.103*	-0.409
		(0.080)†††	(0.050)	(3.302)	(0.092)††	(0.060)	(0.518)
	Mean	0.58	0.58	0.58	0.53	0.53	0.53
	Bandwidth	66	66	66	87	87	87
	Observations	1,378	1,378	1,378	1,076	1,076	1,076
Divorced	Schooling	-0.001	-0.015	-0.037	-0.001	0.012	0.013
		(0.002)	(0.023)	(0.101)	(0.002)	(0.025)	(0.026)
	Schooling × Childhood violence	-0.003	-0.006	0.018	-0.001	-0.025	-0.019
		(0.009)	(0.046)	(0.106)	(0.006)	(0.040)	(0.027)
	Childhood violence	0.069	0.046	-0.095	0.031	0.030	0.156
		(0.069)	(0.035)	(0.817)	(0.050)	(0.032)	(0.203)
	Mean	0.05	0.05	0.05	0.05	0.05	0.05
	Bandwidth	71	71	71	122	122	122
	Observations	1,444	1,444	1,444	1,414	1,414	1,414

Notes: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. The sample includes women who have children. The optimal bandwidth is estimated by using the Imbens and Kalyanaraman (2009) algorithm. The RD treatment effects of the reform are reported by exposure to childhood violence, i.e., whether the respondent experienced violence from her own family members during her childhood. Columns 1 reports OLS results using years of schooling as the independent variable for an optimal bandwidth $\hat{h}$ estimated by the Imbens and Kalyanaraman algorithm. Columns 2 – 3 report reduced-form RD treatment effects and two-stage least-squares RD treatment effects (by using treatment as an instrument for years of schooling) of being born after January 1987 with a linear control function in the month-year of birth on each side of the discontinuity. Columns 1 – 3 report these results for the overall sample, and columns 4 – 6 report results from the same specifications for the subsample of respondents whose childhood region of residence was rural. The variables are described in Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively (based on p-values unadjusted for multiple-hypothesis testing). †††, ††, and † denote significance at the 1, 5, and 10 percent levels, respectively (based on p-values adjusted for multiple-hypothesis testing using Simes adjustment).

TABLE 10: EFFECTS OF EDUCATION ON SPOUSAL VIOLENCE

		Overall sample			Rural sample		
		(1) OLS	(2) RF	(3) IV	(4) OLS	(5) RF	(6) IV
Physical violence index	Schooling	-0.016***	-0.112	-0.371	-0.021***	-0.040	-0.031
		(0.006)††	(0.079)	(0.813)	(0.007)†††	(0.089)	(0.104)
	Schooling × Childhood violence	-0.061***	-0.124	0.127	-0.044*	-0.303	-0.141
		(0.022)††	(0.187)	(0.704)	(0.026)	(0.197)	(0.129)
	Childhood violence	1.005***	0.600***	-0.462	0.754***	0.547***	1.419
		(0.195)†††	(0.115)†††	(5.387)	(0.233)†††	(0.130)†††	(0.919)
	Mean	0.00	0.00	0.00	0.01	0.01	0.01
	Bandwidth	83	83	83	108	108	108
	Observations	1,682	1,682	1,682	1,311	1,311	1,311
Psychological violence index	Schooling	-0.021***	0.024	0.271	-0.025***	0.036	0.041
		(0.004)†††	(0.057)	(1.600)	(0.005)†††	(0.084)	(0.090)
	Schooling × Childhood violence	0.006	0.027	-0.215	0.004	-0.097	-0.071
		(0.016)	(0.112)	(1.494)	(0.027)	(0.133)	(0.090)
	Childhood violence	0.298***	0.338***	1.987	0.252	0.315***	0.785
		(0.114)†††	(0.051)†††	(11.353)	(0.189)	(0.076)†††	(0.648)
	Mean	0.04	0.04	0.04	0.06	0.06	0.06
	Bandwidth	115	115	115	79	79	79
	Observations	2,177	2,177	2,177	980	980	980
Financial control index	Schooling	-0.008	0.064	0.335	-0.009	0.141	0.147
		(0.005)	(0.070)	(0.606)	(0.007)	(0.108)	(0.132)
	Schooling × Childhood violence	-0.041	-0.082	-0.303	-0.076**	-0.191	-0.177
		(0.026)	(0.162)	(0.507)	(0.032)†	(0.174)	(0.131)
	Childhood violence	0.546**	0.280***	2.569	0.661**	0.220	1.426
		(0.228)††	(0.091)†††	(3.884)	(0.298)††	(0.141)	(0.981)
	Mean	-0.04	-0.04	-0.04	-0.04	-0.04	-0.04
	Bandwidth	83	83	83	74	74	74
	Observations	1,675	1,675	1,675	926	926	926

Notes: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. The sample includes women who have children. The optimal bandwidth is estimated by using the Imbens and Kalyanaraman (2009) algorithm. The RD treatment effects of the reform are reported by exposure to childhood violence, i.e., whether the respondent experienced violence from her own family members during her childhood. Columns 1 reports OLS results using years of schooling as the independent variable for an optimal bandwidth $\hat{h}$ estimated by the Imbens and Kalyanaraman algorithm. Columns 2 – 3 report reduced-form RD treatment effects and two-stage least-squares RD treatment effects (by using treatment as an instrument for years of schooling) of being born after January 1987 with a linear control function in the month-year of birth on each side of the discontinuity. Columns 1 – 3 report these results for the overall sample, and columns 4 – 6 report results from the same specifications for the subsample of respondents whose childhood region of residence was rural. The variables are described in Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level.

*** Significant at the 1 percent level.
 ** Significant at the 5 percent level.
  * Significant at the 10 percent level.
(Based on p-values unadjusted for multiple-hypothesis testing.)

††† Significant at the 1 percent level.
 †† Significant at the 5 percent level.
  † Significant at the 10 percent level.
(Based on p-values adjusted for multiple-hypothesis testing using Simes adjustment.)

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Appendix A   List of Variables

Outcome Variables:

- Years of schooling: Number of years of school that the respondent completed.

- Completed junior high school: A dummy variable equal to one if the respondent completed junior high school or above (i.e., completed at least 8 years of schooling).

- Completed high school: A dummy variable equal to one if the respondent completed high school or above (i.e., completed at least 11 years of schooling).

- Completed primary school: A dummy variable equal to one if the respondent completed primary school or above (i.e., completed at least 5 years of schooling).

- Child abuse: A dummy variable equal to one if the respondent has ever hit or physically abused her children.

- Frequent child abuse: A dummy variable equal to one if the respondent has hit or physically abused her children frequently, e.g., a number of times, or many times.

- Men can beat their partners in certain situations: A dummy variable equal to one if the respondent agrees with the statement that men can beat their partners in certain situations.

- It may be necessary to beat children for discipline: A dummy variable equal to one if the respondent agrees with the statement that it may be necessary to beat children for discipline.

- Age at first pregnancy: The age of the respondent during her first pregnancy.

- Number of children: The number of children that the respondent has.

- Employed: A dummy variable equal to one if the respondent was employed last week.

- Employed in services: A dummy variable equal to one if the respondent was employed in services last week.

- Social security: A dummy variable equal to one if the respondent had social security benefits from her job last week.

- Personal income index: A z-score constructed by averaging the z-scores of the income dummy variables, which are calculated by using the mean and standard deviation of the variable. These dummy variables take the value of one if the respondent earns a personal income from the following six sources: rent from owning land, rent from owning a house, income from owning a company or workplace, income from owning a vehicle, having money in the bank, and income from other asset ownership.

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- Asset ownership index: A z-score constructed by averaging the z-scores of the asset ownership dummy variables, which are calculated by using the mean and standard deviation of the variable. These dummy variables take the value of one if the respondent's household owns the asset. The following assets are included: refrigerator, deep freezer, gas/electric oven, microwave oven, dishwasher, garbage dispenser, washing machine, drying machine, iron, vacuum cleaner, plasma TV (LCD), home theater, television, satellite TV, paid TV service, DVD/VCD player, cellphone, non-mobile telephone, laptop/tablet computer, desktop computer, internet, air conditioner, car, taxi/mini-bus/bus or other commercial vehicles, and tractor.

- Partner's years of schooling: Number of years of school completed by the respondent's partner.

- Partner's age: The age of the respondent's partner.

- Partner's religiosity index: A z-score calculated as an average of z-scores of partners' characteristics, including a dummy variable that takes the value of one if the partner never drinks alcoholic beverages, a dummy variable that takes the value of one if the partner never gambles, a dummy variable that takes the value of one if the partner never uses narcotic drugs, and a dummy variable that takes the value of one of the partner never had an affair.

- Marriage age: The age of the respondent at the time of her first marriage.

- Marriage decision: A dummy variable equal to one if the respondent decided on marriage together with her husband instead of the decision being made by her or his family.

- Divorced: A dummy variable equal to one if the respondent has ever divorced.

- Physical violence index: A z-score constructed by averaging the z-scores from each of the 6 physical violence indicators, including dummy variables that equal one if the respondent reports that she experienced intimate partner violence acts of (i) slapping or throwing an object that would hurt; (ii) pushing, shoving, or pulling hair; (iii) hitting with his fist or in a way that hurts; (iv) kicking, pushing on the ground, or beating; and (v) choking or burning.

- Psychological violence index: A z-score constructed by averaging the z-scores from each of the following indicators, including dummy variables that equal one if the respondent reports that she experienced intimate partner violence acts of (i) insulting, (ii) humiliating, (iii) scaring or threatening, (iv) attempting to isolate her from her friends, (v) attempting to prevent contact with her family, (vi) insisting on knowing her location, (vii) ignoring her, (viii) becoming angry if she speaks to other men, (ix) suspecting that she is cheating on him, (x) wanting his permission before she seeks healthcare, and (xi) intervening in her clothing choices.

- Financial control index: A z-score constructed by averaging the z-scores from two of the financial control behaviors, including dummy variables that equal one if the respondent reports that she experienced the following behaviors from her intimate partner: (i) taking income from her despite her disapproval and (ii) refusing to give her money for household spending.

