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.

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

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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 a 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). Child physical abuse encompasses a range of actions intended to cause physical pain, including hitting, shaking, slapping, spanking, or beating the child. The overall prevalence of child physical abuse in developed countries, such as the United States is also startling, as roughly 25 percent of children experience 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. 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

[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; Newcomb and Locke 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), 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), and intimate partner violence (Reitzel-Jaffe and Wolfe 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).

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 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.[3] 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 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).[4] 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

[3]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. 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.

[4]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.

of attitudes through their teachers and peers. This engagement with alternative role models may result in a change in attitudes, including attitudes toward violence. To the extent that corporal punishment is not an acceptable form of behavior in schools,[5] 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 (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 (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.[6] Education may function as a coping resource, facilitate effective problem solving, and reduce the probability of experiencing depression (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 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

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

[6]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). 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). A recent study 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).

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, which increased mandatory school attendance from five to eight years.[7] We use the 2014 National Survey on Domestic Violence against Women in Turkey (NSDVW 2014) 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 2018). 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. 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.[8] After quantifying

[7]Our earlier paper (Erten and Keskin 2018) 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. Section 1.1 provides a more detailed discussion of the relationship of this study to our earlier work.

[8]There are at least three reasons that we focused only on mothers, not fathers, in this study. First, the prevalence of physical abuse by fathers is much lower than that of maternal child abuse. The TNSDVW 2014 survey asks women whether their partner has ever physically hurt their children. The prevalence of physical abuse by partners within our static bandwidth of 85 months around the cutoff is 19 percent. In comparison, women's likelihood of physically abusing their own children is 48 percent, and the difference

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. We also document suggestive evidence that the reform led to a differential reduction in children's aggression toward their peers and mothers.

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.[9] 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 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

between men and women is statistically significant. Second, since the average years of schooling for men prior to the reform was already higher than 8 years, the reform did not have a significant impact on men's educational attainment, as shown in Figure 2. Third, our main dataset, TNSDVW 2014, does not have information on month-of-birth for men, which does not allow us utilize the RD design.

[9] Although earlier studies in cognitive research show that retrospective reports on core autobiographical facts do not change over time (Fivush 1993), one could have a similar concern about the reliability of self-reported physical abuse in childhood. Indeed, respondents' current mood, including any anxiety or depression during their time of recall and their present parent-child relationship, can influence their recollection of physical abuse during adolescence (White et al. 2007). However, Prescott et al. (2000) show that respondents' reports of specific adverse childhood experiences such as physical punishment are less likely to be affected by current mood relative to normative perceptual memories. (The questions requiring normative evaluations of childhood treatment included true/false questions such as "My parents' use of discipline was reasonable." On the other hand, assessments requiring recollections of more concrete childhood events included those asking respondents to determine the occurrence of concrete events such as physical discipline in mild to severe levels.) To the best of our knowledge, there is no study causally investigating the role of education in influencing the reliability of retrospective self-reports on childhood abuse, but previous research suggests that education does not have a significant association with reliable reports of childhood adversity such as chronic illnesses (Krall et al. 1988).

data on child maltreatment and concluded that these data are valid as long as they are collected properly (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 paper contributes to an existing psychology literature documenting the presence of the intergenerational transmission of violence against children without making any causal inference (Merrill et al. 1996; Craig and Sprang 2007). These studies 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. In a similar vein, Milner et al. (2010) document that psychological trauma symptoms mediate the transmission of child abuse across generations.

Our paper is 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

[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.

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.

A related body of empirical work 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; Grépin and Bharadwaj 2015). 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), 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 (2018) and Gulesci and Meyersson (2016), who find a negative effect of the reform on women's religiosity, and Dincer et al. (2014) and Gunes (2016), who find a negative effect on fertility and child mortality. 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.

Finally, this study relates to our earlier work in a number of relevant respects. First, using an earlier version of this dataset—2008 TNSDVW—Erten and Keskin (2018) find that this reform led to an increase in psychological violence and financial control behavior experienced by women, without observing any change in physical violence from their intimate partners. Interestingly, we also find that the same reform led to an improvement in women's labor outcomes given an age cutoff of 21 years (as the change in the law affected those born after January 1987). We interpreted these results as suggestive evidence for instrumental theories of violence to the extent

that unanticipated increases in women's income provide incentives for male partners to use threats of violence as coercive instruments to extract resources from their partners. In contrast, in this study, we use the 2014 TNSDVW dataset and find no evidence of a significant change in women's labor market outcomes given an age cutoff of 27 years. As we show in Section 5.4, the evidence from the 2013 Turkey Demographic and Health Survey (TDHS) confirms that although the reform had a significant impact on women's employment by providing them with better skills at younger ages (17 to 21 years old), the labor market effects fade away once the women have children and begin to drop out of the labor market after age 21. The combination of these findings implies that the effects of education reforms may vary over the lifetime of women, particularly in countries where childcare services are not common or affordable. Second, although there is no evidence of domestic violence for this age group in their late 20s, the past experience of psychological violence for these women is likely to result in long-term effects through scarring. In particular, a history of psychological abuse may cause further deterioration in the mental health of women who were abused as children and affected by the reform. This potential scarring effect implies that the positive effects of the reform that we find on mental health outcomes are likely to be lower bound estimates.

