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.

I. 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, eight of ten children aged 2 to 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; roughly 25% 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 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 (Straus et al., 1980; Eamon, 2001). 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 of against children for discipline (Widom, 1989). However, if socialization within the family is one environment in which individuals acquire social norms of appropriate behavior, another sphere

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[1] Several studies in the psychology literature document a strong, positive correlation between exposure to childhood violence and child physical abuse later in life (Kaufman & Zigler, 1987; Newcomb & 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 & Rogosch, 2001), developmental delays (Veltman & Browne, 2001), and poor academic performance (Kendall-Tackett & Eckenrode, 1996). Child physical abuse is also associated with an increased likelihood of interpersonal violence (Benda & Corwyn, 2002; Reitzel-Jaffe & Wolfe, 2001), as well as a higher probability of engaging in criminal activities, such as burglary or armed robbery (Currie & 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).

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of early socialization is the school environment, where individuals may be exposed to a different set 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,[3] 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 fail to encode benign social cues, thus leading them to become hypervigilant toward actions that they misread as threatening (Crittenden & 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, 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.[4] Education may function as a coping resource, facilitate effective problem solving, and reduce the probability of experiencing depression (Ross & Mirowsky, 1989). A reduction in maternal depression may then 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 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 (BEP) in Turkey, which increased mandatory school attendance from five to eight years.[5] 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 12, 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 & 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.[6] After quantifying the impacts of education on the prevalence of child abuse in this high-risk group, we explore the potential mechanisms underlying this effect. We find that women in the treated cohorts and with a history of childhood abuse were more likely to experience an improvement in their mental health outcomes. In contrast, we find no evidence of a differential impact of the reform on attitudes toward violence, labor market outcomes, partner characteristics, spousal violence, or fertility decisions for women who experienced childhood maltreatment. 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.[7] While we cannot rule out this possibility, we find no evidence of a significant impact of

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

[4] Recent evidence from the neuroscience literature indicates that effects of education on socioemotional 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).

[5] Our earlier paper (Erten & 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. Section IA provides a more detailed discussion of the relationship of this study to our earlier work.

[6] We focused only on mothers, not fathers, in this study. 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%. In comparison, women's likelihood of physically abusing their own children is 48%. The difference between men and women is statistically significant.

[7] Although earlier studies in cognitive research show that retrospective reports on core autobiographical facts do not change over time (Fivush,

increased education on mothers' attitudes toward violence, including attitudes toward violence against children. A total of 29% 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: 38% of them agree that men are justified in beating their partners in certain situations. More important, almost half of the women in our sample (48%) report that they have at least once physically abused their children, and an astonishing 41% report that they have used physical violence multiple times or frequently.

Moreover, previous studies that have relied on similar national surveys to examine the relationship of child maltreatment to other outcomes investigated the validity of self-reported data on child maltreatment and concluded that these data are valid as long as they are collected properly (Currie & Tekin, 2012). Finally, as Currie and Tekin (2012) explained in detail, 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 & 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.

A. *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, Hervig, & Milner, 1996; Craig & 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, Kawachi and Sparrow (1999) document a negative association between educational attainment and long-term stress. Similarly, 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 andi Egana Del-Sol (2019) is closest to our work in spirit. Their findings indicate that exposure to an after-school 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 the 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 & Li, 2009; Grépin & 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 & Krueger, 1991; Oreopolous, 2006), health (Lleras-Muney, 2005), fertility behavior (Black, Devereux, & Salvanes, 2008; McCrary & 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 reform on other outcomes in Turkey. These studies include Cesur and Mocan (2018) and Gulesci and Meyersson (2015), who find a negative effect of the reform on women's religiosity, and Dincer, Kaushal, and Grossman (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 data set, 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 data set 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 VE, the evidence from the 2013 Turkey

1993), one could have a similar concern about the reliability of self-reported physical abuse in childhood. Indeed, respondents' current mental state and current parent-child relationship can influence their recollection of physical abuse during adolescence (White, Widom, & Chen, 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.

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 child care 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 II provides an overview of the 1997 compulsory schooling law. Section III introduces the data and the identification strategy. Section IV presents the main results. Section V explores potential causal channels. Section VI concludes.