- Somatic depression index: A z-score calculated by averaging the z-scores from each of the 4 somatic depression indicators, including dummy variables equal to one if the respondent reports that she experienced the following within the last four weeks: (i) frequent headaches, (ii) trembling hands, (iii) digestion problems, and (iv) heartburn or other stomach problems.

- Nonsomatic depression index: A z-score calculated by averaging the z-scores from each of the 16 nonsomatic depression indicators, including dummy variables equal to one if the respondent reports that she experienced the following within the last four weeks: (i) appetite loss, (ii) trouble sleeping, (iii) felt easily frightened from several things, (iv) felt anxious or nervous, (v) had trouble in thinking clearly, (vi) felt unhappy, (vii) cried more often, (viii) did not enjoy daily activities, (ix) had difficulty making decisions, (x) delayed daily activities, (xi) felt useless, (xii) lost interest in activities that she previously enjoyed, (xiii) felt worthless, (xiv) thought about suicide, (xv) felt tired all the time, and (xvi) got tired easily.

- Overall depression index: A z-score calculated by averaging the z-scores from 20 depression indicators, including 4 somatic and 16 nonsomatic depression indicators, as listed above.

- Childhood region, rural: A dummy variable equal to one if the respondent lived in a rural village or district until she was 12 years old.

- Childhood region, urban: A dummy variable equal to one if the respondent lived in an urban area until she was 12 years old.

Covariates:

- Non-Turkish Speaker: A dummy variable equal to one if the respondent speaks a non-Turkish language as her primary language.

- Region dummies: Dummy variables for each of the 12 regions where the respondents lived until they were 12 years old.

- Childhood violence: A dummy variable equal to one if the respondent experienced physical or sexual violence from her own family after age of 15.

Outcome Variables in Appendix B:

- Childhood violence intensity: A dummy variable equal to one if the respondent experienced violence from her own family often during childhood.

- Childhood violence (overall): A dummy variable equal to one if the respondent experienced violence from her own family or others such as teachers, strangers, etc. during childhood.

- Childhood violence intensity (overall): A dummy variable equal to one if the respondent experienced violence from her own family or others such as teachers, strangers, etc. often during childhood.

- Home violence: A dummy variable equal to one if the respondent witnessed her mother experiencing domestic violence from her husband.

FOR ONLINE PUBLICATION

Appendix B Additional Figures and Tables – For Online Publication

FIGURE A1: PERCENTAGE OF CHILDREN AGED 2 TO 14 YEARS WHO EXPERIENCED ANY VIOLENT DISCIPLINE (PSYCHOLOGICAL AGGRESSION AND/OR PHYSICAL PUNISHMENT) IN THE PAST MONTH
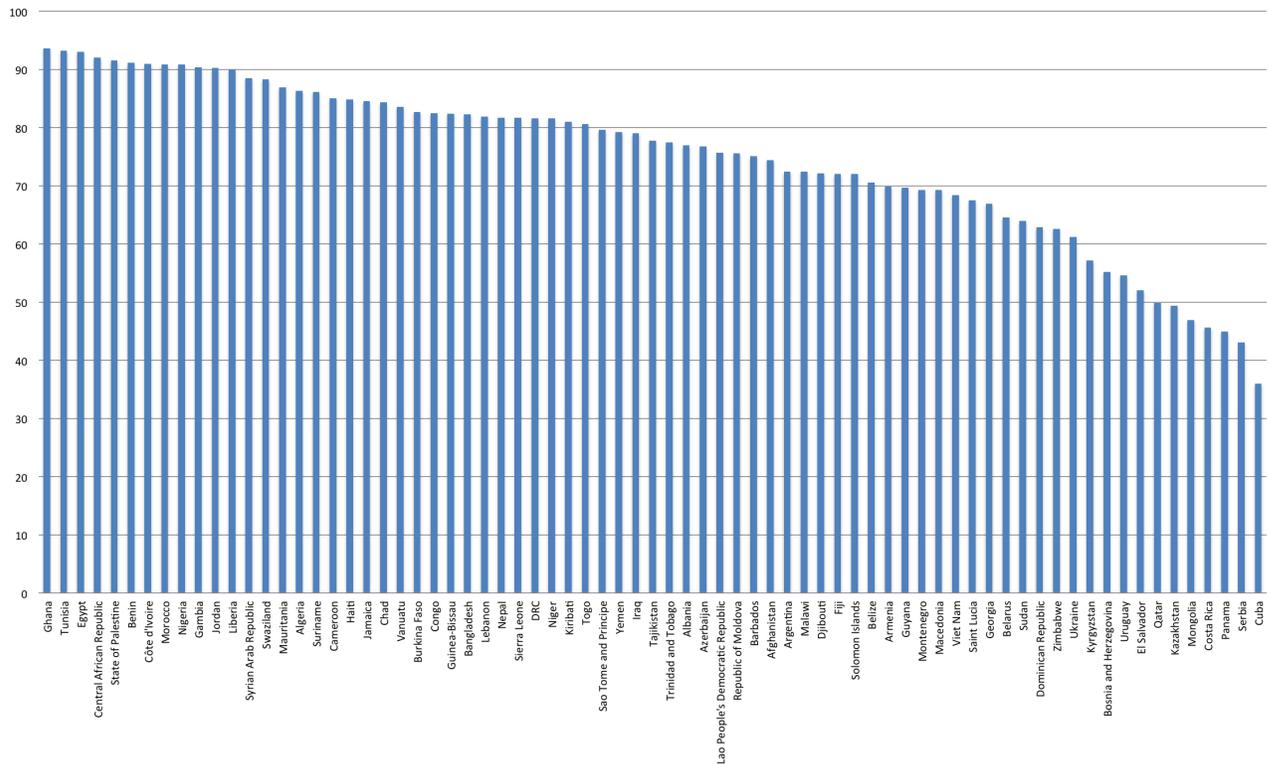
Note: Data are from UNICEF global databases, 2016, based on Demographic and Health Surveys (DHS) and Multiple Indicator Cluster Surveys (MICS) (2005-2015), accessed from `https://data.unicef.org/topic/child-protection/violence/violent-discipline/` on June 20, 2017.

		Overall sample	Rural sample
		(1) RF	(2) RF
Years of schooling	Schooling	0.662* (0.336)†	0.979** (0.467)††
	Schooling × Childhood violence	0.608 (0.544)	0.904 (0.683)
	Childhood violence	-0.544 (0.371)	-0.560 (0.445)
	Mean	8.48	7.43
	Bandwidth	89	89
	Observations	2,377	1,382
Completed junior high school	Schooling	0.180*** (0.033)†††	0.248*** (0.050)†††
	Schooling × Childhood violence	0.081 (0.054)	0.113 (0.070)
	Childhood violence	-0.037 (0.040)	-0.042 (0.054)
	Mean	0.60	0.49
	Bandwidth	118	118
	Observations	3,162	1,832

Notes: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. The sample includes all women. The optimal bandwidth is estimated by using the Imbens and Kalyanaraman (2009) algorithm. The RD treatment effects of the reform are reported by exposure to childhood violence, i.e., whether the respondent experienced violence from her own family members during her childhood. Columns 1 and 2 report reduced-form RD treatment effects of being born after January 1987 with a linear control function in the month-year of birth on each side of the discontinuity for the overall sample and the subsample of women whose childhood region is rural. The variables are described in Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level.
*** Significant at the 1 percent level.
 ** Significant at the 5 percent level.
   * Significant at the 10 percent level.
(Based on p-values unadjusted for multiple-hypothesis testing.)

††† Significant at the 1 percent level.
 †† Significant at the 5 percent level.
   † Significant at the 10 percent level.
(Based on p-values adjusted for multiple-hypothesis testing using Simes adjustment.)

		Overall sample			Rural sample		
		(1) OLS	(2) RF	(3) IV	(4) OLS	(5) RF	(6) IV
Childhood violence	Schooling	-0.002	-0.004	-0.006	-0.001	-0.034	-0.036
		(0.002)	(0.032)	(0.046)	(0.003)	(0.039)	(0.044)
	Mean	0.15	0.15	0.15	0.14	0.14	0.14
	Bandwidth	95	95	95	95	95	95
	Observations	2,526	2,526	2,526	1,471	1,471	1,471
Childhood violence intensity	Schooling	-0.004**	-0.001	-0.002	-0.005**	-0.006	-0.005
		(0.002)††	(0.021)	(0.03)	(0.002)†	(0.028)	(0.025)
	Mean	0.08	0.08	0.08	0.09	0.09	0.09
	Bandwidth	91	91	91	91	91	91
	Observations	2,423	2,423	2,423	1,408	1,408	1,408
Childhood violence (overall)	Schooling	-0.001	-0.026	-0.041	-0.001	-0.061	-0.061
		(0.003)	(0.032)	(0.052)	(0.003)	(0.04)	(0.047)
	Mean	0.18	0.18	0.18	0.17	0.17	0.17
	Bandwidth	91	91	91	91	91	91
	Observations	2573	2573	2573	1494	1494	1494
Childhood violence intensity (overall)	Schooling	-0.001	-0.026	-0.041	-0.001	-0.061	-0.061
		(0.003)	(0.032)	(0.052)	(0.003)	(0.04)	(0.047)
	Mean	0	0	0	0	0	0
	Bandwidth	91	91	91	91	91	91
	Observations	2573	2573	2573	1494	1494	1494
Number of children	Schooling	-0.088***	-0.007	-0.017	-0.094***	-0.019	-0.018
		(0.006)†††	(0.073)	(0.181)	(0.009)†††	(0.103)	(0.092)
	Mean	1.99	1.99	1.99	2.08	2.08	2.08
	Bandwidth	86	86	86	86	86	86
	Observations	1806	1806	1806	1112	1112	1112
Has children	Schooling	-0.047***	-0.06	-0.072	-0.039***	-0.057	-0.049
		(0.003)†††	(0.044)	(0.052)	(0.004)†††	(0.047)	(0.04)
	Mean	0.76	0.76	0.76	0.81	0.81	0.81
	Bandwidth	83	83	83	83	83	83
	Observations	2,332	2,332	2,332	1,357	1,357	1,357

Notes: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. The sample includes all women. The optimal bandwidth is estimated by using the Imbens and Kalyanaraman (2009) algorithm. Column 1 reports OLS results using years of schooling as the independent variable for an optimal bandwidth $\hat{h}$ estimated by the Imbens and Kalyanaraman algorithm. Columns 2 – 3 report reduced-form RD treatment effects and two-stage least-squares RD treatment effects (by using treatment as an instrument for years of schooling) of being born after January 1987 with a linear control function in the month-year of birth on each side of the discontinuity. Columns 1 – 3 report these results for the overall sample, and columns 4 – 6 report results from the same specifications for the subsample of respondents whose childhood region of residence was rural. The variables are described in Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

(Based on p-values unadjusted for multiple-hypothesis testing.)