This paper is organized as follows. Section 2 provides an overview of the 1997 compulsory schooling law in Turkey. Section 3 introduces the data and the identification strategy. Section 4 presents the main results, and Section 5 explores potential causal channels. Section 6 concludes.

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.

[12] Cesur and Mocan (2018) 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.

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 respondents' use of violence against their children, their history of violence from their own family members during childhood, their exposure to spousal violence, 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, also includes data on the socioeconomic indicators of households, demographics, labor market and marital histories, mental health indicators, and gender role attitudes.

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. 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. 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).

Table A1 reports summary statistics for major indicators of women who have children from the 2014 NSDVW survey. These are for women between the ages of 20 and 34 because the estimated optimal 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. 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.

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 A1 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).

Our survey also includes a subset of malaise inventory questions on several features of an individual's mental health. These questions, first developed by Rutter et al. (1970), are designed to identify depression in non-clinical settings and focus on the classic psychological symptoms of depression, such as poor concentration or thoughts of suicide, and less-known somatic symptoms such as frequent aches or digestion problems (Chevalier and Feinstein 2006). These indicators have been proven to be good predictors of depression when analyzed together. Following Duflo 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

[13]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,

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. Panel D of Table A1 reports summary statistics for these mental health outcomes.

Table A1 is also important for understanding the fundamentals of the environment where our study takes place: the average age of the respondent at first marriage was 21 years. Approximately 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. On average, 14 percent of the respondents had experienced violence from a family member during their childhood.[14] Only 19 percent of women were employed. Appendix B discusses the summary statistics for the relevant variables in greater detail.

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.

Following previous research (Oreopolous 2006), 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. 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

(xiv) thoughts about suicide, (xv) constant feelings of tiredness, and (xvi) tiring easily.

[14]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.

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 linear function of the forcing variable on each side of the cutoff point. We use local linear regressions in our RD estimations (Imbens and Lemieux 2008) and conduct optimal bandwidth selection using the Imbens and Kalyanaraman (2009) (IK) procedure. We 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. This allows for an easier comparison of estimates in the full dataset as well as the subsamples.[15] 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.

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 v_i is exposure to violence from family members during childhood. The 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.

We include the following control variables in 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.

Finally, since we evaluate the effects of education on a large number of outcomes, we adjust standard errors for multiple-hypothesis testing using the step-down procedure described in Romano and Wolf (2016) (RW) in Appendix D. Outcome variables in each table (e.g. labor market outcomes) are considered to be within the same family to carry out the multiple-hypothesis testing.[16] All our

[15] In addition, we use specifications that select an optimal bandwidth for each outcome variable examined using the Imbens and Kalyanaraman (2009) procedure. These are included in Appendix C. This optimal bandwidth approach complements the former results for which we use a static bandwidth.

[16] For each sample (overall and rural samples), by using a modified version of the rwolf command in Stata written by Clarke (2016), we adjust the p-values for three coefficients, including the main effect of schooling, its interaction with childhood violence, and the childhood violence itself. Since the RW procedure relies on

main findings are robust to this adjustment.

3.3 Preliminary Checks

We provide two standard validity checks for the RD design (Imbens and Lemieux 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 A2.

Second, we examine whether the predetermined covariates are balanced around the discontinuity. In Figure 1, each graph represents local averages of the outcome in one-month bins plotted against the forcing variable. The predetermined characteristics that we plot are regional dummy variables capturing whether the respondent was raised in a rural area, 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.[17] 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 these outcomes. The last two graphs in Figure 1 show no evidence of a significant effect of the reform on having children or on the number of children. Hence, there is no reason to expect that the reform affected the selection into the sample of women who have children, and this sample will therefore be our focus of analysis.

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 spent in school by the individuals exposed to the compulsory schooling law make them spend less time with their parents

bootstrapped p-values, for ease of comparison we report the original bootstrapped p-values together with the multiple hypothesis corrected bootstrapped p-values in Appendix D tables. We also report multiple-hypothesis-corrected p-values using Simes (1986) to accommodate the varying optimal bandwidths chosen for each outcome in Appendix C tables.

[17] In regression-based tests of whether the covariates exhibit any significant jumps at the discontinuity, we note that all coefficients are smaller than 0.05, with all p-values being greater than 20 percent. A SUR test of the coefficients' joint significance results in a p-value of 0.75.

at home and mechanically decrease their chances of being physically abused. The RD estimates reported in Table 1 and graphically illustrated in Figure A3 indicate no evidence of a significant impact on childhood violence or its intensity.[18]

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 2 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 male junior high school completion rate was already close to 90 percent.