II. 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. In 1997, the parliament of Turkey passed Law No. 4306, which extended compulsory schooling to eight years. This law, referred to as the Basic Education Program (BEP), applied to all students who had not already completed primary school 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 BEP 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 or she turns 6 years old. The 1997 BEP, which made eight years of primary education compulsory, was effectively implemented in the 1997–1998 school year. A student who had completed fifth grade in 1997 could drop out. However, a student who had completed fourth grade in 1997 was required to continue school through eighth grade. The combination of the school starting age law and the 1997 BEP 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.

The BEP required substantial investments in schooling infrastructure, which led to an increase in the share of MONE in the public investment budget from 15% in 1997 to 37% in 1998. Referred to as a "big bang" approach to education reform, the BEP necessitated the restoration of old schools and the construction of new ones, the hiring of 103,000 additional teachers (a 41% increase), and the construction of 80,000 new classrooms (a 36% increase) between 1996 and 2003. A standardized bus system was implemented in 2000 to transport students from rural areas to nearby schools, and a program was established to distribute free books and meals to low-income students.

The BEP was successful in substantially increasing enrollment in primary education. From 1997 to 2000, the net schooling ratio rose from 84.7% to 93.5%. 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% to 97%. Due to the massive investments in schooling infrastructure, the student-to-classroom ratio remained fairly constant, implying that the quality of education did not deteriorate over this period.

III. Data and Empirical Methodology

A. 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. The survey, 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. One woman per household was randomly selected. 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 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%, the high school completion rate was 31%, and 89% of the women had completed primary school. Women raised in rural areas had lower educational attainment.

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: (a) an indicator variable of whether the respondent has ever physically abused her children and (b) an indicator variable of whether the respondent has frequently abused her children, including a few times and many times. Panel B of Table A1 show that 48% of women in Turkey have at least once used physical violence against their children. The propensity to frequently abuse children is also high: approximately 41% of women have physically abused their children often.

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, Tizard, and Whitmore (1970), are designed to identify depression in nonclinical 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. These indicators have been proven to be good predictors of depression when analyzed together. Following Duflo, Glennerster, and Kremer (2007), we construct three summary indices: (a) an overall depression index, which is an average of the z-scores of twenty mental health indicators; (b) a somatic depression index, which is an average of four indicators that are related to the body and are therefore more objective measures of depression; and (c) a nonsomatic depression index, which is an average of the remaining sixteen indicators that represent more subjective assessments of depression. This aggregation approach provides greater statistical power to identify effects in the same direction for a group of indicators that captures similar symptoms of psychological well-being. Higher index values indicate higher levels of depression. 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. Thirty-eight percent of women agree with the statement that men can beat their partners in certain situations, and 29% agree with the statement that it may be necessary to beat children for discipline. On average, 14% of the respondents had experienced violence from a family member during their childhood.[8] Only 19 percent of women were employed. Appendix B discusses the summary statistics for the relevant variables in greater detail.

B. *Identification*

The 1997 compulsory schooling law, together with the law on school starting age, required the completion of eight years of schooling by individuals born after January 1987, whereas those born earlier could drop out after five years, as explained in section II. 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 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:

$$y_i = \alpha + \beta t_i + f(x_i) + \epsilon_i,$$
$$\forall x_i \in (c - h, c + h), \tag{1}$$

where y_i is the dependent variable, t_i is the treatment status, x_i is the forcing variable, and h is the bandwidth around the cutoff point c. We allow the slope to vary on each side of the cutoff. The control function, $f(x_i)$, is a continuous linear function of the forcing variable on each side of the cutoff point. We use local linear regressions in our RD estimations (Imbens & 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 data set as well as the subsamples.[9] 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

$$y_i = \alpha + \beta t_i + \gamma t_i \times v_i + \delta v_i + f(x_i) + u_i,$$
$$\forall x_i \in (c - h, c + h), \tag{2}$$

[8] 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 that an individual faced as a child.

[9] As a robustness check, 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.

where v_i is exposure to violence from family members during childhood. δ captures whether exposure to childhood violence affects the individual's adult behavior toward her own children or other individual outcomes of interest, and γ shows whether the education reform had a differential impact on individuals exposed to childhood violence.

We include the following control variables: a dummy variable for whether the respondent grew up in a rural area, 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.[10] All our main findings are robust to this adjustment.