††† Significant at the 1 percent level.

†† Significant at the 5 percent level.

† Significant at the 10 percent level.

(Based on p-values adjusted for multiple-hypothesis testing using Simes adjustment.)

Outcome	(1) Linear RD $\hat{h}$ bandwidth	(2) Linear RD $0.75\hat{h}$ bandwidth	(3) Linear RD $1.5\hat{h}$ bandwidth	(4) Bandwidth	(5) N	(6) Mean
Years of schooling	0.825** (0.335)††	0.674* (0.369)†	0.981*** (0.274)†††	85	2,386	8.48
Completed education:						
Junior high school	0.201*** (0.037)†††	0.172*** (0.04)†††	0.186*** (0.03)†††	85	2,386	0.59
High school	0.092** (0.041)††	0.116*** (0.043)††	0.160*** (0.038)†††	85	2,386	0.40
Primary school	-0.018 (0.026)	-0.032 (0.029)	-0.017 (0.021)	85	2,386	0.91

Notes: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. All columns use a static bandwidth of 85 months, which is the optimal bandwidth estimated for the years of schooling in rural regions of childhood. Columns 1 – 3 report local RD regressions with linear polynomials in the month-year of birth using the static bandwidth $\hat{h}$, 0.75 $\hat{h}$ and 1.5 $\hat{h}$, respectively. Column 5 reports the number of observations used in estimations, and column 6 reports the outcome mean within the static bandwidth. All results are reported for the full sample of women. The variables are described in Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.
(Based on p-values unadjusted for multiple-hypothesis testing.)

††† Significant at the 1 percent level.
†† Significant at the 5 percent level.
† Significant at the 10 percent level.
(Based on p-values adjusted for multiple-hypothesis testing using Simes adjustment.)

	(1)	(2)
Outcome	Quadratic RD optimal bandwidth	Quadratic RD static bandwidth
Years of schooling	0.694**	0.808**
	0.303††	(0.339)††
Mean	8.48	8.48
Bandwidth	89	85
Observations	2,492	2,386
Completed education:		
Junior high school	1.191***	0.201***
	(0.031)†††	(0.037)†††
Mean	0.60	0.59
Bandwidth	118	85
Observations	3,308	2,386
High school	0.123***	0.091**
	(0.045)††	(0.042)††
Mean	0.40	0.40
Bandwidth	65	85
Observations	1,837	2,386
Primary school	-0.022	-0.021
	(0.025)	(0.026)
Mean	0.91	0.91
Bandwidth	93	85
Observations	2,630	2,386

Notes: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. All results are reported for the full sample of women. Columns 1 and 2 report local RD regressions with quadratic polynomials in the month-year of birth using the optimal bandwidth estimated by the Imbens and Kalyanaraman (2009) algorithm, and the static bandwidth of 85 months, which is the optimal bandwidth estimated for the years of schooling in rural regions of childhood, respectively. The variables are described in Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level.
*** Significant at the 1 percent level.
 ** Significant at the 5 percent level.
  * Significant at the 10 percent level.
(Based on p-values unadjusted for multiple-hypothesis testing.)

††† Significant at the 1 percent level.
 †† Significant at the 5 percent level.
  † Significant at the 10 percent level.
(Based on p-values adjusted for multiple-hypothesis testing using Simes adjustment.)

	Rural childhood region			Urban childhood region		
	(1)	(2)	(3)	(4)	(5)	(6)
Bandwidth:	ĥ	0.75 ĥ	1.5 ĥ	ĥ	0.75 ĥ	1.5 ĥ
Panel A: Sample of All Women						
Years of schooling	1.160**	1.112**	1.307***	0.523	0.439	0.526
	(0.456)	(0.518)	(0.367)	(0.468)	(0.507)	(0.399)
Mean	7.42	7.40	7.47	9.68	9.68	9.64
Bandwidth	85	64	128	85	64	128
Observations	1,385	1,036	2,027	1,001	747	1,508
Panel B: Sample of Women Who Have Children						
Years of schooling	1.151**	1.103*	1.184***	-0.328	-0.235	-0.671
	(0.517)	(0.578)	(0.452)	(0.509)	(0.478)	(0.459)
Mean	6.73	6.81	6.70	8.55	8.44	8.52
Bandwidth	85	64	128	85	64	128
Observations	1,100	847	1,504	684	521	933

Notes: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. Columns 1 – 3, and 4 – 6 report local RD regressions with linear polynomials in the month-year of birth using the static bandwidth ĥ, 0.75 ĥ, and 1.5 ĥ, respectively. The static bandwidth is 85 months, which is the optimal bandwidth estimated for the years of schooling in rural regions of childhood. The outcome mean, bandwidth, and observation numbers are reported in the rows under the dependent variables. Columns 1 – 3 report the results for the sample of women who grew up in a rural region, and columns 4 – 6 report them for the sample of women who grew up in an urban region. Panel A reports the results for the sample of all women, and Panel B reports them for the sample of women who have children. The variables are described in Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

TABLE A6: EFFECTS OF EDUCATION ON VIOLENCE AGAINST CHILDREN (STATIC BAND-WIDTH)

		Overall sample			Rural sample		
		(1) OLS	(2) RF	(3) IV	(4) OLS	(5) RF	(6) IV
Panel A: RD Treatment Effects							
Child abuse	Schooling	-0.020***	0.007	0.017	-0.026***	0.030	0.027
		(0.003)†††	(0.047)	(0.109)	(0.005)†††	(0.069)	(0.062)
	Mean	0.48	0.48	0.48	0.51	0.51	0.51
	Bandwidth	85	85	85	85	85	85
	Observations	1,776	1,776	1,776	1,095	1,095	1,095
Frequent child abuse	Schooling	-0.020***	0.040	0.096	-0.021***	0.049	0.043
		(0.004)†††	(0.048)	(0.134)	(0.005)†††	(0.075)	(0.069)
	Mean	0.41	0.41	0.41	0.44	0.44	0.44
	Bandwidth	85	85	85	85	85	85
	Observations	1,776	1,776	1,776	1,095	1,095	1,095
Panel B: RD Treatment Effects by Exposure to Childhood Violence							
Child abuse	Schooling	-0.018***	0.039	0.247	-0.025***	0.093	0.099
		(0.004)†††	(0.048)	(0.508)	(0.005)†††	(0.071)	(0.089)
	Schooling × Childhood violence	0.003	-0.081	-0.257	0.002	-0.229**	-0.154**
		(0.011)	(0.085)	(0.431)	(0.018)	(0.106)††	(0.077)††
	Childhood violence	0.186**	0.243***	2.180	0.233*	0.341***	1.347**
		(0.093)††	(0.049)†††	(3.311)	(0.125)†	(0.058)†††	(0.548)††
	Mean	0.48	0.48	0.48	0.51	0.51	0.51
	Bandwidth	85	85	85	85	85	85
	Observations	1,711	1,711	1,711	1,051	1,051	1,051
Frequent child abuse	Schooling	-0.018***	0.072	0.421	-0.020***	0.114	0.121
		(0.004)†††	(0.049)	(0.812)	(0.005)†††	(0.077)	(0.100)
	Schooling × Childhood violence	-0.007	-0.104	-0.407	-0.001	-0.292***	-0.194**
		(0.012)	(0.083)	(0.680)	(0.018)	(0.106)††	(0.082)††
	Childhood violence	0.249**	0.241***	3.322	0.263*	0.363***	1.629***
		(0.106)††	(0.049)†††	(5.236)	(0.135)†	(0.063)†††	(0.589)††
	Mean	0.41	0.41	0.41	0.44	0.44	0.44
	Bandwidth	85	85	85	85	85	85
	Observations	1,711	1,711	1,711	1,051	1,051	1,051

Notes: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. The sample includes women who have children. All columns use a static bandwidth of 85 months, which is the optimal bandwidth estimated for the years of schooling in rural regions of childhood. Panel A reports the RD treatment effects of the reform, and Panel B reports them by exposure to childhood violence, i.e., whether the respondent experienced violence from her own family members during her childhood. Columns 1 reports OLS results using years of schooling as the independent variable. Columns 2 – 3 report reduced-form RD treatment effects and two-stage least-squares RD treatment effects (by using treatment as an instrument for years of schooling) of being born after January 1987 with a linear control function in the month-year of birth on each side of the discontinuity. Columns 1 – 3 report these results for the overall sample, and columns 4 – 6 report results from same specifications for the subsample of respondents whose childhood region of residence was rural. The variables are described in Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively (based on p-values unadjusted for multiple-hypothesis testing). †††, ††, and † denote significance at the 1, 5, and 10 percent levels, respectively (based on p-values adjusted for multiple-hypothesis testing using Simes adjustment).