Focusing on the sample of women, Figure 3 provides a graphical illustration of the RD design. The left-side graph plots the average junior high school completion rates in month-of-birth bins, 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. The left graph shows a clear jump at the discontinuity 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 3 examines the same relationship in which the age cutoff is the same, comparing 27- and 28-year-old women, and shows no evidence of a jump in completing junior high school. 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.

[18] We acknowledge that childhood violence may proxy for other background characteristics that may potentially generate heterogeneous treatment effects. However, due to data restrictions, we do not have any information the women's family background characteristics, including parental education and income. Nevertheless, we believe that it is important to analyze whether education may have a differential impact on this high-risk population that is prone to transmit violence across generations.

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 using a static bandwidth of 85 months around the cutoff, which is the optimal bandwidth estimated using the IK algorithm for the years of schooling for women who lived in rural regions during childhood.[19]

The first row of Table 2 presents estimates of the RD treatment effects on the years of schooling obtained by all women. Based on a local linear specification, column 1 presents an RD estimate of 0.825 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 0.825 years in the years of schooling corresponds to an 9.7 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 junior high school completion. Column 1 reports an RD estimate of 20 ppt, corresponding to 34 percent relative to the mean. In alternative specifications, the estimate remains significant. The third row shows a positive RD estimate for high school completion, implying that the reform had long-term effects. As expected, all RD estimates for whether the respondent completed primary school are insignificant.[20]

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 heterogeneous effects as a result of regional disparities in access to female education. Whereas some of these disparities result from insufficient schooling infrastructure in rural areas, some are related to the more conservative attitudes toward sending girls to school. The linear RD estimate in the first row of Panel A and column 1 shows that the reform had a positive effect of

[19]The results do not change qualitatively if we use the optimal bandwidth estimated for the years of schooling for the full sample, which is 89 months.

[20]Table A1 in Appendix C 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.

1.2 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 1.3 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.

Panel B of Table 3 focuses on the RD treatment effects on women with 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 1.2 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. 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. Similar to 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 0.8 years for all women and slightly more than one year (approximately 1.2 years) for women raised in rural regions and women with children who were raised in rural regions.[21] 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.[22]

[21] As a robustness check, Tables A2 and A3 and column 2 of Table A1 in Appendix C report the RD estimates using optimal bandwidths calculated by the IK algorithm. The findings in these tables are very similar to those in Tables 2 and 3.

[22] The compulsory schooling reform of 1997 coincided with a set of important changes in the Turkish education system that were intended to reinstate a more secular curriculum. Unfortunately, 2014 TNSDVW does not have any information on the religious school attendance or religious practices of the surveyed women. However, we use the 2013 TDHS data to test whether the reform affected women's religiosity. The results are reported in Appendix Table A4. We find no evidence of a significant effect of the reform on any of the indicators of religious practices, including praying, fasting, or wearing a headscarf, or a summary index constructed by taking the average of z-scores for these indicators. Moreover, we do not find any significant changes in attending Koran courses outside of formal education for the women who are affected by the reform. However, note that the 2013 TDHS data do not contain any information on women's experience of abuse during their childhood. Thus, we cannot test whether there is a differential effect on religiosity for

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. Columns 1 and 4 report OLS results using years of schooling as the independent variable. Columns 2 and 5 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. Finally, columns 3 and 6 present two-stage least-squares (i.e., fuzzy) RD treatment effects by using exposure to the reform as an instrument for years of schooling. 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, as well as years of schooling and frequent child abuse. 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 although some of them are noisily estimated.

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. 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 positive correlation implies a potential 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.

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 23 ppt. These are sizable effects, given the outcome mean of 51 percent. The sum of the two

women who were abused as children.

coefficients is not statistically different from zero, indicating that the reform eliminated the risk of abusing their own children for mothers who were subjected to violence during childhood. The IV estimates in column 6 are consistent with the RF estimates. The magnitudes of the estimates are slightly larger for the outcome of frequent child abuse.[23] This implies that the reform has reduced not only the probability of violence against children but also its intensity.

These findings become more striking in terms of the intergenerational transmission of violence once we investigate the outcomes for children. Although we have little data on child behavior, and the existing information is only reported for children aged 6 to 14, Table 5 provides some suggestive evidence that children of abused mothers affected by the reform are not only being physically abused less, but they are also showing signs of reduced violent behaviors themselves. These children are less likely to be physically aggressive against their mothers and peers, although we do not find any differentiated effect of having an abused but more educated mother on other child outcomes, such as wetting the bed or being shy.[24] This evidence is in line with medical research, as mentioned in the Introduction, that even milder forms of violence against children may have major implications for child aggression. Thus, the positive impact of education on maternal behavior might snowball over time and lead to a larger reduction in violence across generations.[25]

As an additional robustness check, Table A7 in 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

[23] As a robustness check, Table A5 in Appendix C reports the RD estimates using an optimal bandwidth calculated by the IK algorithm around the cutoff. The findings are quite similar to those shown in Table 4.