C. *Preliminary Checks*

We provide two standard validity checks for the RD design (Imbens & 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 graphs do not indicate any significant jumps at the cutoff point.[11] Overall, we conclude that the predetermined covariates appear to be balanced around the threshold.

Because all of the violence against children-related questions is 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 at home and mechanically decrease their chances of being physically abused. The RD estimates reported in table A2 and graphically illustrated in figure A3 indicate no evidence of a significant impact on childhood violence or its intensity.[12]

IV. Effects of the Compulsory Schooling Law

A. *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. Panel A in 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. 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 for men. This result implies that the reform had a smaller effect on men, possibly because the male junior high school completion rate was already close to 90%.

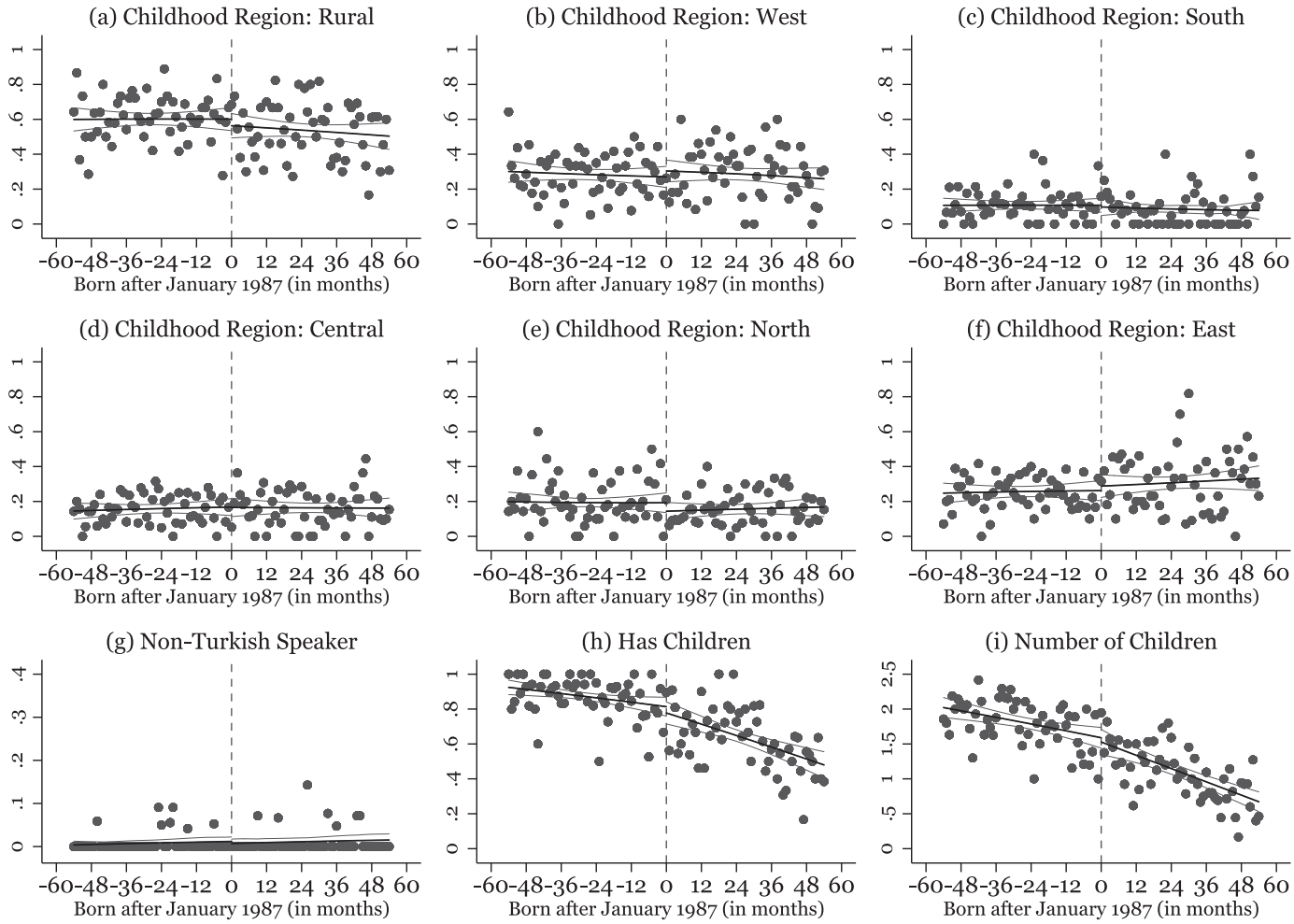
Focusing only on women, panel B of figure 2 provides a graphical illustration of the RD design. The left-side graph plots the average junior high school completion rates in month-of-birth bins, with a cutoff of January 1987 using the 2014 NSDVW survey. The left graph shows a clear jump at the discontinuity in the probability of completing junior high school. We use data from the 2008 NSDVW to conduct a placebo test. The right-side graph of panel B 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.