		Overall sample			Rural sample		
		(1) OLS	(2) RF	(3) IV	(4) OLS	(5) RF	(6) IV
Panel A: RD Treatment Effects by Exposure to Childhood Violence (Overall)							
Child abuse	Schooling	-0.019***	0.026	0.819	-0.023***	0.080	0.094
		(0.004)†††	(0.047)	(7.802)	(0.005)†††	(0.068)	(0.099)
	Schooling × Childhood violence (overall)	-0.010	-0.071	-0.547	-0.006	-0.195**	-0.134*
		(0.009)	(0.078)	(4.718)	(0.014)	(0.096)††	(0.070)†
	Childhood violence (overall)	0.267***	0.220***	4.428	0.292***	0.312***	1.181**
		(0.073)†††	(0.043)†††	(36.698)	(0.096)†††	(0.048)†††	(0.492)††
	Mean	0.49	0.49	0.49	0.51	0.51	0.51
	Bandwidth	94	94	94	89	89	89
	Observations	1,930	1,930	1,930	1,139	1,139	1,139
Frequent child abuse	Schooling	-0.018***	0.072	0.774	-0.020***	0.106	0.133
		(0.003)†††	(0.044)	(2.834)	(0.005)†††	(0.073)	(0.125)
	Schooling × Childhood violence	-0.016*	-0.049	-0.530	-0.008	-0.219**	-0.159**
		(0.009)	(0.075)	(1.902)	(0.014)	(0.096)††	(0.075)†
	Childhood violence	0.278***	0.181***	4.246	0.285***	0.296***	1.334**
		(0.078)†††	(0.040)†††	(14.655)	(0.103)†††	(0.051)†††	(0.538)††
	Mean	0.42	0.42	0.42	0.44	0.44	0.44
	Bandwidth	106	106	106	92	92	92
	Observations	2,129	2,129	2,129	1,163	1,163	1,163
Panel B: RD Treatment Effects by Exposure to Home Violence							
Child abuse	Schooling	-0.018***	-0.005	-0.020	-0.021***	0.039	0.044
		(0.004)†††	(0.047)	(0.201)	(0.005)†††	(0.073)	(0.088)
	Schooling × Home violence	-0.011	0.031	0.059	-0.017**	-0.019	-0.023
		(0.007)	(0.059)	(0.124)	(0.009)†	(0.074)	(0.064)
	Home violence	0.266***	0.176***	-0.264	0.369***	0.247***	0.392
		(0.058)†††	(0.033)†††	(0.936)	(0.066)†††	(0.039)†††	(0.439)
	Mean	0.49	0.49	0.49	0.51	0.51	0.51
	Bandwidth	94	94	94	89	89	89
	Observations	1,878	1,878	1,878	1,102	1,102	1,102
Frequent child abuse	Schooling	-0.019***	0.038	0.104	-0.020***	0.053	0.065
		(0.004)†††	(0.045)	(0.206)	(0.006)	(0.076)	(0.106)
	Schooling × Home violence	-0.008	0.052	0.103	-0.010	0.008	-0.013
		(0.007)	(0.060)	(0.200)	(0.009)†††	(0.078)	(0.071)
	Home violence	0.216***	0.143***	-0.627	0.280***	0.204***	0.287
		(0.061)†††	(0.034)†††	(1.514)	(0.078)†††	(0.043)†††	(0.486)
	Mean	0.42	0.42	0.42	0.44	0.44	0.44
	Bandwidth	106	106	106	92	92	92
	Observations	2,067	2,067	2,067	1,126	1,126	1,126

Notes: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. The sample includes women who have children. The optimal bandwidth is estimated by using the Imbens and Kalyanaraman (2009) algorithm. Panel A reports the RD treatment effects of the reform by exposure to overall childhood violence, i.e., whether the respondent experienced violence from her own family members or others (teachers, strangers, etc.) during her childhood; and Panel B reports them by exposure to home violence, i.e., whether she witnessed violence against her mother during her childhood. Columns 1 reports OLS results using years of schooling as the independent variable for an optimal bandwidth $\hat{h}$ estimated by the Imbens and Kalyanaraman algorithm. Columns 2 – 3 report reduced-form RD treatment effects and two-stage least-squares RD treatment effects (by using treatment as an instrument for years of schooling) of being born after January 1987 with a linear control function in the month-year of birth on each side of the discontinuity. Columns 1 – 3 report these results for the overall sample, and columns 4 – 6 report results from the same specifications for the subsample of respondents whose childhood region of residence was rural. The variables are described in Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively (based on p-values unadjusted for multiple-hypothesis testing). †††, ††, and † denote significance at the 1, 5, and 10 percent levels, respectively (based on p-values adjusted for multiple-hypothesis testing using Simes adjustment).

TABLE A8: EFFECTS OF EDUCATION ON ATTITUDES TOWARD VIOLENCE

		Overall sample			Rural sample		
		(1) OLS	(2) RF	(3) IV	(4) OLS	(5) RF	(6) IV
Panel A: RD Treatment Effects							
Men can beat their partners in certain situations.	Schooling	-0.024***	0.024	0.040	-0.024***	0.023	0.021
		(0.004)†††	(0.057)	(0.101)	(0.005)†††	(0.069)	(0.063)
	Mean	0.38	0.38	0.38	0.41	0.41	0.41
	Bandwidth	83	83	83	88	88	88
	Observations	1,651	1,651	1,651	1,080	1,080	1,080
It may be necessary to beat children for discipline.	Schooling	-0.019***	0.031	0.104	-0.025***	0.022	0.022
		(0.003)†††	(0.041)	(0.185)	(0.004)†††	(0.051)	(0.051)
	Mean	0.28	0.28	0.28	0.29	0.29	0.29
	Bandwidth	107	107	107	99	99	99
	Observations	2,131	2,131	2,131	1,236	1,236	1,236
Panel B: RD Treatment Effects by Exposure to Childhood Violence (Static Bandwidth)							
Men can beat their partners in certain situations.	Schooling	-0.024***	0.051	0.161	-0.023***	0.060	0.056
		(0.004)†††	(0.054)	(0.225)	(0.005)†††	(0.069)	(0.068)
	Schooling × Childhood violence	-0.001	-0.080	-0.188	-0.012	-0.084	-0.067
		(0.011)	(0.091)	(0.213)	(0.013)	(0.113)	(0.068)
	Childhood violence	0.110	0.129***	1.532	0.195*	0.141**	0.587
		(0.088)	(0.047)††	(1.622)	(0.104)	(0.057)††	(0.464)
	Mean	0.38	0.38	0.38	0.41	0.41	0.41
	Bandwidth	85	85	85	85	85	85
	Observations	1,625	1,625	1,625	998	998	998
It may be necessary to beat children for discipline.	Schooling	-0.018***	0.045	0.175	-0.024***	0.054	0.051
		(0.004)†††	(0.046)	(0.381)	(0.004)†††	(0.051)	(0.057)
	Schooling × Childhood violence	0.001	0.027	-0.110	-0.005	0.011	-0.017
		(0.011)	(0.070)	(0.339)	(0.014)	(0.091)	(0.056)
	Childhood violence	0.042	0.048	0.918	0.105	0.084	0.219
		(0.094)	(0.045)	(2.602)	(0.116)	(0.060)	(0.400)
	Mean	0.29	0.29	0.29	0.30	0.30	0.30
	Bandwidth	85	85	85	85	85	85
	Observations	1,712	1,712	1,712	1,052	1,052	1,052

Notes: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. The sample includes women who have children. Panel A reports the RD treatment effects of the reform, and Panel B reports them by exposure to childhood violence, i.e., whether the respondent experienced violence from her own family members during her childhood. Panel A uses an optimal bandwidth $\hat{h}$ estimated by the Imbens and Kalyanaraman algorithm, and Panel B uses a static bandwidth of 85 months, which is the optimal bandwidth estimated for the years of schooling in rural regions of childhood. Columns 1 reports OLS results using years of schooling as the independent variable. Columns 2 – 3 report reduced-form RD treatment effects and two-stage least-squares RD treatment effects (by using treatment as an instrument for years of schooling) of being born after January 1987 with a linear control function in the month-year of birth on each side of the discontinuity. Columns 1 – 3 report these results for the overall sample, and columns 4 – 6 report results from same specifications for the subsample of respondents whose childhood region of residence was rural. The variables are described in Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively (based on p-values unadjusted for multiple-hypothesis testing). †††, ††, and † denote significance at the 1, 5, and 10 percent levels, respectively (based on p-values adjusted for multiple-hypothesis testing using Simes adjustment).

TABLE A9: EFFECTS OF EDUCATION ON MENTAL HEALTH OUTCOMES

		Overall sample			Rural sample		
		(1) OLS	(2) RF	(3) IV	(4) OLS	(5) RF	(6) IV
Panel A: RD Treatment Effects							
Overall depression index	Schooling	-0.024***	-0.037	-0.073	-0.023***	-0.060	-0.062
		(0.005)†††	(0.046)	(0.106)	(0.005)†††	(0.065)	(0.071)
	Mean	0.02	0.02	0.02	0.03	0.03	0.03
	Bandwidth	77	77	77	115	115	115
	Observations	1,644	1,644	1,644	1,412	1,412	1,412
Somatic depression index	Schooling	-0.035***	-0.019	-0.056	-0.033***	-0.028	-0.031
		(0.005)†††	(0.055)	(0.162)	(0.006)†††	(0.071)	(0.075)
	Mean	0.00	0.00	0.00	0.03	0.03	0.03
	Bandwidth	106	106	106	96	96	96
	Observations	2,122	2,122	2,122	1,212	1,212	1,212
Nonsomatic depression index	Schooling	-0.023***	-0.037	-0.063	-0.020***	-0.032	-0.036
		(0.005)†††	(0.050)	(0.096)	(0.005)†††	(0.076)	(0.085)
	Mean	0.03	0.03	0.03	0.04	0.04	0.04
	Bandwidth	75	75	75	95	95	95
	Observations	1,574	1,574	1,574	1,201	1,201	1,201
Panel B: RD Treatment Effects by Exposure to Childhood Violence (Static Bandwidth)							
Overall depression index	Schooling	-0.023***	0.015	0.165	-0.021***	0.033	0.043
		(0.004)†††	(0.047)	(0.351)	(0.005)†††	(0.075)	(0.080)
	Schooling × Childhood violence	0.006	-0.140	-0.254	-0.007	-0.285**	-0.159**
		(0.011)	(0.093)	(0.335)	(0.017)	(0.123)†	(0.080)†
	Childhood violence	0.282***	0.375***	2.242	0.323**	0.379***	1.382**
		(0.094)†††	(0.045)†††	(2.558)	(0.125)††	(0.066)†††	(0.574)†
	Mean	0.02	0.02	0.02	0.04	0.04	0.04
	Bandwidth	85	85	85	85	85	85
	Observations	1,718	1,718	1,718	1,056	1,056	1,056
Somatic depression index	Schooling	-0.033***	0.036	0.353	-0.030***	0.056	0.065
		(0.006)†††	(0.061)	(0.679)	(0.007)†††	(0.079)	(0.091)
	Schooling × Childhood violence	0.021	-0.275**	-0.521	0.006	-0.292*	-0.172*
		(0.015)	(0.114)†	(0.622)	(0.026)	(0.151)†	(0.104)†
	Childhood violence	0.127	0.380***	4.216	0.142	0.303***	1.400*
		(0.117)	(0.061)†††	(4.775)	(0.177)	(0.088)†††	(0.751)††
	Mean	0.00	0.00	0.00	0.02	0.02	0.02
	Bandwidth	85	85	85	85	85	85
	Observations	1,718	1,718	1,718	1,056	1,056	1,056
Nonsomatic depression index	Schooling	-0.021***	0.010	0.119	-0.019***	0.027	0.038
		(0.005)†††	(0.050)	(0.299)	(0.006)†††	(0.080)	(0.083)
	Schooling × Childhood violence	0.002	-0.106	-0.188	-0.011	-0.284**	-0.155*
		(0.012)	(0.099)	(0.295)	(0.018)	(0.134)†	(0.083)†
	Childhood violence	0.321***	0.374***	1.752	0.368***	0.398***	1.379**
		(0.103)†††	(0.049)†††	(2.247)	(0.132)††	(0.070)†††	(0.594)††
	Mean	0.02	0.02	0.02	0.04	0.04	0.04
	Bandwidth	85	85	85	85	85	85
	Observations	1,718	1,718	1,718	1,056	1,056	1,056