[24] Due to space constraints, Table 5 only presents the results for child aggression. See Table C1 in Appendix E for the full set of results.

[25] In Table A6 in Appendix C, 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 for 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 we found in child maltreatment by women who were themselves physically abused 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. These results should be interpreted with caution since the coefficients are highly noisily estimated.

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.

Finally, we check the robustness of our results by using an alternative optimal bandwidth selection method proposed by Calonico et al. (2014) (CCT). Table A8 in Appendix C shows that the coefficient estimates using the CCT bandwidth selectors are similar in magnitude to those 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, 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 divide our analysis into six subsections by focusing on the effects of the compulsory schooling reform on the following outcomes: (i) attitudes toward violence, (ii) mental health, (iii) fertility outcomes, (iv) labor market outcomes, (v) partner characteristics and marriage market outcomes, and (vi) spousal violence.

5.1 Changes in Attitudes toward Violence

If additional years of schooling change women's beliefs on violence, they might become less prone to using violence against their children.[27] Moreover, if socialization in the school environment

[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.

[27]The empirical evidence on the effects of compulsory schooling on violence-related attitudes is mixed. Some studies find that increased 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 2018).

exposes women to a different set of attitudes through interactions with teachers and peers as alternative role models, it may have a stronger effect on women who experienced childhood violence as they emulate and acquire norms that disapprove of violence toward children.

We explore this mechanism by testing whether the reform had a differential effect on the attitudes of mothers who experienced childhood violence. Table 6 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 6 show that the years of mother's schooling is negatively correlated with the probability of agreeing with these statements.

The RD estimates show 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, 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. Note that the correlation between having experienced childhood violence and having attitudes that favor violence toward children is not significant. This implies that the social learning mechanism, whereby our early-life observations shape our attitudes toward violence as a problem-solving tool, is weak in this context.[28] Overall, we conclude that the attitude channel does not seem to explain our main results.

5.2 Changes in Mental Health Outcomes

Additional years of schooling may also allow women to learn how to cope with emotional dysfunction and change their mental reactions to upsetting events. Hence, if increased schooling enables the mother to become less depressed, anxious, and aggressive, she will be less likely to abuse her children physically. This argument is consistent with recent evidence, which shows that mothers who received cognitive behavioral therapy in response to postpartum depression displayed better parenting behaviors, providing a better home environment and investing more in their children's

[28] As a robustness check, Table A9 in Appendix C reports the RD estimates using an IK optimal bandwidth for each dependent variable. 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 2018). In Panel B, the RD estimates using the optimal bandwidth show that the results in Table 6 are robust to using alternative bandwidths in the estimation.

education (Baranov et al. 2016).

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 toward her children (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 impair a woman's ability to read social cues such that she perceives certain cues as threatening even the benign ones (Crittenden and Ainsworth 1989). If additional schooling allow women to better encode social cues and become less hypervigilant, 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 of Table 7 indicate that female schooling is negatively correlated with all depression measures. The RD estimates 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 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 and the IV estimates 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. These results imply that additional years of 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.[29] Altogether, these results provide suggestive evidence that the mental health channel can potentially explain our main results.

[29] As a robustness check, Table A10 in Appendix C reports the RD treatment effects of the reform using the optimal IK bandwidth. The results are consistent with those shown in Table 7.

5.3 Changes in Fertility Outcomes

Another potential channel through which maternal education may affect child physical maltreatment is that additional years of schooling may result in a decline in fertility by increasing the time spent in school and raising the opportunity costs of having children. If additional years of female schooling lead to a decline in the number of children that women have, then it is likely to improve mothers' parenting behavior by increasing the time available per child and reducing stress through a lesser childcare burden.

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). Similarly, studies on the same education reform have found that additional years of education resulted in a decline in fertility and child mortality in Turkey (Dincer et al. 2014; Gunes 2016). In contrast, other papers showed 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). In any case, 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 check whether the reform had a significant impact on the fertility outcomes of women with experience of childhood violence. The OLS results in Table 8 indicate that more educated women have higher ages of first pregnancy and a lower number of children. 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 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.[30] Hence, the fertility channel cannot explain our main results. However, given that some

[30]Table A11 in Appendix C provides some robustness analysis using the IK bandwidth for each dependent variable. The RD estimates shown in Panel A indicate that the reform led to a significant increase in the age at first pregnancy in rural regions. 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 later as completed fertility catches up over time (Breierova and Duflo 2004).

of these coefficients are highly noisy estimates, they should be interpreted with caution.