[10] 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: the main effect of schooling, its interaction with childhood violence, and the childhood violence itself. Since the RW procedure relies on 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.

[11] In regression-based tests, we note that all coefficients are smaller than 0.05, with all *p*-values being greater than 20%. A SUR test of the coefficients' joint significance results in a *p*-value of 0.75.

[12] 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 on the women's background characteristics. 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.

FIGURE 1.—BALANCED COVARIATES



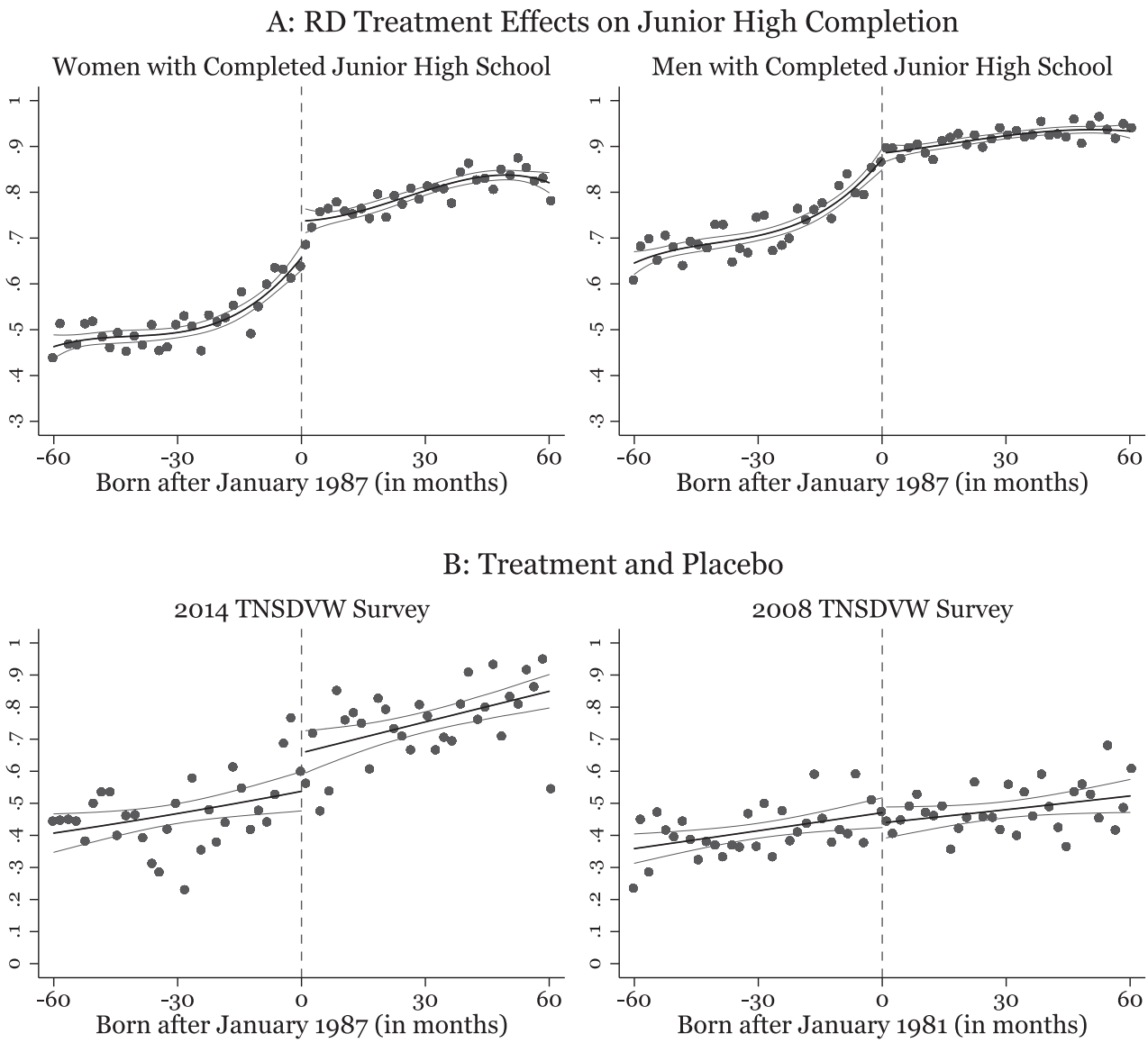
The panels plot predetermined covariates in monthly bins against the month-year of birth of being born in January 1987. Gray lines show 95% confidence intervals. Variable definitions are in appendix A.
Source: Data are from the 2014 NSDVW.