Notes: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. The sample includes all women. Panel A reports the RD treatment effects of the reform, and Panel B reports them by exposure to childhood violence, i.e., whether the respondent experienced violence from her own family members during her childhood. Panel A uses an optimal bandwidth $\hat{h}$ estimated by the Imbens and Kalyanaraman algorithm, and Panel B uses a static bandwidth of 85 months, which is the optimal bandwidth estimated for the years of schooling in rural regions of childhood. Columns 1 reports OLS results using years of schooling as the independent variable. Columns 2 – 3 report reduced-form RD treatment effects and two-stage least-squares RD treatment effects (by using treatment as an instrument for years of schooling) of being born after January 1987 with a linear control function in the month-year of birth on each side of the discontinuity. Columns 1 – 3 report these results for the overall sample, and columns 4 – 6 report results from same specifications for the subsample of respondents whose childhood region of residence was rural. The variables are described in Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively (based on p-values unadjusted for multiple-hypothesis testing). †††, ††, and † denote significance at the 1, 5, and 10 percent levels, respectively (based on p-values adjusted for multiple-hypothesis testing using Simes adjustment).

TABLE A10: EFFECTS OF EDUCATION ON FERTILITY OUTCOMES

		Overall sample			Rural sample		
		(1) OLS	(2) RF	(3) IV	(4) OLS	(5) RF	(6) IV
Panel A: RD Treatment Effects							
Age at first pregnancy	Schooling	0.441***	0.183	0.442	0.357***	0.723**	0.703*
		(0.025)†††	(0.253)	(0.600)	(0.035)†††	(0.335)†	(0.373)
	Mean	21.48	21.48	21.48	21.14	21.14	21.14
	Bandwidth	121	121	121	106	106	106
	Observations	2,481	2,481	2,481	1,388	1,388	1,388
Number of children	Schooling	-0.144***	-0.113	-0.137	-0.147***	-0.149	-0.137
		(0.007)	(0.100)†††	(0.112)	(0.009)†††	(0.108)	(0.099)
	Mean	1.51	1.51	1.51	1.69	1.69	1.69
	Bandwidth	73	73	73	88	88	88
	Observations	2,056	2,056	2,056	1,445	1,445	1,445
Panel B: RD Treatment Effects by Exposure to Childhood Violence (Static Bandwidth)							
Age at first pregnancy	Schooling	0.418***	0.364	0.443	0.359***	0.855**	0.770*
		(0.028)†††	(0.290)	(1.235)	(0.038)†††	(0.398)†	(0.437)
	Schooling × Childhood violence	0.029	0.911*	0.606	-0.019	1.107*	0.177
		(0.077)	(0.499)	(1.498)	(0.086)	(0.621)	(0.435)
	Childhood violence	-0.496	-0.519	-4.789	0.086	-0.179	-0.900
		(0.582)	(0.392)	(11.657)	(0.613)	(0.476)	(3.209)
	Mean	21.34	21.34	21.34	21.08	21.08	21.08
	Bandwidth	85	85	85	85	85	85
	Observations	1,801	1,801	1,801	1,096	1,096	1,096
Number of children	Schooling	-0.141***	-0.183*	-0.229*	-0.142***	-0.171	-0.169
		(0.007)†††	(0.096)	(0.131)	(0.010)†††	(0.119)	(0.121)
	Schooling × Childhood violence	-0.029*	-0.141	-0.007	-0.030	-0.104	0.025
		(0.015)	(0.136)	(0.072)	(0.024)	(0.207)	(0.078)
	Childhood violence	0.137	0.022	-0.032	0.096	-0.025	-0.286
		(0.160)	(0.098)	(0.635)	(0.219)	(0.129)	(0.623)
	Mean	1.50	1.50	1.50	1.70	1.70	1.70
	Bandwidth	85	85	85	85	85	85
	Observations	2,274	2,274	2,274	1,322	1,322	1,322

Notes: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. The sample includes all women. Panel A reports the RD treatment effects of the reform, and Panel B reports them by exposure to childhood violence, i.e., whether the respondent experienced violence from her own family members during her childhood. Panel A uses an optimal bandwidth $\hat{h}$ estimated by the Imbens and Kalyanaraman algorithm, and Panel B uses a static bandwidth of 85 months, which is the optimal bandwidth estimated for the years of schooling in rural regions of childhood. Columns 1 reports OLS results using years of schooling as the independent variable. Columns 2 – 3 report reduced-form RD treatment effects and two-stage least-squares RD treatment effects (by using treatment as an instrument for years of schooling) of being born after January 1987 with a linear control function in the month-year of birth on each side of the discontinuity. Columns 1 – 3 report these results for the overall sample, and columns 4 – 6 report results from same specifications for the subsample of respondents whose childhood region of residence was rural. The variables are described in Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively (based on p-values unadjusted for multiple-hypothesis testing). †††, ††, and † denote significance at the 1, 5, and 10 percent levels, respectively (based on p-values adjusted for multiple-hypothesis testing using Simes adjustment).

TABLE A11: EFFECTS OF EDUCATION ON LABOR MARKET OUTCOMES

		Overall sample			Rural sample		
		(1) OLS	(2) RF	(3) IV	(4) OLS	(5) RF	(6) IV
Panel A: RD Treatment Effects							
Employed	Schooling	0.018*** (0.003)†††	0.002 (0.031)	0.009 (0.141)	0.012*** (0.004)†††	-0.010 (0.036)	-0.010 (0.038)
	Mean	0.19	0.19	0.19	0.18	0.18	0.18
	Bandwidth	96	96	96	93	93	93
	Observations	1,960	1,960	1,960	1,184	1,184	1,184
Employed in services	Schooling	0.023*** (0.003)†††	-0.015 (0.028)	-0.063 (0.161)	0.020*** (0.004)†††	-0.024 (0.030)	-0.023 (0.032)
	Mean	0.14	0.14	0.14	0.11	0.11	0.11
	Bandwidth	94	94	94	88	88	88
	Observations	1,940	1,940	1,940	1,141	1,141	1,141
Social security	Schooling	0.023*** (0.003)†††	0.008 (0.024)	0.018 (0.052)	0.020*** (0.004)†††	-0.011 (0.027)	-0.010 (0.026)
	Mean	0.11	0.11	0.11	0.10	0.10	0.10
	Bandwidth	81	81	81	97	97	97
	Observations	1,723	1,723	1,723	1,232	1,232	1,232
Personal income index	Schooling	0.023*** (0.003)†††	-0.050 (0.038)	-0.198 (0.308)	0.016*** (0.005)†††	-0.061 (0.054)	-0.061 (0.065)
	Mean	-0.05	-0.05	-0.05	-0.10	-0.10	-0.10
	Bandwidth	143	143	143	89	89	89
	Observations	2,633	2,633	2,633	1,145	1,145	1,145
Asset ownership index	Schooling	0.029*** (0.002)	-0.004 (0.021)	-0.009 (0.053)	0.030*** (0.002)	0.028 (0.027)	0.024 (0.021)
	Mean	0.03	0.03	0.03	0.00	0.00	0.00
	Bandwidth	84	84	84	87	87	87
	Observations	1,762	1,762	1,762	1,120	1,120	1,120
Panel B: RD Treatment Effects by Exposure to Childhood Violence (Static Bandwidth)							
Employed	Schooling	0.022*** (0.004)†††	0.025 (0.034)	0.127 (0.234)	0.015*** (0.004)†††	0.021 (0.039)	0.022 (0.038)
	Schooling × Childhood violence	-0.002 (0.009)	-0.033 (0.067)	-0.127 (0.219)	-0.006 (0.013)	-0.041 (0.081)	-0.030 (0.049)
	Childhood violence	0.055 (0.067)	0.048 (0.043)	1.007 (1.684)	0.093 (0.097)	0.073 (0.056)	0.270 (0.357)
	Mean	0.19	0.19	0.19	0.18	0.18	0.18
	Bandwidth	85	85	85	85	85	85
	Observations	1,718	1,718	1,718	1,056	1,056	1,056