5.4 Changes in Labor Market Outcomes

An increase in maternal education may also result in better labor market outcomes for mothers, including a higher probability of finding a job and having a personal income. 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). On the other hand, if women's working conditions are harsh, being employed may act as an additional stressor, inducing more violence against 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. Table 9 presents the results. The OLS estimates indicate a positive correlation between female years of schooling and labor market outcomes. However, the RD estimates in Table 9 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.[31] Thus, the labor market channel does not seem to explain our main results.

This may seem contrary to our earlier work in which we found that the same reform induced an improvement in the labor market outcomes for women using an earlier version of this dataset, TNSDVW 2008 (Erten and Keskin 2018). In that study, we examined the effects of education on intimate partner violence experience of women for the age cutoff of 21 years using the same RD design. This raises the question of whether the age of women in question may affect whether we observe a significant impact on labor market outcomes. In the context of Turkey, it is 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). To test

[31] Due to space constraints, Table 9 only presents the results for a selection of the outcome variables. See Table C2 in Appendix E for the full set of results. As a robustness check, Table A12 in Appendix C provides the RD treatment effects using the optimal IK bandwidth for each variable.

this possibility, we plot the coefficients on a dummy variable of being born after January 1987 in a reduced-form RD design for the outcome variables of whether the woman has worked at a particular age (conditional on reaching that age and/or being older) using the 2013 TDHS. As Figure A4 clearly shows, although the reform is likely to have a significant impact on women's employment at younger ages (17 to 21 years old), the effects are likely to disappear once the women have children and begin to drop out of the labor market after age 21. These findings suggest that the impact of education reforms may vary over women's lifetimes, particularly in countries that lack a social infrastructure for childcare. If public childcare facilities are not common and private childcare may not be affordable at lower income levels, many women may opt to be stay-at-home mothers and assume childcare responsibilities.

5.5 Changes in Partner Characteristics and Marriage Market Outcomes

If additional years of schooling allow women to have a 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. 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 schooling may render women less violent.

We explore this channel by testing whether the additional years of 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 10, the OLS estimates indicate that schooling is positively correlated with the partner's schooling and age, her marriage age and her marriage decision. The RD estimates in Table 10 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.[32] Note that we may not have sufficient power to precisely estimate these coefficients, which should be interpreted with caution.[33] Overall, we conclude that

[32]Due to space constraints, Table 10 only presents the results for a selection of the outcome variables. See Table C3 in Appendix E for the full set of results.

[33]In Table A13, we reestimate the results in Table 10 using the optimal bandwidth calculated by the IK algorithm as a robustness check. The results are similar to those reported in Table 10.

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 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 threats of violence as an instrument of control (Erten and Keskin 2018). This 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 schooling may alter this risk and produce a differential impact.

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 11, OLS estimates indicate that education is negatively correlated with experiencing physical and psychological violence from partners. The RD estimates show 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. Moreover, none of the interaction terms are significant, indicating that the reform did not have a differential impact on women with a history of childhood maltreatment.[34] Hence, we find no evidence that spousal violence can explain our main results. Although this result may seem contrary to results from our earlier work (Erten and Keskin 2018), given the findings in Section 5.4 that the reform did not lead to an improvement in women's labor market outcomes or income for the particular age group we focus on in this study, spouses do not have an incentive to use instruments of violence to extract resources from women.

As a final robustness check, Table A8 in Appendix C shows that the RD treatment effect estimates using the CCT bandwidth selectors are similar in magnitude and statistical significance to those using IK bandwidths.

[34]Table A14 in Appendix C reports the RD treatment effects using the optimal IK bandwidth calculated separately for each dependent variable. The findings are very similar to those reported in Table 11.

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 schooling for women, and the main compliers were women who grew up in rural regions. Our findings reveal that the reform 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 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 non-maltreated 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. We also document suggestive evidence that the reform led to a differential reduction in children's aggression toward other children and their mothers.