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 A3 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, the optimal bandwidth estimated using the IK algorithm for the years of schooling for women who lived in rural regions during childhood. The first row of table A3 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 a significant RD estimate of 0.825 years for the treatment effect on years of schooling, corresponding to a 9.7% increase relative to the mean. For robustness, we 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 A3 show that the reform had a significant positive impact on junior high school completion, as well as high school completion. As expected, all RD estimates for whether the respondent completed primary school are insignificant.[13]

In table 1, 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 education. We find that the reform had a positive effect of 1.2 years on the schooling of women raised in rural areas, corresponding to a 15% increase relative to the mean. The RD estimates in the alternative specifications 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.

[13]Table A4 reports the RD estimates using a quadratic control function with an optimal bandwidth selection method in column 1, which are in line with those reported in table A3.

FIGURE 2.—JUNIOR HIGH SCHOOL COMPLETION



The panels plot junior high school completion rates in monthly bins for women on the left and men on the right. The figures plot a dummy variable equal to 1 if the respondent completed junior high school in monthly bins. Gray lines show 95% confidence intervals around the mean level.
Sources: In panel A, data are from the 2014 Household Labor Force Survey. In panel B, data are from the 2014 and 2008 National Surveys on Domestic Violence against Women in Turkey, respectively.

TABLE 1.—RD TREATMENT EFFECTS ON SCHOOLING OUTCOMES BY CHILDHOOD REGION

	(1)	(2)	(3)	(4)	(5)	(6)
	Rural Childhood Region			Urban Childhood Region		
Bandwidth:	$\hat{h} = 85$	$0.75\,\hat{h} = 64$	$1.5\,\hat{h} = 128$	$\hat{h} = 85$	$0.75\,\hat{h} = 64$	$1.5\,\hat{h} = 128$
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
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

Columns 1 to 3, and 4 to 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% levels, respectively.
Source: Data are from the 2014 NSDVW in Turkey.

TABLE 2.—EFFECTS OF EDUCATION ON VIOLENCE AGAINST CHILDREN

	(1)	(2)	(3)	(4)	(5)	(6)
		Overall Sample			Rural Sample	
	OLS	RF	IV	OLS	RF	IV
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
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

All columns use a static bandwidth of 85 months, the optimal bandwidth estimated for the years of schooling in rural regions of childhood. Standard errors are clustered at the month-year cohort level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Source: Data are from the 2014 NSDVW in Turkey, including women who have children.

Panel B of table 1 focuses on the RD treatment effects on women with children, 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% 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 to 6 of panel B in table 1 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 1 year (approximately 1.2 years) for women raised in rural regions and women with children raised in rural regions.[14] Since we find no evidence of a significant impact of the reform on women raised in urban areas, in the following sections, we report the results for the overall and rural samples.[15]

B. *Education and Violence against Children*

In this section, we test whether the reform had a significant impact on violence against children. Table 2 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 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

[14] As a robustness check, tables A5 and A6 and column 2 of table A4 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 A3 and 1.