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TABLE A11: EFFECTS OF EDUCATION ON LABOR MARKET OUTCOMES, CONT'D

		Overall sample			Rural sample		
		(1) OLS	(2) RF	(3) IV	(4) OLS	(5) RF	(6) IV
Employed in services	Schooling	0.026***	0.015	0.132	0.022***	-0.001	0.002
		(0.004)†††	(0.028)	(0.236)	(0.004)†††	(0.032)	(0.031)
	Schooling × Childhood violence	-0.003	-0.092*	-0.184	-0.007	-0.071	-0.036
		(0.009)	(0.055)	(0.215)	(0.014)	(0.061)	(0.040)
	Childhood violence	0.050	0.055	1.417	0.077	0.056	0.277
		(0.059)	(0.041)	(1.658)	(0.094)	(0.048)	(0.299)
	Mean	0.14	0.14	0.14	0.11	0.11	0.11
	Bandwidth	85	85	85	85	85	85
	Observations	1,718	1,718	1,718	1,056	1,056	1,056
Social security	Schooling	0.025***	0.020	0.117	0.019***	-0.009	-0.006
		(0.003)†††	(0.026)	(0.222)	(0.004)†††	(0.030)	(0.030)
	Schooling × Childhood violence	-0.006	-0.045	-0.130	-0.004	-0.049	-0.021
		(0.008)	(0.043)	(0.202)	(0.012)	(0.054)	(0.036)
	Childhood violence	0.047	0.015	0.988	0.029	0.021	0.148
		(0.053)	(0.035)	(1.559)	(0.071)	(0.048)	(0.281)
	Mean	0.11	0.11	0.11	0.09	0.09	0.09
	Bandwidth	85	85	85	85	85	85
	Observations	1,718	1,718	1,718	1,056	1,056	1,056
Personal income index	Schooling	0.021***	-0.060	-0.198	0.014***	-0.059	-0.053
		(0.004)†††	(0.044)	(0.426)	(0.005)†††	(0.061)	(0.070)
	Schooling × Childhood violence	0.009	-0.061	0.094	0.026	-0.082	-0.017
		(0.017)	(0.065)	(0.376)	(0.033)	(0.095)	(0.073)
	Childhood violence	-0.091	-0.008	-0.778	-0.210	-0.011	0.064
		(0.112)	(0.052)	(2.895)	(0.192)	(0.079)	(0.556)
	Mean	-0.08	-0.08	-0.08	-0.10	-0.10	-0.10
	Bandwidth	85	85	85	85	85	85
	Observations	1,718	1,718	1,718	1,056	1,056	1,056
Asset ownership index	Schooling	0.030***	-0.010	-0.086	0.030***	0.016	0.013
		(0.002)†††	(0.021)	(0.203)	(0.002)†††	(0.028)	(0.025)
	Schooling × Childhood violence	-0.013**	0.059	0.120	-0.005	0.050	0.019
		(0.006)††	(0.038)	(0.183)	(0.008)	(0.048)	(0.027)
	Childhood violence	0.039	-0.077***	-0.966	-0.018	-0.069**	-0.177
		(0.039)	(0.022) †††	(1.407)	(0.054)	(0.031)	(0.195)
	Mean	0.03	0.03	0.03	-0.01	-0.01	-0.01
	Bandwidth	85	85	85	85	85	85
	Observations	1,718	1,718	1,718	1,056	1,056	1,056

Notes: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. The sample includes all women. Panel A reports the RD treatment effects of the reform, and Panel B reports them by exposure to childhood violence, i.e., whether the respondent experienced violence from her own family members during her childhood. Panel A uses an optimal bandwidth $\hat{h}$ estimated by the Imbens and Kalyanaraman algorithm, and Panel B uses a static bandwidth of 85 months, which is the optimal bandwidth estimated for the years of schooling in rural regions of childhood. Columns 1 reports OLS results using years of schooling as the independent variable. Columns 2 – 3 report reduced-form RD treatment effects and two-stage least-squares RD treatment effects (by using treatment as an instrument for years of schooling) of being born after January 1987 with a linear control function in the month-year of birth on each side of the discontinuity. Columns 1 – 3 report these results for the overall sample, and columns 4 – 6 report results from same specifications for the subsample of respondents whose childhood region of residence was rural. The variables are described in Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively (based on p-values unadjusted for multiple-hypothesis testing). †††, ††, and † denote significance at the 1, 5, and 10 percent levels, respectively (based on p-values adjusted for multiple-hypothesis testing using Simes adjustment).

TABLE A12: EFFECTS OF EDUCATION ON PARTNER CHARACTERISTICS AND MARRIAGE MARKET OUTCOMES

		Overall sample			Rural sample		
		(1) OLS	(2) RF	(3) IV	(4) OLS	(5) RF	(6) IV
Panel A: RD Treatment Effects							
Partner's years of schooling	Schooling	0.517***	0.571*	1.613	0.496***	0.994**	0.966*
		(0.027)†††	(0.338)	(1.572)	(0.031)†††	(0.446)†	(0.508)
	Mean	8.80	8.80	8.80	8.47	8.47	8.47
	Bandwidth	87	87	87	79	79	79
	Observations	1,816	1,816	1,816	1,020	1,020	1,020
Partner's age	Schooling	0.288***	0.227	0.737	0.287***	0.211	0.213
		(0.035)†††	(0.400)	(1.423)	(0.039)†††	(0.530)	(0.518)
	Mean	24.89	24.89	24.89	24.71	24.71	24.71
	Bandwidth	106	106	106	111	111	111
	Observations	2,136	2,136	2,136	1,374	1,374	1,374
Partner's religiosity index	Schooling	0.006	0.005	0.010	-0.004	0.007	0.006
		(0.006)	(0.078)	(0.169)	(0.005)	(0.065)	(0.061)
	Mean	0.01	0.01	0.01	0.06	0.06	0.06
	Bandwidth	84	84	84	82	82	82
	Observations	1,784	1,784	1,784	1,066	1,066	1,066
Marriage age	Schooling	0.307***	0.048	0.271	0.270***	-0.245	-0.866
		(0.032)†††	(0.405)	(2.058)	(0.038)†††	(0.532)	(3.044)
	Mean	20.94	20.94	20.94	20.74	20.74	20.74
	Bandwidth	37	37	37	40	40	40
	Observations	849	849	849	557	557	557
Marriage decision	Schooling	0.041***	0.150***	0.299	0.043***	0.190***	0.167**
		(0.004)†††	(0.050)††	(0.228)	(0.004)†††	(0.055)†††	(0.082)
	Mean	0.58	0.58	0.58	0.53	0.53	0.53
	Bandwidth	66	66	66	87	87	87
	Observations	1,427	1,427	1,427	1,120	1,120	1,120
Divorced	Schooling	-0.001	-0.018	-0.031	-0.001	0.006	0.006
		(0.002)	(0.021)	(0.039)	(0.002)	(0.023)	(0.020)
	Mean	0.05	0.05	0.05	0.05	0.05	0.05
	Bandwidth	71	71	71	122	122	122
	Observations	1,496	1,496	1,496	1,467	1,467	1,467
Panel B: RD Treatment Effects by Exposure to Childhood Violence (Static Bandwidth)							
Partner's years of schooling	Schooling	0.513***	0.588*	2.242	0.504***	1.135***	1.190*
		(0.029)†††	(0.354)	(4.346)	(0.035)†††	(0.429)††	(0.628)
	Schooling × Childhood violence	-0.047	1.052*	-0.964	-0.094	0.806	-0.284
		(0.067)	(0.540)	(3.915)	(0.104)	(0.731)	(0.533)
	Childhood violence	-0.016	-0.848***	7.334	0.299	-0.756*	1.871
		(0.548)	(0.325)††	(30.142)	(0.835)	(0.427)	(3.802)
	Mean	8.81	8.81	8.81	8.43	8.43	8.43
	Bandwidth	85	85	85	85	85	85
	Observations	1,703	1,703	1,703	1,043	1,043	1,043

		Overall sample			Rural sample		
		(1) OLS	(2) RF	(3) IV	(4) OLS	(5) RF	(6) IV
Partner's age	Schooling	0.299***	0.170	-0.366	0.283***	0.427	0.353
		(0.036)†††	(0.439)	(2.244)	(0.048)†††	(0.598)	(0.555)
	Schooling × Childhood violence	-0.193	1.412	1.607	0.084	1.350	0.512
		(0.155)	(0.886)	(2.428)	(0.134)	(0.876)	(0.561)
	Childhood violence	2.013	0.140	-11.335	-0.219	0.003	-2.984
		(1.282)	(0.408)	(18.336)	(0.948)	(0.541)	(3.966)
	Mean	24.81	24.81	24.81	24.60	24.60	24.60
	Bandwidth	85	85	85	85	85	85
	Observations	1,715	1,715	1,715	1,054	1,054	1,054
Partner's religiosity index	Schooling	0.004	0.001	0.023	0.004	-0.024	-0.031
		(0.005)	(0.068)	(0.292)	(0.006)	(0.068)	(0.067)
	Schooling × Childhood violence	0.023	-0.028	-0.043	-0.023	0.187	0.105
		(0.032)	(0.169)	(0.303)	(0.030)	(0.160)	(0.097)
	Childhood violence	-0.457*	-0.282***	0.033	-0.038	-0.265**	-0.930
		(0.265)	(0.106)††	(2.323)	(0.228)	(0.124)	(0.723)
	Mean	0.01	0.01	0.01	0.06	0.06	0.06
	Bandwidth	85	85	85	85	85	85
	Observations	1,718	1,718	1,718	1,056	1,056	1,056
Marriage age	Schooling	0.354***	0.313	0.580	0.282***	0.489	0.421
		(0.025)†††	(0.310)	(1.394)	(0.033)†††	(0.416)	(0.388)
	Schooling × Childhood violence	-0.081	0.888**	0.376	0.008	1.001*	0.303
		(0.083)	(0.439)	(1.381)	(0.113)	(0.587)	(0.366)
	Childhood violence	0.311	-0.563*	-2.866	-0.079	-0.160	-1.801
		(0.665)	(0.330)	(10.561)	(0.841)	(0.436)	(2.652)
	Mean	21.28	21.28	21.28	21.09	21.09	21.09
	Bandwidth	85	85	85	85	85	85
	Observations	1,715	1,715	1,715	1,054	1,054	1,054
Marriage decision	Schooling	0.036***	0.118***	0.409	0.037***	0.163***	0.144
		(0.004)†††	(0.044)†	(0.682)	(0.005)†††	(0.059)††	(0.088)
	Schooling × Childhood violence	0.029***	0.094	-0.225	0.042***	0.271***	0.069
		(0.010)††	(0.075)	(0.585)	(0.011)†††	(0.086)††	(0.076)
	Childhood violence	-0.247***	-0.075*	1.726	-0.299***	-0.106*	-0.450
		(0.087)††	(0.044)	(4.508)	(0.092)†††	(0.061)	(0.540)
	Mean	0.57	0.57	0.57	0.52	0.52	0.52
	Bandwidth	85	85	85	85	85	85
	Observations	1,718	1,718	1,718	1,056	1,056	1,056
Divorced	Schooling	-0.002	-0.008	-0.031	-0.002	0.010	0.010
		(0.002)	(0.021)	(0.110)	(0.002)	(0.029)	(0.028)
	Schooling × Childhood violence	0.004	-0.003	0.021	-0.004	-0.010	-0.009
		(0.008)	(0.042)	(0.110)	(0.007)	(0.044)	(0.028)
	Childhood violence	0.010	0.043	-0.120	0.042	0.019	0.080
		(0.060)	(0.031)	(0.844)	(0.062)	(0.036)	(0.211)
	Mean	0.06	0.06	0.06	0.05	0.05	0.05
	Bandwidth	85	85	85	85	85	85
	Observations	1,718	1,718	1,718	1,056	1,056	1,056