Overall, our findings underscore the importance of education in regulating emotional dysfunction and reducing child maltreatment. Given the recent scientific evidence on the crucial role of the adolescent years for brain development and emotional regulation, our results 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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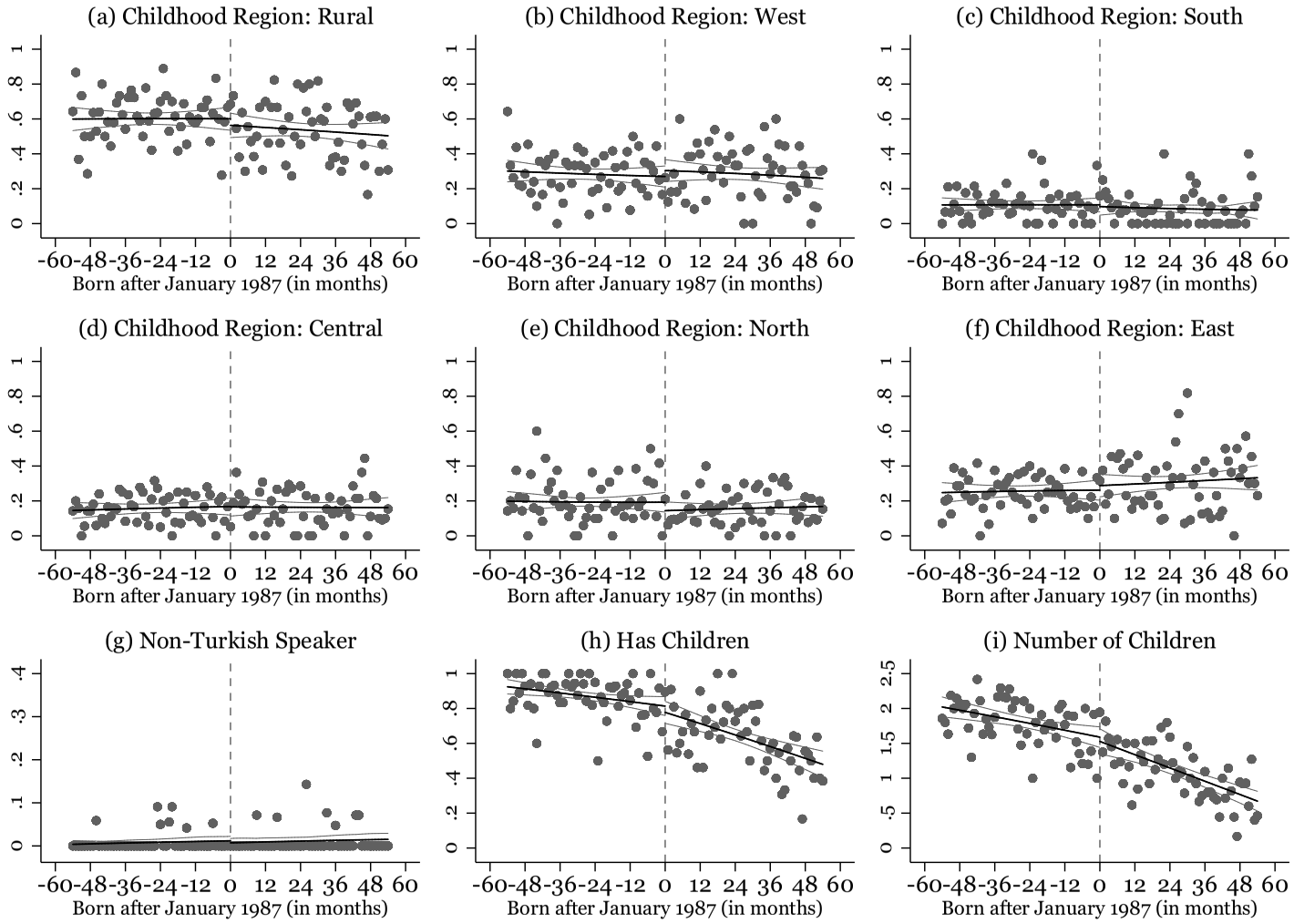
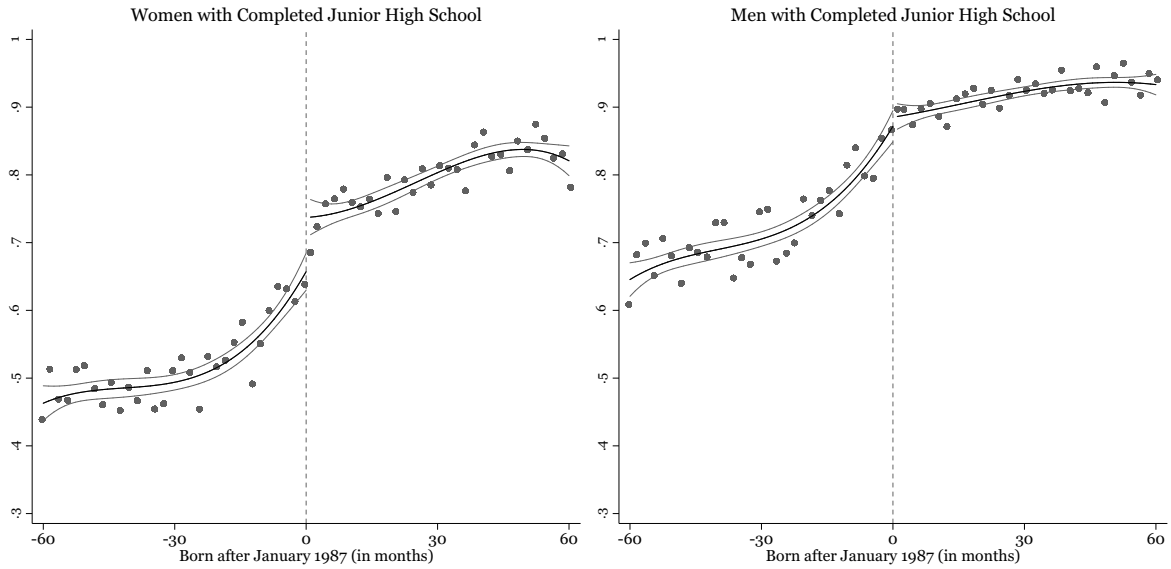
Figure 1: Balanced Covariates