[15] The compulsory schooling reform of 1997 coincided with a set of important changes in the Turkish education system intended to reinstate a more secular curriculum. Using the 2013 TDHS data, we test whether the reform affected women's religiosity. The results reported in table A7 show 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. However, since the 2013 TDHS data do not contain any information on women's experience of abuse during their childhood, we cannot test whether there is a differential effect on religiosity for women who were abused as children.

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 2, 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. 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 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 2 indicates that women raised in rural areas are 34 percentage points more likely to abuse their children if they experienced physical maltreatment in childhood themselves. Being exposed to the reform reduces this probability by 23 percentage points. These are sizable effects, given the outcome mean of 51%. The sum of the two coefficients is not statistically different from 0, 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.[16] 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 few data on child behavior and the existing information is reported only for children aged 6 to 14, table A9 provides some suggestive evidence that children of abused mothers affected by the reform are not only being physically abused less; they are also showing a reduction in 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 on other child outcomes, such as wetting the bed or being shy. This evidence is in line with medical research 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.

As an additional robustness check, table A10 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—violence from family members or others, including teachers and strangers. The RD estimates in columns 5 and 6 indicate that the reform had a significant negative impact on child abuse or frequent child abuse for women who experienced overall childhood violence. In panel B of table A10, 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, Cattaneo, and Titiunik (2014; CCT). Table A11 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.[17]

V. Examining Causal Channels

In this section, we 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. We divide our analysis into six subsections by focusing on the effects of the compulsory schooling reform on the following outcomes: (a) attitudes toward violence, (b) mental health, (c) fertility outcomes, (d) labor market outcomes, (e) partner characteristics and marriage market outcomes, and (f) spousal violence.

A. Changes in Attitudes toward Violence

If education changes women's beliefs on violence, they might become less prone to using violence against their children. Moreover, if socialization in the school environment exposes women to a different set of attitudes through interactions with teachers and peers as alternative role models, these experiences may have a stronger effect on women who experienced childhood violence as they acquire norms that disapprove of violence toward children.

[16] As a robustness check, table A8 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 2.

[17] 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.

TABLE 3.—EFFECTS OF EDUCATION ON ATTITUDES TOWARD VIOLENCE

	(1)	(2)	(3)	(4)	(5)	(6)
	Overall Sample			Rural Sample		
	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

Data are from the 2014 NSDVW in Turkey. See the note to table 2.

We explore this mechanism by testing whether the reform had a differential effect on the attitudes of mothers who experienced childhood violence. Table 3 reports our findings, focusing on the probability that the respondent agrees with the following: (a) men can beat their partners in certain situations, and (b) it may be necessary to beat children for discipline. The correlations reported in columns 1 and 4 of table 3 show that the years of mother's schooling are 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. 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 0 and insignificant.[18] Overall, we conclude that the attitude channel does not seem to explain our main results.

B. 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 that mothers who received cognitive behavioral therapy in response to postpartum depression displayed better parenting behaviors (Baranov et al., forthcoming).

[18] As a robustness check, table A12 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 & Keskin, 2018). In panel B, the RD estimates using the optimal bandwidth show that the results in table A12 are robust to using alternative bandwidths.

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 render her more impulsive toward her children (Pomeroy, 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 even the benign cues as threatening (Crittenden & Ainsworth, 1989). If additional schooling allows women to better encode social cues and become less hypervigilant, this 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 4 indicate that 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

TABLE 4.—EFFECTS OF EDUCATION ON MENTAL HEALTH OUTCOMES

	(1)	(2)	(3)	(4)	(5)	(6)
	Overall Sample			Rural Sample		
	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

Data are from the 2014 NSDVW in Turkey. See the note to table 2.

experiencing mental illness.[19] Altogether, these results provide suggestive evidence that the mental health channel can potentially explain our main results.

C. *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, it is likely to improve mothers' parenting behavior by increasing the time available per child and reducing stress through a lesser child care burden.

An extensive literature has examined the effects of education on fertility outcomes. Some studies found evidence that increased female schooling increases the age of first pregnancy (Black et al., 2008; Silles, 2011). In contrast, other papers showed no significant impact of schooling on the probability of having children or the age of first pregnancy (McCrary & 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 & 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 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 5 indicate that more educated women have higher ages of first pregnancy and fewer 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.[20] Hence, the fertility channel cannot explain our main results. However, given that some of these coefficients are highly noisy estimates, they should be interpreted with caution.