Notes: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. The sample includes all women. Panel A reports the RD treatment effects of the reform, and Panel B reports them by exposure to childhood violence, i.e., whether the respondent experienced violence from her own family members during her childhood. Panel A uses an optimal bandwidth $\hat{h}$ estimated by the Imbens and Kalyanaraman algorithm, and Panel B uses a static bandwidth of 85 months, which is the optimal bandwidth estimated for the years of schooling in rural regions of childhood. Columns 1 reports OLS results using years of schooling as the independent variable. Columns 2 – 3 report reduced-form RD treatment effects and two-stage least-squares RD treatment effects (by using treatment as an instrument for years of schooling) of being born after January 1987 with a linear control function in the month-year of birth on each side of the discontinuity. Columns 1 – 3 report these results for the overall sample, and columns 4 – 6 report results from same specifications for the subsample of respondents whose childhood region of residence was rural. The variables are described in Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively (based on p-values unadjusted for multiple-hypothesis testing). †††, ††, and † denote significance at the 1, 5, and 10 percent levels, respectively (based on p-values adjusted for multiple-hypothesis testing using Simes adjustment).

TABLE A13: EFFECTS OF EDUCATION ON SPOUSAL VIOLENCE

		Overall sample			Rural sample		
		(1) OLS	(2) RF	(3) IV	(4) OLS	(5) RF	(6) IV
Panel A: RD Treatment Effects							
Physical violence index	Schooling	-0.026***	-0.163**	-0.382	-0.026***	-0.119	-0.121
		(0.006)†††	(0.081)	(0.365)	(0.007)†††	(0.089)	(0.101)
	Mean	0.00	0.00	0.00	0.01	0.01	0.01
	Bandwidth	83	83	83	108	108	108
	Observations	1,747	1,747	1,747	1,362	1,362	1,362
Psychological violence index	Schooling	-0.022***	-0.014	-0.049	-0.027***	-0.045	-0.039
		(0.004)†††	(0.058)	(0.209)	(0.006)†††	(0.082)	(0.070)
	Mean	0.04	0.04	0.04	0.06	0.06	0.06
	Bandwidth	115	115	115	79	79	79
	Observations	2,260	2,260	2,260	1,020	1,020	1,020
Financial control index	Schooling	-0.015***	0.031	0.069	-0.016**	0.062	0.051
		(0.005)†††	(0.074)	(0.172)	(0.007)††	(0.103)	(0.087)
	Mean	-0.04	-0.04	-0.04	-0.04	-0.04	-0.04
	Bandwidth	83	83	83	74	74	74
	Observations	1,740	1,740	1,740	963	963	963
Panel B: RD Treatment Effects by Exposure to Childhood Violence (Static Bandwidth)							
Physical violence index	Schooling	-0.016***	-0.121	-0.376	-0.019**	-0.049	-0.031
		(0.006)†††	(0.079)	(0.737)	(0.008)††	(0.098)	(0.096)
	Schooling × Childhood violence	-0.052**	-0.153	0.145	-0.045	-0.373*	-0.168
		(0.024)†	(0.185)	(0.699)	(0.029)	(0.217)	(0.136)
	Childhood violence	0.951***	0.618***	-0.608	0.794***	0.613***	1.635*
		(0.196)†††	(0.111)†††	(5.347)	(0.259)†††	(0.156)†††	(0.988)
	Mean	0.00	0.00	0.00	0.00	0.00	0.00
	Bandwidth	85	85	85	85	85	85
	Observations	1,718	1,718	1,718	1,056	1,056	1,056
Psychological violence index	Schooling	-0.018***	0.027	0.135	-0.024***	0.042	0.045
		(0.004)†††	(0.065)	(0.363)	(0.006)†††	(0.079)	(0.082)
	Schooling × Childhood violence	0.002	-0.033	-0.132	0.001	-0.124	-0.080
		(0.018)	(0.116)	(0.356)	(0.025)	(0.127)	(0.079)
	Childhood violence	0.349***	0.381***	1.381	0.264	0.323***	0.845
		(0.130)†††	(0.057)†††	(2.708)	(0.171)	(0.073)†††	(0.564)
	Mean	0.05	0.05	0.05	0.05	0.05	0.05
	Bandwidth	85	85	85	85	85	85
	Observations	1,718	1,718	1,718	1,056	1,056	1,056
Financial control index	Schooling	-0.009*	0.068	0.341	-0.013*	0.083	0.086
		(0.005)†	(0.071)	(0.570)	(0.007)†	(0.099)	(0.104)
	Schooling × Childhood violence	-0.023	-0.120	-0.368	-0.062**	-0.167	-0.120
		(0.029)	(0.163)	(0.525)	(0.028)†	(0.161)	(0.102)
	Childhood violence	0.439*	0.316***	3.077	0.530**	0.186	0.976
		(0.231)†	(0.095)†††	(4.028)	(0.265)†	(0.132)	(0.769)
	Mean	-0.04	-0.04	-0.04	-0.05	-0.05	-0.05
	Bandwidth	85	85	85	85	85	85
	Observations	1,711	1,711	1,711	1,051	1,051	1,051

Notes: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. The sample includes all women. Panel A reports the RD treatment effects of the reform, and Panel B reports them by exposure to childhood violence, i.e., whether the respondent experienced violence from her own family members during her childhood. Panel A uses an optimal bandwidth $\hat{h}$ estimated by the Imbens and Kalyanaraman algorithm, and Panel B uses a static bandwidth of 85 months, which is the optimal bandwidth estimated for the years of schooling in rural regions of childhood. Columns 1 reports OLS results using years of schooling as the independent variable. Columns 2 – 3 report reduced-form RD treatment effects and two-stage least-squares RD treatment effects (by using treatment as an instrument for years of schooling) of being born after January 1987 with a linear control function in the month-year of birth on each side of the discontinuity. Columns 1 – 3 report these results for the overall sample, and columns 4 – 6 report results from same specifications for the subsample of respondents whose childhood region of residence was rural. The variables are described in Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively (based on p-values unadjusted for multiple-hypothesis testing). †††, ††, and † denote significance at the 1, 5, and 10 percent levels, respectively (based on p-values adjusted for multiple-hypothesis testing using Simes adjustment).

TABLE A14: RD TREATMENT EFFECTS IN RURAL CHILDHOOD REGIONS WITH DIFFERENT OPTIMAL BANDWIDTH SELECTION METHODS

		CCT			IK		
		(1) OLS	(2) RF	(3) IV	(4) OLS	(5) RF	(6) IV
Years of schooling	Schooling		1.172** (0.464)			1.134** (0.451)	
	Mean		7.45			7.42	
	Bandwidth		81			85	
	Observations		1,316			1,396	
Child abuse	Schooling	-0.021*** (0.005)	0.069 (0.061)	0.073 (0.068)	-0.023*** (0.005)	0.083 (0.068)	0.097 (0.096)
	Schooling × Childhood violence	0.007 (0.014)	-0.157* (0.095)	-0.113* (0.066)	0.002 (0.018)	-0.224** (0.103)	-0.148** (0.074)
	Childhood violence	0.165* (0.095)	0.267*** (0.045)	1.008** (0.454)	0.238* (0.123)	0.341*** (0.056)	1.308** (0.531)
	Mean	0.51	0.51	0.51	0.51	0.51	0.51
	Bandwidth	158	158	158	89	89	89
	Observations	1,703	1,703	1,703	1,096	1,096	1,096
Frequent child abuse	Schooling	-0.019*** (0.007)	0.111 (0.087)	0.164 (0.220)	-0.020*** (0.005)	0.108 (0.073)	0.134 (0.122)
	Schooling × Childhood violence	0.034 (0.021)	-0.225* (0.120)	-0.174 (0.147)	-0.001 (0.018)	-0.268** (0.103)	-0.185** (0.084)
	Childhood violence	0.113 (0.163)	0.427*** (0.065)	1.590 (1.066)	0.250* (0.134)	0.344*** (0.062)	1.557** (0.613)
	Mean	0.45	0.45	0.45	0.44	0.44	0.44
	Bandwidth	51	51	51	92	92	92
	Observations	640	640	640	1,119	1,119	1,119
Men can beat their partners in certain situations.	Schooling	-0.012 (0.008)	0.049 (0.152)	-1.958 (76.347)	-0.023*** (0.005)	0.053 (0.068)	0.052 (0.070)
	Schooling × Childhood violence	0.019 (0.032)	0.024 (0.184)	0.980 (37.465)	-0.010 (0.013)	-0.067 (0.110)	-0.056 (0.065)
	Childhood violence	-0.033 (0.277)	0.094 (0.133)	-6.871 (266.660)	0.169 (0.103)	0.120** (0.056)	0.490 (0.446)
	Mean	0.43	0.43	0.43	0.41	0.41	0.41
	Bandwidth	24	24	24	88	88	88
	Observations	284	284	284	1,039	1,039	1,039
It may be necessary to beat children for discipline.	Schooling	-0.024*** (0.004)	0.039 (0.051)	0.041 (0.061)	-0.024*** (0.004)	0.025 (0.049)	0.027 (0.055)
	Schooling × Childhood violence	-0.004 (0.014)	0.019 (0.088)	-0.008 (0.053)	-0.008 (0.013)	0.009 (0.084)	-0.009 (0.048)
	Childhood violence	0.098 (0.114)	0.076 (0.058)	0.147 (0.383)	0.119 (0.104)	0.075 (0.052)	0.147 (0.344)
	Mean	0.30	0.30	0.30	0.29	0.29	0.29
	Bandwidth	90	90	90	99	99	99
	Observations	1,097	1,097	1,097	1,189	1,189	1,189
Age at first pregnancy	Schooling	0.338*** (0.041)	0.448 (0.473)	0.535 (0.586)	0.360*** (0.037)	0.650* (0.359)	0.672 (0.437)
	Schooling × Childhood violence	-0.067 (0.092)	1.128 (0.680)	0.100 (0.586)	-0.095 (0.083)	0.871 (0.547)	0.161 (0.401)
	Childhood violence	0.798 (0.588)	0.076 (0.439)	-0.314 (4.216)	0.767 (0.599)	0.036 (0.411)	-0.778 (2.922)
	Mean	20.76	20.76	20.76	21.14	21.14	21.14
	Bandwidth	55	55	55	106	106	106
	Observations	712	712	712	1,336	1,336	1,336