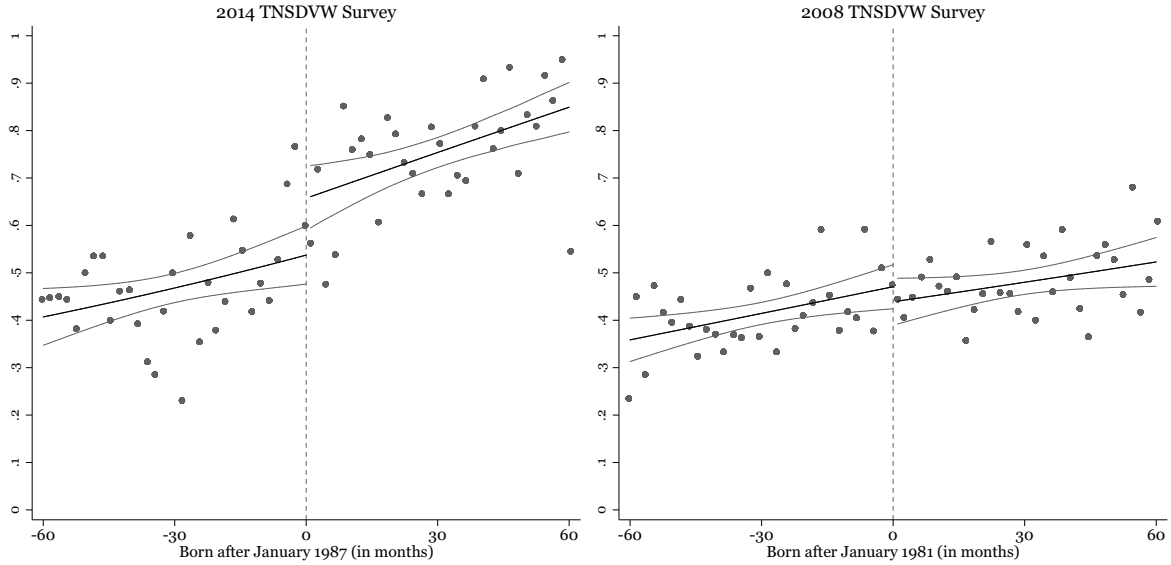
Note: Data are from the 2014 National Survey on Domestic Violence against Women in Turkey. The figures plot predetermined covariates in monthly bins against the month-year of birth of being born in January 1987. The vertical line in each graph represents the cut-off point, January 1987. Gray lines show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

Figure 2: RD Treatment Effects on Junior High School Completion



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 3: Treatment and Placebo



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 if the respondent completed junior high school in monthly bins. Gray lines show 95 percent confidence intervals around the mean level.

Table 1: Effects of the Reform on Childhood Violence

		Overall sample			Rural sample			Urban sample		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		OLS	RF	IV	OLS	RF	IV	OLS	RF	IV
Childhood violence	Schooling	-0.002	-0.023	-0.026	-0.001	-0.056	-0.047	-0.002	0.020	0.034
		(0.002)	(0.033)	(0.038)	(0.003)	(0.039)	(0.038)	(0.003)	(0.052)	(0.094)
	Observations	2,274	2,274	2,274	1,322	1,322	1,322	952	952	952
Childhood violence	Schooling	-0.003**	-0.011	-0.013	-0.004*	-0.021	-0.017	-0.003	0.006	0.010
intensity		(0.002)	(0.023)	(0.026)	(0.002)	(0.030)	(0.025)	(0.002)	(0.037)	(0.065)
	Observations	2,268	2,268	2,268	1,319	1,319	1,319	949	949	949

Notes: Data are from the 2014 NSDVW in Turkey, including all women. 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. Column 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. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

Table 2: RD Treatment Effects on Schooling Outcomes

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

Notes: Data are from the 2014 NSDVW in Turkey, including the full sample of women. 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. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