D. *Changes in Labor Market Outcomes*

An increase in 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,

[19] As a robustness check, table A13 reports the RD treatment effects of the reform using the optimal IK bandwidth. The results are consistent with table 4.

[20] Table A14 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 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 & Duflo, 2004).

TABLE 5.—EFFECTS OF EDUCATION ON FERTILITY OUTCOMES

	(1)	(2)	(3)	(4)	(5)	(6)
	Overall Sample			Rural Sample		
	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

Data are from the 2014 NSDVW in Turkey. Sample includes all women. See the note to table 2.

mothers' increased economic empowerment may allow them to more effectively respond to children's needs, resulting in a lower propensity to resort to violence for discipline (Paxson & Waldfogel, 2002). However, if women's working conditions are harsh, being employed may act as an additional stressor and induce more violence. Moreover, if women with a history of childhood violence have lower cognitive ability or noncognitive traits that disqualify them from attaining certain skills, 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 schooling had a significant impact on the labor market outcomes of women exposed to childhood violence. The OLS estimates presented in table 6 indicate a positive correlation between schooling and labor market outcomes. However, our RD estimates 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 differential impact on the labor market outcomes of women who experienced childhood abuse.[21] 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 data set, TNSDVW 2008 (Erten & 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 & Kırdar, 2010). To test 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 or being older, or both) 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 child care. If public child care facilities are not common and private child care may not be affordable at lower income levels, many women may opt to be stay-at-home mothers and assume child care responsibilities.

E. Changes in Partner Characteristics and Marriage Market Outcomes

If increased education allows women to have a less violence-prone partner, this 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.

[21] Due to space constraints, table 6 presents only the results for a selection of the outcome variables. See table A28 in appendix E for the full set of results. As a robustness check, table A15 in appendix C provides the RD treatment effects using the optimal IK bandwidth for each variable.

TABLE 6.—EFFECTS OF EDUCATION ON LABOR MARKET OUTCOMES

	(1)	(2) Overall Sample	(3)	(4)	(5) Rural Sample	(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

Data are from the 2014 NSDVW in Turkey. See the note to table 2.

TABLE 7.—EFFECT OF EDUCATION ON PARTNER CHARACTERISTICS AND MARRIAGE MARKET OUTCOMES

	(1)	(2) Overall Sample	(3)	(4)	(5) Rural Sample	(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

Data are from the 2014 NSDVW in Turkey. See the not to Table 2.

We explore this channel by testing whether the additional years of schooling had a differential effect on the partner characteristics and marriage market outcomes of women exposed to childhood violence. In table 7, 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 7 indicate no evidence of a significant impact on the interaction terms of exposure

TABLE 8.—EFFECTS OF EDUCATION ON SPOUSAL VIOLENCE

	(1)	(2)	(3)	(4)	(5)	(6)
	Overall Sample			Rural Sample		
	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

Data are from the 2014 NSDVW in Turkey. See the note to Table 2.

to the reform and childhood violence, with the exception of only two of twelve RD estimates.[22] Note that we may not have sufficient power to precisely estimate these coefficients, which should be interpreted with caution.[23] Overall, we conclude that the marriage market channel does not appear to explain our main results.

F. Changes in Spousal Violence

An improvement in education may also affect the probability that a woman experiences spousal violence. Schooling that economically empowers women and improves their bargaining position within the household may lead to a decline in the probability of facing spousal violence and result in lower levels of stress and child abuse. However, if such economic empowerment creates incentives for partners to extract rents from women, an increase in violence as an instrument of control (Erten & Keskin, 2018) may result. 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 8, 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.[24] 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 & Keskin, 2018), given the findings in section VD 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 A11 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.

[22] Due to space constraints, table 7 presents the results only for a selection of the outcome variables. See table A29 in appendix E for the full set of results.

[23] In table A16, we reestimate the results in table 7 using the optimal bandwidth calculated by the IK algorithm. The results are similar to those reported in table 7.

[24] Table A17 reports the RD treatment effects using the optimal IK bandwidth calculated separately for each dependent variable. The findings are similar to table 8.

VI. Conclusion

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 nonmaltreated mothers. However, women in the treated cohorts and with a history of childhood abuse are more likely to experience an improvement in their mental health outcomes. 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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