		CCT			IK		
		(1) OLS	(2) RF	(3) IV	(4) OLS	(5) RF	(6) IV
Number of children	Schooling	-0.135***	-0.095	-0.077	-0.146***	-0.135	-0.139
		(0.011)	(0.129)	(0.113)	(0.010)	(0.113)	(0.120)
	Schooling × Childhood violence	-0.039	-0.184	-0.026	-0.014	-0.109	0.006
		(0.026)	(0.224)	(0.099)	(0.026)	(0.198)	(0.077)
	Childhood violence	0.168	-0.025	0.104	-0.059	-0.045	-0.164
		(0.240)	(0.145)	(0.792)	(0.242)	(0.125)	(0.616)
	Mean	1.71	1.71	1.71	1.69	1.69	1.69
	Bandwidth	68	68	68	88	88	88
	Observations	1,055	1,055	1,055	1,382	1,382	1,382
Employed	Schooling	0.013**	-0.017	1.384	0.014***	0.027	0.034
		(0.005)	(0.059)	(171.338)	(0.004)	(0.037)	(0.046)
	Schooling × Childhood violence	-0.014	-0.164	-0.532	-0.003	-0.061	-0.044
		(0.013)	(0.114)	(55.249)	(0.014)	(0.079)	(0.049)
	Childhood violence	0.184**	0.135*	4.192	0.087	0.088	0.378
		(0.091)	(0.071)	(436.585)	(0.100)	(0.055)	(0.360)
	Mean	0.12	0.12	0.12	0.18	0.18	0.18
	Bandwidth	36	36	36	93	93	93
	Observations	482	482	482	1,139	1,139	1,139
Personal income index	Schooling	0.015**	-0.033	-0.027	0.013***	-0.052	-0.053
		(0.006)	(0.068)	(0.073)	(0.005)	(0.058)	(0.074)
	Schooling × Childhood violence	0.034	-0.070	-0.018	0.026	-0.069	-0.011
		(0.039)	(0.107)	(0.087)	(0.033)	(0.091)	(0.069)
	Childhood violence	-0.269	-0.017	0.078	-0.218	-0.020	0.019
		(0.226)	(0.090)	(0.666)	(0.190)	(0.077)	(0.528)
	Mean	-0.08	-0.08	-0.08	-0.10	-0.10	-0.10
	Bandwidth	69	69	69	89	89	89
	Observations	877	877	877	1,101	1,101	1,101
Partner's years of schooling	Schooling	0.500***	0.948	2.886	0.502***	1.043**	1.149*
		(0.043)	(0.603)	(5.023)	(0.034)	(0.440)	(0.669)
	Schooling × Childhood violence	-0.164	1.803*	-1.151	-0.150	0.647	-0.301
		(0.121)	(0.924)	(3.280)	(0.095)	(0.726)	(0.566)
	Childhood violence	0.627	-1.164**	8.299	0.632	-0.730*	1.901
		(0.935)	(0.558)	(24.158)	(0.765)	(0.420)	(4.052)
	Mean	8.59	8.59	8.59	8.47	8.47	8.47
	Bandwidth	46	46	46	79	79	79
	Observations	600	600	600	979	979	979
Marriage decision	Schooling	0.035***	0.116	0.148	0.038***	0.162***	0.146*
		(0.006)	(0.070)	(0.144)	(0.005)	(0.057)	(0.086)
	Schooling × Childhood violence	0.038**	0.213**	-0.014	0.041***	0.265***	0.064
		(0.015)	(0.103)	(0.117)	(0.011)	(0.085)	(0.072)
	Childhood violence	-0.265**	-0.071	0.105	-0.292***	-0.103*	-0.409
		(0.123)	(0.080)	(0.836)	(0.092)	(0.060)	(0.518)
	Mean	0.55	0.55	0.55	0.53	0.53	0.53
	Bandwidth	53	53	53	87	87	87
	Observations	664	664	664	1,076	1,076	1,076

TABLE A14: RD TREATMENT EFFECTS IN RURAL CHILDHOOD REGIONS WITH DIFFERENT OPTIMAL BANDWIDTH SELECTION METHODS, CONT'D

		CCT			IK		
		(1) OLS	(2) RF	(3) IV	(4) OLS	(5) RF	(6) IV
Physical violence index	Schooling	-0.014	-0.147	0.182	-0.021***	-0.040	-0.031
		(0.012)	(0.228)	(0.519)	(0.007)	(0.089)	(0.104)
	Schooling × Childhood violence	-0.024	-0.471	-0.317	-0.044*	-0.303	-0.141
		(0.066)	(0.337)	(0.300)	(0.026)	(0.197)	(0.129)
	Childhood violence	0.591	0.561**	2.619	0.754***	0.547***	1.419
		(0.433)	(0.271)	(2.170)	(0.233)	(0.130)	(0.919)
	Mean	0.02	0.02	0.02	0.01	0.01	0.01
	Bandwidth	24	24	24	108	108	108
	Observations	325	325	325	1,311	1,311	1,311
Psychological violence index	Schooling	-0.025***	0.021	0.026	-0.025***	0.036	0.041
		(0.006)	(0.088)	(0.088)	(0.005)	(0.084)	(0.090)
	Schooling × Childhood violence	-0.003	-0.107	-0.068	0.004	-0.097	-0.071
		(0.029)	(0.136)	(0.096)	(0.027)	(0.133)	(0.090)
	Childhood violence	0.289	0.317***	0.758	0.252	0.315***	0.785
		(0.196)	(0.084)	(0.697)	(0.189)	(0.076)	(0.648)
	Mean	0.06	0.06	0.06	0.06	0.06	0.06
	Bandwidth	70	70	70	79	79	79
	Observations	877	877	877	980	980	980
Overall depression index	Schooling	-0.018**	-0.062	-0.294	-0.022***	0.017	0.039
		(0.008)	(0.096)	(0.850)	(0.005)	(0.067)	(0.088)
	Schooling × Childhood violence	-0.033	-0.552***	-0.100	0.004	-0.286**	-0.169**
		(0.030)	(0.143)	(0.395)	(0.015)	(0.115)	(0.081)
	Childhood violence	0.509***	0.449***	0.858	0.257**	0.368***	1.449**
		(0.188)	(0.087)	(2.916)	(0.111)	(0.056)	(0.575)
	Mean	0.05	0.05	0.05	0.03	0.03	0.03
	Bandwidth	44	44	44	115	115	115
	Observations	567	567	567	1,360	1,360	1,360
Somatic depression index	Schooling	-0.027***	-0.003	0.004	-0.033***	0.054	0.071
		(0.008)	(0.085)	(0.091)	(0.007)	(0.075)	(0.112)
	Schooling × Childhood violence	-0.005	-0.403***	-0.178*	0.013	-0.340**	-0.192*
		(0.031)	(0.137)	(0.106)	(0.025)	(0.143)	(0.103)
	Childhood violence	0.162	0.276***	1.348*	0.122	0.333***	1.543**
		(0.210)	(0.091)	(0.774)	(0.170)	(0.081)	(0.736)
	Mean	0.01	0.01	0.01	0.03	0.03	0.03
	Bandwidth	64	64	64	96	96	96
	Observations	817	817	817	1,167	1,167	1,167
Nonsomatic depression index	Schooling	-0.016*	-0.170	-1.379	-0.019***	0.040	0.054
		(0.009)	(0.115)	(9.691)	(0.005)	(0.077)	(0.106)
	Schooling × Childhood violence	-0.039	-0.546***	0.226	-0.011	-0.290**	-0.160**
		(0.033)	(0.190)	(3.675)	(0.017)	(0.127)	(0.081)
	Childhood violence	0.584***	0.476***	-1.758	0.378***	0.407***	1.414**
		(0.207)	(0.111)	(28.426)	(0.120)	(0.063)	(0.575)
	Mean	0.06	0.06	0.06	0.04	0.04	0.04
	Bandwidth	37	37	37	95	95	95
	Observations	496	496	496	1,156	1,156	1,156

Notes: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. The sample includes women who have children and whose childhood region is rural. The optimal bandwidth is estimated by using the Calonico et al. (2014) (CCT) algorithm in columns 1 - 3, and the Imbens and Kalyanaraman (2009) (IK) algorithm in columns 4 - 6. The RD treatment effects of the reform are reported by exposure to childhood violence, i.e., whether the respondent experienced violence from her own family members during her childhood. Columns 1 and 2 report OLS results using years of schooling as the independent variable. Columns 2 and 5, and columns 3 and 6 report reduced-form RD treatment effects and two-stage least-squares RD treatment effects (by using treatment as an instrument for years of schooling) of being born after January 1987 with a linear control function in the month-year of birth on each side of the discontinuity, respectively. The variables are described in Appendix A. All specifications control for a dummy variable for whether the respondent grew up in a rural location, a dummy variable for whether the respondent's mother tongue is not Turkish, month-of-birth fixed effects, region fixed effects, and interactions of region fixed effects with an indicator of rural regions. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.