Table 3: RD Treatment Effects on Schooling Outcomes by Childhood Region

	Rural childhood region			Urban childhood region		
	(1)	(2)	(3)	(4)	(5)	(6)
Bandwidth:	$\hat{h} = 85$	$0.75\ \hat{h} = 64$	$1.5\ \hat{h} = 128$	$\hat{h} = 85$	$0.75\ \hat{h} = 64$	$1.5\ \hat{h} = 128$
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
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
Observations	1,100	847	1,504	684	521	933

Notes: Data are from the 2014 NSDVW 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 $\hat{h}$, 0.75 $\hat{h}$, and 1.5 $\hat{h}$, respectively. 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)	(2)	(3)	(4)	(5)	(6)
		OLS	RF	IV	OLS	RF	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)
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)
	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)
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)
	Observations	1,711	1,711	1,711	1,051	1,051	1,051

Notes: Data are from the 2014 NSDVW in Turkey, including 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. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

Table 5: Effects of Education on Child Behavior

		Overall sample			Rural sample		
		(1)	(2)	(3)	(4)	(5)	(6)
		OLS	RF	IV	OLS	RF	IV
Child is aggressive	Schooling	-0.005	0.035	0.080	0.001	0.123	0.156
		(0.005)	(0.066)	(0.097)	(0.007)	(0.097)	(0.149)
	Schooling × Childhood violence	-0.011	-0.202	-0.154	-0.049**	-0.499***	-0.248*
		(0.015)	(0.122)	(0.126)	(0.020)	(0.097)	(0.136)
	Childhood violence	0.161	0.111**	1.112	0.390***	0.164**	1.578*
		(0.118)	(0.052)	(0.852)	(0.146)	(0.071)	(0.845)
	Observations	1,075	1,075	1,075	684	684	684

Notes: Data are from the 2014 NSDVW in Turkey. Please see Table 4 for table notes.

Table 6: Effects of Education on Attitudes toward Violence

		Overall sample			Rural sample		
		(1)	(2)	(3)	(4)	(5)	(6)
		OLS	RF	IV	OLS	RF	IV
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)
	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)
	Observations	1,712	1,712	1,712	1,052	1,052	1,052

Notes: Data are from the 2014 NSDVW in Turkey. Please see Table 4 for table notes.

Table 7: Effects of Education on Mental Health Outcomes

		Overall sample			Rural sample		
		(1)	(2)	(3)	(4)	(5)	(6)
		OLS	RF	IV	OLS	RF	IV
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)
	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)
	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)
	Observations	1,718	1,718	1,718	1,056	1,056	1,056

Notes: Data are from the 2014 NSDVW in Turkey. Please see Table 4 for table notes.

Table 8: Effects of Education on Fertility Outcomes

		Overall sample			Rural sample		
		(1)	(2)	(3)	(4)	(5)	(6)
		OLS	RF	IV	OLS	RF	IV
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)
	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)
	Observations	2,274	2,274	2,274	1,322	1,322	1,322

Notes: Data are from the 2014 NSDVW in Turkey. Sample includes all women. Please see Table 4 for table notes.

Table 9: Effects of Education on Labor Market Outcomes

		Overall sample			Rural sample		
		(1)	(2)	(3)	(4)	(5)	(6)
		OLS	RF	IV	OLS	RF	IV
Employed	Schooling	0.022***	0.025	0.127	0.015***	0.021	0.022
		(0.004)	(0.034)	(0.234)	(0.004)	(0.039)	(0.038)
	Schooling × Childhood violence	-0.002	-0.033	-0.127	-0.006	-0.041	-0.030
		(0.009)	(0.067)	(0.219)	(0.013)	(0.081)	(0.049)
	Childhood violence	0.055	0.048	1.007	0.093	0.073	0.270
		(0.067)	(0.043)	(1.684)	(0.097)	(0.056)	(0.357)
	Observations	1,718	1,718	1,718	1,056	1,056	1,056
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)
	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)
	Observations	1,718	1,718	1,718	1,056	1,056	1,056

Notes: Data are from the 2014 NSDVW in Turkey. Please see Table 4 for table notes.

Table 10: Effect of Education on Partner Characteristics and Marriage Market Outcomes

		Overall sample			Rural sample		
		(1)	(2)	(3)	(4)	(5)	(6)
		OLS	RF	IV	OLS	RF	IV
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)
	Observations	1,703	1,703	1,703	1,043	1,043	1,043
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)
	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)
	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)
	Observations	1,718	1,718	1,718	1,056	1,056	1,056

Notes: Data are from the 2014 NSDVW in Turkey. Please see Table 4 for table notes.

Table 11: Effects of Education on Spousal Violence

		Overall sample			Rural sample		
		(1)	(2)	(3)	(4)	(5)	(6)
		OLS	RF	IV	OLS	RF	IV
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)
	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)
	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)
	Observations	1,711	1,711	1,711	1,051	1,051	1,051

Notes: Data are from the 2014 NSDVW in Turkey. Please see Table 4 for table notes.